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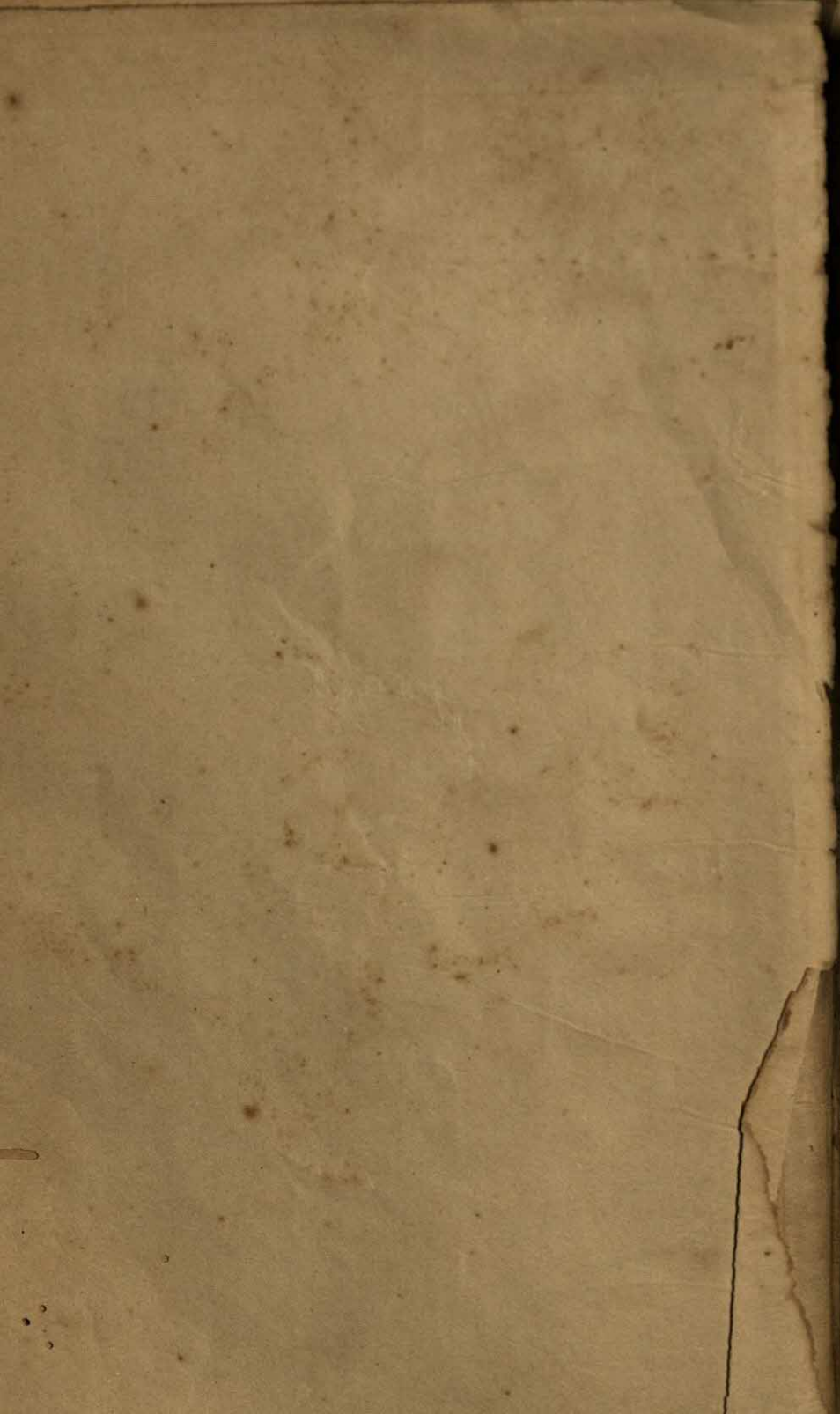
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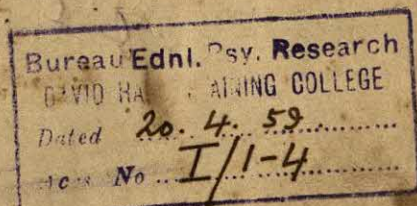


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No. 1

## AUDITORY THRESHOLDS IN THE PIGEON

By GEORGE A. HEISE, Oberlin College

This investigation is concerned with the responses of pigeons to auditory stimulation at near-threshold levels of intensity. Its results have significance both for the electrophysiology of the auditory system and for the systematic study of behavior. The recent literature on hearing in animals is listed in the Harvard bibliography.<sup>1</sup> Studies up to 1932 have been summarized by Horton,<sup>2</sup> and by Herington and Gundlach.<sup>3</sup> Munn has provided a review of audition in the rat,<sup>4</sup> and investigations of hearing in birds have been described by Wever and Bray,<sup>5</sup> and by Schwartzkopff,<sup>6</sup> whose survey is complete and up to date.

In studying absolute auditory thresholds in animals, previous investigators have utilized four types of response.

(1) 'Unconditioned' responses. Wada<sup>7</sup> and Jellinek<sup>8</sup> measured the effectiveness of various tonal frequencies in waking pigeons from a 'hypnotic' state. The use of the

\* Accepted for publication February 18, 1952. This research was conducted in the Psycho-Acoustic Laboratory of Harvard University under contract with the U. S. Navy, Office of Naval Research (Contract N5ori-76, Project NR142-201, Report PNR-119). The author is indebted to C. B. Ferster, G. A. Miller, and W. A. Rosenblith for helpful suggestions and criticisms.

<sup>1</sup> G. A. Miller, et al. *A Bibliography in Audition*. Cambridge: Harvard University Press, 1950, 2 vols.

<sup>2</sup> C. P. Horton, A quantitative study of hearing in the guinea pig (*Cavia Cobaya*), *J. Comp. Psychol.*, 15, 1933, 59-73.

<sup>3</sup> G. B. Herington and R. H. Gundlach, How well can guinea pigs and cats hear tones? *J. Comp. Psychol.*, 16, 1933, 287-301.

<sup>4</sup> N. L. Munn, *Handbook of Psychological Research on the Rat*, 1950, 158-168.

<sup>5</sup> E. G. Wever and C. W. Bray, Hearing in the pigeon as studied by the electrical responses of the inner ear, *J. Comp. Psychol.*, 22, 1936, 353-363.

<sup>6</sup> J. Schwartzkopff, Über Sitz und Leistung von Gehör und Vibrationsinn bei Vögeln, *Zsch. vergl. Physiol.*, 31, 1949, 527-608.

<sup>7</sup> Y. Wada, Beiträge zur vergleichende Physiologie des Gehörorgans, *Arch. f. d. ges. Physiol.*, 202, 1924, 46-69.

<sup>8</sup> A. Jellinek, Versuche über das Gehör der Vogel. II. Gehörprüfungen an Tauben nach Exstirpation des Mittelohres, *Arch. f. d. ges. Physiol.*, 211, 1926, 73-81.



Tullio reflex in pigeons to study the physiology of the ear has been described most recently by Van Eunen, Huizing, and Huizinga.<sup>9</sup> If a hole is made in the bony semi-circular canal, the pigeon's head moves in the plane of the canal affected when a tone is sounded. These 'reflex' responses are not suitable for precise measurement of normal hearing capacity.

(2) *Electrophysiological responses.* The electrical response to sinusoidal stimulation, picked up by an electrode placed on the round or oval window, has been used by Wever and Bray to study the hearing process in animals.<sup>10</sup> As will be shown later in this paper, however, their electrophysiological data for the pigeon are not directly related to auditory sensitivity as measured behaviorally.

(3) *Classical conditioned responses.* Several investigators have obtained reliable thresholds by the use of classical conditioning techniques. Horton, for example, subjected guinea pigs to pairings of tone and shock, and measured the change in breathing produced by the tone alone.<sup>11</sup> Cowles and Pennington conditioned the rat's squeak by pairing a tone with shock to the tail.<sup>12</sup> In recent work with the rat by Jamison, the decrement in heart-rate caused by ammonia was conditioned to a tone.<sup>13</sup>

(4) *Instrumental responses.* Most investigators of auditory sensitivity in animals have employed an instrumental conditioning technique. A food reward contingent upon the proper response in the presence of tone has been used to measure absolute thresholds in dogs and cats by Dworkin and his collaborators,<sup>14</sup> in monkeys by Wendt,<sup>15</sup> in chimpanzees by Elder,<sup>16</sup> and in bullfinches by Schwartzkopff.<sup>17</sup> In the procedure used by Culler and his colleagues,<sup>18</sup> the animal learns to avoid shock by emitting a distinctive response when the tone is sounded. One such response, also utilized by Lipman and Grassi in the determination of absolute thresholds in the dog, is conditioned paw-withdrawal.<sup>19</sup> Harris required his monkeys to move a rotating drum or stabilimeter when the tone sounded in order to avoid shock.<sup>20</sup> Gould

<sup>9</sup> A. J. H. Van Eunen, H. C. Huizing and E. Huizinga, Die Tulliosche Reaktion in Zusammenhang mit der Funktion des Mittelohres, *Acta oto-laryng.*, 31, 1943, 265-339.

<sup>10</sup> Wever and Bray, *op. cit.*, 355 ff.

<sup>11</sup> Horton, *op. cit.*, 64-66.

<sup>12</sup> J. T. Cowles and L. A. Pennington, An improved conditioning technique for determining auditory acuity of the rat, *J. Psychol.*, 15, 1943, 41-47.

<sup>13</sup> J. H. Jamison, Measurement of auditory intensity thresholds in the rat by conditioning of an autonomic response, *J. Comp. Physiol. Psychol.*, 44, 1951, 118-125.

<sup>14</sup> S. Dworkin, J. Katzman, G. A. Hutchison, and J. R. McCabe, Hearing acuity of animals as measured by conditioning methods, *J. Exper. Psychol.*, 26, 1940, 281-298.

<sup>15</sup> G. R. Wendt, Auditory acuity of monkeys, *Comp. Psychol. Monog.*, 10, 1934 (No. 49), 1-51.

<sup>16</sup> J. H. Elder, Auditory acuity of the chimpanzee, *J. Comp. Psychol.*, 17, 1934, 157-183.

<sup>17</sup> Schwartzkopff, *op. cit.*, 527-608.

<sup>18</sup> Elmer Culler, Glen Finch and Edward Girden, Apparatus for motor conditioning in cats, *Science*, 79, 1934, 525-526; Culler, Finch, Girden and Wilfred Brogden, Measurements of acuity by the conditioned-response technique, *J. Gen. Psychol.*, 12, 1935, 223-227.

<sup>19</sup> E. A. Lipman and J. R. Grassi, Comparative auditory sensitivity of man and dog, this JOURNAL, 55, 1942, 84-89.

<sup>20</sup> J. D. Harris, The auditory acuity of pre-adolescent monkeys, *J. Comp. Psychol.*, 35, 1943, 255-265.



and Morgan,<sup>21</sup> and Hunter and Pennington,<sup>22</sup> taught rats to escape from shock by moving from one part of a cage to another when the tone sounded. Trainer employed a similar response with several varieties of birds.<sup>23</sup>

Using these techniques, investigators of animal hearing have had varying degrees of success in dealing with two major problems: (1) the accurate specification and control of the physical characteristics of the effective stimulus for the animal; and (2) the selection of a response that signifies unambiguously that the animal hears the auditory stimulus. A third problem, suggested by the wide variety of techniques noted above, has received somewhat less recognition: to what extent are conclusions about auditory sensitivity dependent upon the particular methods used?

The present study was designed to suggest some answers to these problems, as well as to obtain data on the sensitivity of pigeons for later use in physiological and behavioral studies. The method used was an adaptation of the operant conditioning technique developed by Skinner.<sup>24</sup> Pigeons were trained to peck at a small key in an experimental box. A distinctive pecking response—a 'burst' of ten pecks—was reinforced with grain if it occurred in the presence of an auditory stimulus. Upon completion of the extensive training required to establish a satisfactory auditory discrimination, absolute thresholds were determined by a modified method of limits.

#### METHOD AND PROCEDURE

(1) *Apparatus.* The experimental box, an insulated portable icebox with a hinged lid, was placed in a small experimental room, the walls of which were lined with sound-absorbing material. A transverse masonite panel divided the experimental box into two compartments. The pigeon was placed in one compartment, a 12 x 12 x 12-in. enclosure lined on three sides (except for a small window) and on the top with Celotex. On the floor of this compartment was an inch-deep layer of sawdust covered by a grid made of hardware cloth.

A 2-in. loudspeaker was mounted at the top-center of the compartment's fourth side (the masonite panel) at a slight angle that sound-waves were aimed predominantly at the intersection of the floor and the opposite side of the compartment. The key, a small button fastened to the arm of a microswitch, was mounted on the panel immediately under the loudspeaker. Below the key was an opening, guarded by a shield, allowing access to the food-magazine. Reinforcements were delivered by sliding this shield aside for a period of about 5 sec., which permitted the pigeon a few pecks at the grain in the magazine. The presentation of the magazine was accompanied by a sharp click from a relay and a change of lighting in the box.

<sup>21</sup> James Gould and C. T. Morgan, Auditory sensitivity in the rat, *J. Comp. Psychol.*, 34, 1942, 321-329.

<sup>22</sup> W. A. Hunter and L. A. Pennington, The construction and use of a new apparatus in the training of the rat in auditory discrimination problems, *J. Genet. Psychol.*, 57, 1940, 451-460.

<sup>23</sup> J. E. Trainer, The auditory acuity of certain birds, Unpublished Ph.D. Dissertation, Cornell University, 1946.

<sup>24</sup> B. F. Skinner, *The Behavior of Organisms*, 1938, 3-457; Are theories of learning necessary? *Psychol. Rev.*, 57, 1950, 193-216.



The other compartment of the experimental box contained the food-magazine, the back of the loudspeaker, and associated wiring.

The apparatus for producing the auditory stimuli, for scheduling onset and duration of stimuli and the administration of reinforcements, and for recording responses, was located in a control room adjoining the experimental room. The auditory stimulus could be turned on and off, and the reinforcing mechanism operated, either manually (by switches controlled by the experimenter) or automatically (from a master control panel). Manual control was exercised during the preliminary training, in which the bird learned to peck at the key, and in the actual determination of thresholds. The apparatus was controlled automatically during the long training sessions in which the birds learned to discriminate between the presence and absence of the auditory stimulus.

A thick wall separated the control room from the experimental room. Loud sounds originating in the control room, including some relay clicks, could be heard in the experimental room and probably even inside the experimental box, but extraneous clues to the presence of the auditory stimulus were effectively eliminated by muffling the relays involved in turning the stimulus on and off. The effectiveness of the muffling was demonstrated by the fact that neither human *O*s nor pigeons were able to detect stimuli of intensities well below threshold with better than chance accuracy.

A beat-frequency oscillator and a noise-generator provided the pure tones and the 'white' noise used as auditory stimuli. Absolute thresholds were determined also for acoustic clicks; this work is discussed elsewhere.<sup>25</sup> The auditory stimulus was turned on and off with an electronic switch that provided for a gradual build-up or decay of intensity over a period of 0.2 sec. The use of this switch avoided the undesirable 'click' produced by transients when the intensity of a tone is changed abruptly. An attenuator, graduated in steps of 2 db., was used to vary the intensity of the stimulus during the threshold determinations.

The stimulus-intensities at threshold are reported as minimal audible field (*MAF*). The intensities of pure tones and noise were measured with a calibrated condenser microphone placed at various locations in the pigeon's compartment at the approximate height of a pigeon's ears. The intensity of the stimulus corresponding to a given voltage across the loudspeaker was computed from the output of the condenser microphone. The output of the condenser microphone varied directly with attenuator setting. It varied also with the position of the microphones in relation to the loudspeaker, and, in the case of pure tones, with frequency.

The reference-position for measuring stimulus-intensities was that of a condenser microphone placed on the midline of the box, 1 in. away from and facing the loudspeaker. The intensity measured was, of course, smaller when the microphone was further away from the loudspeaker than the reference-position. The difference between intensity at the reference-position and other positions in the box is recorded in Table I for the three frequencies at which a large number of thresholds were measured.

The reference-position was a reasonable approximation of the observed position of the pigeon's head during threshold-measurements as the pigeon stood, unmoving.

<sup>25</sup> G. A. Heise and W. A. Rosenblith, Electrical responses to acoustic stimuli recorded at the round window of the pigeon, *J. Comp. Physiol. Psychol.* 45, 1952, 401-412.



facing the key and the loudspeaker. Nothing prevented the pigeon from moving to other positions farther away from the loudspeaker, and it probably more than once occupied several different positions during the 15-sec. auditory stimulus. This variation in position would, in general, tend to raise the measured threshold-intensity. There is no reason, however, to suspect systematic movement, and the high reproducibility of results suggests that random movement was not a serious problem. Errors arising from movement might have been eliminated by Skinner's method of suspending the pigeon in a jacket.<sup>26</sup>

The intensities of pure tones of different frequency were equated on the basis of calibration data relating the output of the condenser microphone to frequency of stimulation. As far as possible, thresholds were measured with those frequencies for which the output of the microphone was equal; appropriate corrections were made for other frequencies.

TABLE I  
THE EFFECT OF POSITION IN THE BOX ON INTENSITY OF STIMULATION  
AT SELECTED FREQUENCIES

Displacement from reference-position		Change in intensity (db.)		
Direction	Distance (in.)	430~	2600~	6600~
Back	2	- 8	-11	- 5
	4	-15	-15	- 6
Left	1.25	0	0	- 1
	2	- 4	-10	- 3
Down	2	-12	-10	- 7
	4	-18	-12	-10

(2) *Training.* Work was done with two groups of naïve pigeons. Training and testing of the first group, three domestic pigeons (Nos. 21, 43, and 51) was completed before work was begun with the second group, three White Carneaux (Nos. 77, 78, and 79). Although the result of the training was the same for both groups, the relatively unexcitable White Carneaux adapted more readily to the experimental situation and received a more standardized program of training. The data on training reported here are based on the White Carneaux.

The training was divided into two stages: (1) preliminary training, in which the bird learned to peck at the key; and (2) discriminative training, in which the bird learned to discriminate the presence or absence of the auditory stimulus. A clearly suprathreshold (88 db. *re* 0.0002 dynes/cm<sup>2</sup>) 2600 ~ tone was used for training all the birds.

(a) *Preliminary.* The birds were deprived of food until they reached 80% of their normal weights before preliminary training was begun. In this condition they readily learned to approach and eat from the magazine whenever the shield in front of the opening was removed. The birds were then taught, through reinforcement contingent upon the proper response, to peck at the key. The training tone was intro-

<sup>26</sup> Skinner, *op. cit.*, 1950, 202.



duced as soon as a bird had learned to peck, and only those pecking responses that occurred during the tone were reinforced. The number of pecks required for reinforcement was gradually increased to 10, and, when this response had been acquired, the bird was ready for discriminative training.

(b) *Discriminative.* The purpose of this training was (1) to secure a distinctive burst of 10 or more pecks immediately upon the onset of the tone, and (2) to minimize responding during the silent periods. In contrast to its performance with visual stimuli, the pigeon develops auditory discriminations very slowly. The major problem of the training was to extinguish the tendency to respond during silent periods. Consequently, the training schedule consisted of brief tones interpolated between much longer intervals of silence.

In the procedure for automatically controlled discriminative training, the tone was turned on once during every 2-min. interval, as scheduled by a tape-timer. The time of onset within the interval was randomly determined. The bird was reinforced only when it pecked the key 10 times within the 15-sec. duration of the tone. The tone was turned off when the bird was reinforced, or, if the bird had not pecked 10 times at the end of 15 sec.

Each bird required about 15 hr. of discriminative training, during which it pecked at the key about 20,000 times, before the response was suitable for threshold-measurement. At the conclusion of the training, the onset of the tone touched off an almost immediate burst of responding, and the tone seldom stayed on for more than 5 sec. The final rate of responding in the presence of the tone was approximately 20 times the rate during the silent periods, in contrast to the beginning of the discriminative training when the two rates had been approximately equal. The bird did peck occasionally during the silent periods, but seldom emitted a burst of as many as 10 pecks. The characteristic response during the silent periods was a single peck, or sometimes a group of two or three pecks close together.

(3) *Threshold-measurement.* Except for the changes in the intensity of stimulus and a more flexible scheduling of its onset, the procedure for determining absolute thresholds was similar to the training procedure. The interval between trials varied from about 30 sec. to 2 min., depending upon the amount of responding during the silent periods. In order to avoid ambiguity of interpretation, the stimulus was turned on only when the bird was not pecking at the key. The stimulus was turned off as soon as a burst of 10 pecks had occurred within its duration, or, if the bird failed to respond with 10 pecks, at the end of 15 sec.

Before the final threshold measurements were begun, the intensity of the stimulus was reduced in a series of trials to about 20 or 30 db. above threshold. Psychophysical functions were then obtained by a modified method of limits, in which the bird was given 3 to 5 trials at each intensity before proceeding to the next higher or lower intensity. At least 10 trials were given with each of a series of intensities which were 10 db. apart, and at least five trials were given at some intensity within each 10-db. interval. One day's session with a bird, lasting about 1 hr., was sufficient to obtain its psychophysical function for one stimulus (e.g. noise, clicks, or a particular frequency).

After the bird had been tested with near-threshold stimuli, the frequency of both long and short bursts during the silent periods between trials increased considerably over the low level attained at the end of the discriminative training. Several procedures were adopted to reduce responding between trials. Responses to stimuli within



10 db. of the approximate threshold were not reinforced; instead, reinforced trials with louder stimuli, about 20 or 30 db. above threshold, were interspersed between the trials with near-threshold stimuli. Sometimes the bird could be quieted during the course of measurement by a long extinction period with the stimulus turned off. After completion of the threshold-determination for a particular stimulus, the bird was given additional discriminative training with the next stimulus on the testing program.

A continuous running record of each series of trials was made with a polygraph. All pecks were recorded in their temporal relation to the presence of the auditory stimuli. From the record it was possible to tabulate the number of response bursts and the number of pecks in a burst, and to measure the latent period between the

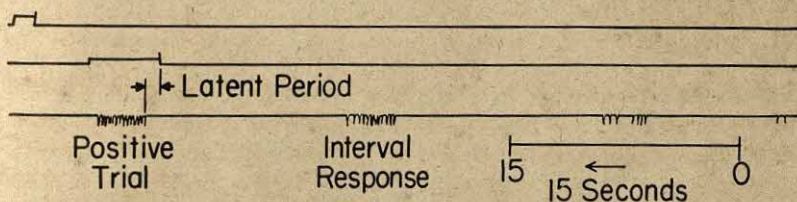


FIG. 1. SAMPLE RECORD

Time runs from right to left. Top line, reinforcement; middle line, auditory stimulus; bottom line, pecking response. Note the two bursts of responses that are not large enough to qualify as 'positive.'

onset of the auditory stimulus and the beginning of the response. A portion of the running record is reproduced in Fig. 1.

Thresholds also were determined for two graduate students at Harvard University. The testing procedure for these *Ss* was made as similar as possible to the procedure used with the pigeons. The experimental box was placed on its side. *S* placed his head inside the experimental box, with his left ear about an inch from the loud-speaker. The same modified method of limits was used, and *S* indicated the presence of the stimulus by pulling a string attached to the key. In addition to the measurements in the experimental box, audiograms for the two *Ss* were made with a Békésy audiometer.<sup>27</sup>

## RESULTS

In the measurement of the threshold for hearing, an *S* who responds in a distinctive, specified manner to an auditory stimulus is said to have 'heard' that stimulus. The relation between the distinctive response and the auditory stimulus is established by training trials in which the *S* learns to execute the distinctive response in the presence of a presumably audible stimulus. In the threshold measurements, *E* knows the intensity of the stimulus and temporal location of the trial. When the distinctive response occurs during a trial, *E* characteristically infers that the conditions for *S* during this trial were comparable to those of the training trials. Thus the *S* who

<sup>27</sup> G. von Békésy, A new audiometer, *Acta oto-laryng.*, 35, 1947, 411-422.



'heard' the stimulus during the training trials is scored as 'hearing' the stimulus on those measurement trials in which he performs the distinctive response. Clearly, this inference is justified only if the occurrence of the distinctive response is restricted to the presentation of the stimulus and if the response never fails to occur when the stimulus intensity is well above threshold. With the pigeons, the distinctive response did not measure 'hearing' unambiguously, since the distinctive response (1) sometimes failed to occur during trials with a loud stimulus, and (2) often occurred during the silent period between stimulus-trials.

The distinctive response that had been reinforced in training was scored as the positive response in the threshold-measurements. A trial was called positive when a burst of at least 10 pecks occurred anywhere within the 15-sec. duration of the stimulus. The 10 pecks, in order to qualify as a positive response, had to occur within a maximal duration of 4 sec.; actually the bird usually took no longer than 2 sec. to execute the 10 pecks. A trial was called negative when the bird either did not respond or fell short of 10 pecks, during the presentation of a stimulus.

The birds sometimes ceased responding to stimuli of *any* intensity, especially toward the end of a day's session. Interpretation of negative trials was not difficult, however, because the cessation of responding was usually abrupt and final. The frequent louder stimuli interpolated among the trials with near-threshold stimuli provided a convenient running check on the possibility that the bird had stopped responding. When the bird did not respond positively to clearly audible stimuli, the data were not used from the immediately preceding trials, back to the last positive trials with loud stimuli.

Ambiguity in the interpretation of positive trials arose because the birds frequently made positive responses during the silent intervals between stimulus-trials. These responses, occurring in the absence of any auditory stimulus, are termed 'interval-responses' (Fig. 1). The presence of interval-responding meant that a certain number of the stimulus-trials would be positive regardless of the intensity of the stimulus. These temporal coincidences between stimulus-trial and positive response would not be distinguishable from other trials, in which the bird was pecking in response to the stimulus. An estimate of the number of these coincidences was provided, however, by the frequency of interval-responding.

The frequency of interval-responses would have been much greater if the numerous groups of less than 10 pecks had not, by definition, been disqualified as positive responses. There was ample justification for the use of this criterion. The shorter bursts were never reinforced in training, and the bird could always be relied upon for a positive response to a loud stimulus unless he had ceased responding completely.

The problem of interval responding, with consequent ambiguity of response, has been present in most investigations of animal hearing. When the stimuli are only slightly above threshold, there is little difference between the stimulus-trials, in which



the animal may be reinforced for the correct response, and the between-trial interval. Consequently the response that is correct for the trial tends to generalize to the between-trial interval. The number of interval-responses also increases if the animal happens to receive reinforcement for responding during a subthreshold stimulus. The most satisfactory solution to the problem of interval-responding is to eliminate the interval in the experimental design by giving clearly defined trials in which the animal is required to perform one of two distinctive responses depending on the presence or absence of the stimulus. The significance of the proportion of correct choices observed can then be tested statistically. This method was used by Schwartzkopff, who required his bullfinches first to land on a perch, and then to go forward to food if a tone sounded, or backward into the cage if the tone did not sound.<sup>28</sup> An analogous technique can be used with the pigeon if two keys are placed in the box. Pecks at one key would then be reinforced in the presence of the stimulus, pecks at the other key in the absence of the stimulus.

Most investigators have attempted to minimize interval-responding by various training and testing procedures. What interval-responding remains is then ignored, or treated on an intuitive basis, in the calculation of the thresholds. In the avoidance conditioning procedure, interval-responding is sometimes negligible or may be kept low by occasional substitution of a shock for the buzzer at low intensities, by punishment for interval responding, or by other means.<sup>29</sup> Investigators using instrumental reward techniques have been less successful in reducing interval-responding to a satisfactory low level. Dworkin and colleagues report, for example, that sometimes "false responses became so frequent as to make it impossible to correlate any of the responses to the known sound-stimuli."<sup>30</sup> This excess activity could sometimes be quelled by a strong stimulus, or by resting the animals for several days to three weeks.

A separate calculation for each block of trials at a particular intensity provided a correction for interval-responding in the present study. A 'block' of trials began when the attenuator was changed to a particular intensity and ended, two to five trials later, when the attenuator was changed to the next intensity in the ascending or descending series. A separate calculation was required for each block because the amount of interval-responding varied from block to block, depending on the particular sequence of intensities on which the bird had previously been tested, on the intensity of the stimuli in the block under consideration, and perhaps on other factors peculiar to the individual bird, such as temperament or degree of food-deprivation.

For each block, the following measures were compiled from the running record of the threshold-determinations: (a) the duration of all the silent periods in the block; (b) the total number of interval-responses; (n) the

<sup>28</sup> Schwartzkopff, *loc. cit.*

<sup>29</sup> Culler *et al.*, *op. cit.*, 1935, 225.

<sup>30</sup> Dworkin *et al.*, *op. cit.*, 293.



number of stimulus-trials in the block; and ( $t$ ) the duration of all the stimulus-trials in the block. Also tabulated were the number of positive trials in the block and the latency of the response on the positive trials. From these data a calculation was made of the 'noise' in each block—the proportion of the trials in the block that would have been positive if the frequency of positive responses during the trials was the same as the frequency of interval-responding in the block. On the assumption that the interval-responses were distributed evenly over the block, the expected rate of interval-responses at any time within the block, in responses per sec., is equal to  $b/a$ . Hence the total number of positive responses expected during the trials, if the bird responded at the same rate in the trials as in the silent intervals, is  $b/a \cdot t$ . Since a block contained  $n$  stimulus-trials, the noise, or average number of positive responses per stimulus, is  $bt/an$ .

The average value of the noise, as estimated by this formula, was about 0.15, with practically all cases falling between zero and 0.40. In other words, an average of 15% of the trials would have been positive even if the stimulus had never actually been presented. Values of noise lower than the average were usually obtained when the intensity of the stimulus was well above threshold; higher than average values were more frequently obtained when the intensity was near or below threshold. The results from the appropriate blocks were combined for each bird to give the proportion of the total trials at each intensity that were observed to be positive, and the proportion of the total trials at each intensity that were positive on the basis of noise. Then the proportion of positive trials observed at each intensity was corrected for noise by the following formula:

$$P = M \cdot C / 1 - C$$

where  $P$  is the corrected proportion of positive trials at a particular intensity,  $M$  is the observed proportion of positive trials, and  $C$  is the proportion of the trials at that intensity expected to be positive on the basis of noise.<sup>31</sup>

The corrected proportions for each bird and each separate stimulus were then plotted as psychophysical functions. The threshold was defined conventionally as the intensity for which the corrected proportion of positive trials was 50%. Individual thresholds for the various birds and the human

<sup>31</sup> This formula was suggested by Dr. H. de Vries. It is derived as follows. If  $P$  is the proportion of the stimulus-trials in which the bird actually responded to the auditory stimulus,  $M = P + C(1 - P)$ . Hence,  $P = M - C / 1 - C$ . It will be noted that  $P$  is zero when the proportion of positive trials observed is equal to the noise level ( $M = C$ ); and  $P = 1$  when the bird always responds to the stimulus ( $M = 1$ ). Further,  $P$  is negative in those cases in which the proportion of positive trials predicted from noise exceeds the proportion of positive trials actually observed.



Ss are given in Table II, and average values are plotted in Fig. 2. The thresholds for the Ss have been corrected for the amount that the audiograms of their left ears deviated from the normal audiogram at the frequencies tested. In general, the pigeons' sensitivity to pure tones was similar to the humans' at low frequencies, but the threshold curve for the pigeons rose abruptly above 4000  $\sim$ . Additional measurements between 4000 and 6600  $\sim$  would have been desirable for more precise specification of the threshold-function in this frequency region.

TABLE II

ABSOLUTE THRESHOLDS (IN DB. $\text{re } 0.0002 \text{ DYNES/CM}^2$ ) FOR PIGEON AND MAN											
S	300 $\sim$	430 $\sim$	700 $\sim$	1000 $\sim$	1600 $\sim$	2600 $\sim$	4000 $\sim$	6600 $\sim$	8000 $\sim$	11,700 $\sim$	Noise
Pigeon No.:											
43	26	11	30	10	19	15	26	68	84		29
21	31	16		17		12	11	72	76		22
77		27				18		74			32
78		27				18		71			29
79		32				28		76			33
Mean	29	23		14		18	19	72	80	Above 88	29
51		73?	77?	59?	35?	31	38	86			38
Man:*											
JM		33 (5)				5 (5)		6 (-22)			16
GH		20 (5)				0 (-5)		1 (6)			9
Mean		27				3		4			12

\* The human thresholds have been corrected for the deviation of each S's audiogram from the normal. The deviations are recorded in parentheses beside the threshold values. The minus sign signifies a hearing loss.

The results from pigeon 51 have nowhere been combined with the data from the five other birds. The deviant data obtained from this bird for the lower frequencies probably resulted from behavioral difficulties rather than from a hearing defect, since the bird's behavior was extremely inconsistent and became progressively worse as the testing proceeded. The lower frequencies were the last tested with this bird.

In Fig. 3 the psychophysical functions for the five birds and the various stimuli have been combined. In order to equate the various psychophysical functions with respect to stimulus intensity, each component function was plotted in terms of relative intensity which was expressed in db. above or below the threshold (arbitrarily set at 0 db.) obtained from the particular function. In general, the component functions were similar in shape and slope, with an average slope of about 7% per db. in the vicinity of threshold. The average slopes of the human functions obtained in this experiment were somewhat steeper—about 10% per db. Fig. 3 shows also the weighted means for each relative intensity obtained by applying the correction for the overall noise at that intensity to the combined proportion of



positive trials observed. The fluctuation of these weighted means about the 0% line at the low intensities indicates a general correspondence between the proportion of positive trials observed at these intensities and the prediction based on noise.

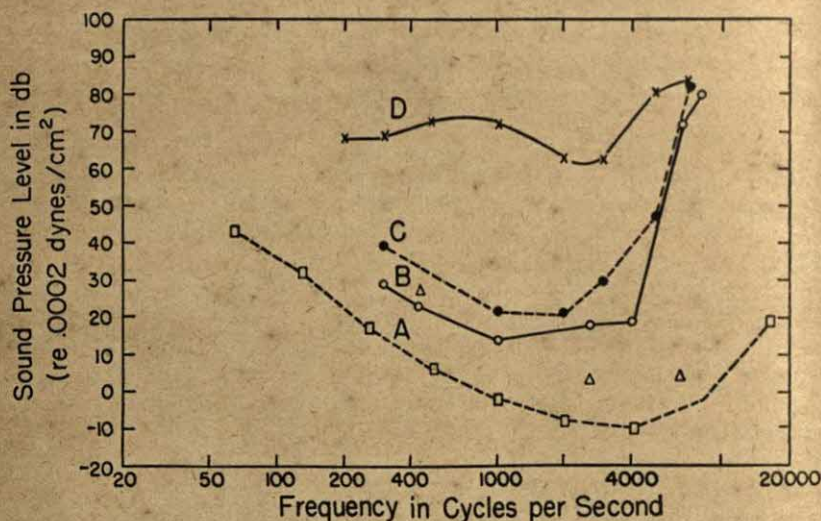


FIG. 2. THE THRESHOLD OF HEARING FOR PIGEON AND MAN DETERMINED BY SEVERAL DIFFERENT METHODS

Human: Curve A, average binaural *MAF* (minimal audible field) for random horizontal incidence (Sivian and White). The triangles are mean thresholds for the two *Ss* of the present experiment, measured with the same apparatus and procedure used for the pigeons. Pigeon: Curve B, mean thresholds obtained in the present experiment. The points at 430, 2600, and 6600 ~ are means for five pigeons; the other points on the curve are means for two pigeons. Curve C, mean thresholds obtained by Trainer with an avoidance conditioning technique. Each value is the mean for three pigeons, except for the point at 7000 ~, which is for one pigeon. Curve D, the intensity of a pure tone required for a cochlear response of 5.4 microvolts (Wever and Bray).

An estimate can be obtained from Fig. 2 of the comparability of the experimental situation employed here with that used by other investigators. The human thresholds obtained in this experiment averaged about 13 db. above the binaural *MAF* for random horizontal incidence reported by Sivian and White.<sup>32</sup> Compared to those used by them, the conditions of the present experiment were far from optimal for measuring human thresholds. The number of *Ss* was small, and variability was in several cases quite large. The *Ss* listened in a somewhat cramped position, and the distance

<sup>32</sup> L. J. Sivian and S. D. White, On minimum audible sound fields, *J. Acoust. Soc. Amer.*, 4, 1933, 288-321.



from loudspeaker to ear probably was not identical for the two Ss. The 'click' of the microswitch when the key was moved may at times have had a masking or distracting effect. Some of these considerations apply to the measurements with the pigeons as well. Consequently, the most valid comparison presented in Fig. 2 probably is the one between the pigeon and human thresholds taken from the present experiment.

It is possible that the thresholds for the pigeons that were obtained in this experiment were to some extent a function of motivational level. Evidence that the maximal auditory sensitivity of the animals was measured in this experiment comes, however, from two sources: (1) the reproducibility of the results under varying conditions; and (2) the close correspondence of the thresholds with those obtained by Trainer, who used an avoidance conditioning technique.<sup>33</sup>

The reproducibility of the data was checked by giving some of the birds an extended series of trials with a 2600 ~ tone of varying intensity. These trials were carried out over several days, and under motivational conditions (defined in terms of food-deprivation) which were not constant either during a single day or from day to day. Nevertheless, the results for the various days could be combined into a consistent psychophysical function for each bird.

Trainer measured the thresholds of three domestic pigeons trapped on the Cornell University campus. The pigeons were required to move from one side of the cage to the other when a tone sounded in order to avoid electric shock. As shown in Fig. 2, the mean thresholds obtained by Trainer fall well within the range of variability of the positively reinforced pigeons of the present experiment.

Wever and Bray have measured the intensity of a pure tone required for a constant magnitude of cochlear response in the pigeon.<sup>34</sup> The top curve in Fig. 2 taken from the plot of their data shows the intensities required with one pigeon for a cochlear response of 5.4 microvolts (the lowest response magnitude for which an equal-response curve was given). The equal-response curve for the cochlear response to pure tones is evidently only a crude indication of the bird's ability to hear as defined by behavioral measures. An electrophysiological measure that does relate more closely to a corresponding behavioral measure is the threshold for the neural component of the round window response to acoustic clicks, as will be shown elsewhere.<sup>35</sup>

The latencies from all the trials for the five birds and the various stimuli were combined to give the observed distribution of latencies for each relative stimulus-intensity. To obtain a sufficiently large number of cases in each distribution, the latencies from each pair of adjacent relative intensities were combined into a single distribution. Thus the latencies from the trials on which the relative stimulus-intensity was 15 or 16 db. were combined into one distribution; the latencies from the trials on which the

<sup>33</sup> Trainer, *op. cit.*, *passim*.

<sup>34</sup> Wever and Bray, *op. cit.*, 359-360.

<sup>35</sup> Heise and Rosenblith, *op. cit.*, 410.



relative stimulus-intensity was 13 or 14 db. were combined into another distribution, and so forth. These paired distributions are plotted in Figs. 4, 5, and 6. Each paired distribution represented in these figures contains

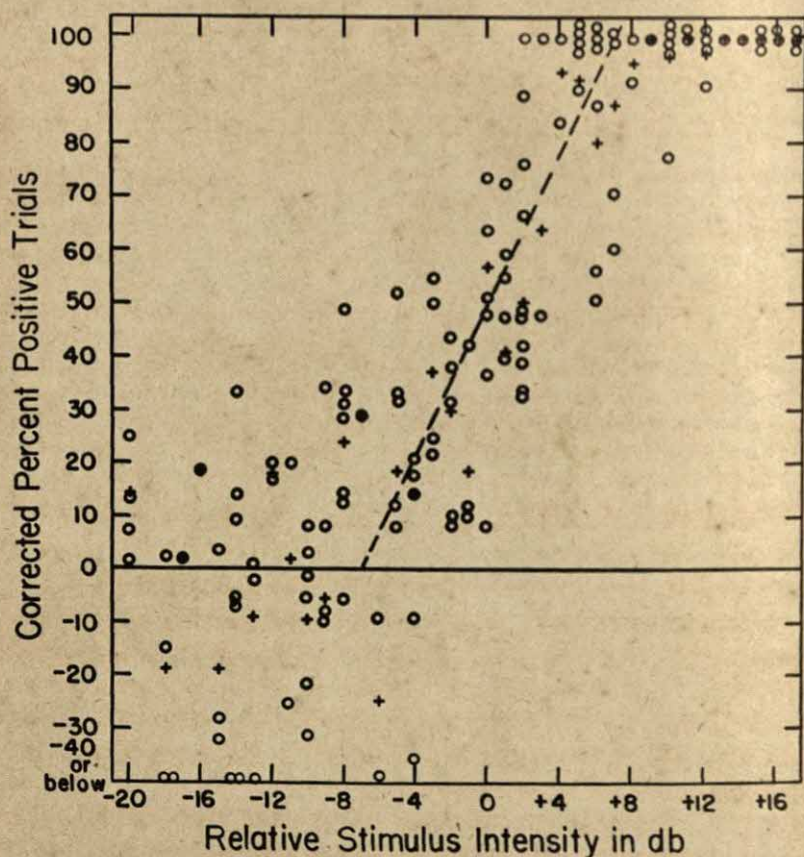


FIG. 3. COMPOSITE PSYCHOPHYSICAL FUNCTION

The points from the corrected psychophysical functions obtained from the five birds for the various stimuli have been placed on one plot by setting the threshold (50% point) of each component function at a relative intensity of 0 db. The crosses are the over-all means for each relative intensity, obtained by combining all the data for that relative intensity and making the appropriate correction for noise. Note the oscillation of the over-all means about the zero baseline at the low relative intensities.

27 or more cases; several distributions contain more than a hundred cases.

The medians and quartiles of the distributions are plotted in Fig. 4. Medians and quartiles rather than means are presented because the distributions include the trials with 'indeterminate latency,' *i.e.* the trials in which the birds did not respond during the 15-sec. interval.

Fig. 4 shows also the median human latencies. The number of latencies in the human distributions was small compared to the number obtained from the pigeons. It is clear, however, that the median latency curve for the humans generally falls well within the interquartile range of the distributions for the pigeons. Interval-responding was negligible for the humans. An even closer correspondence between the two median curves

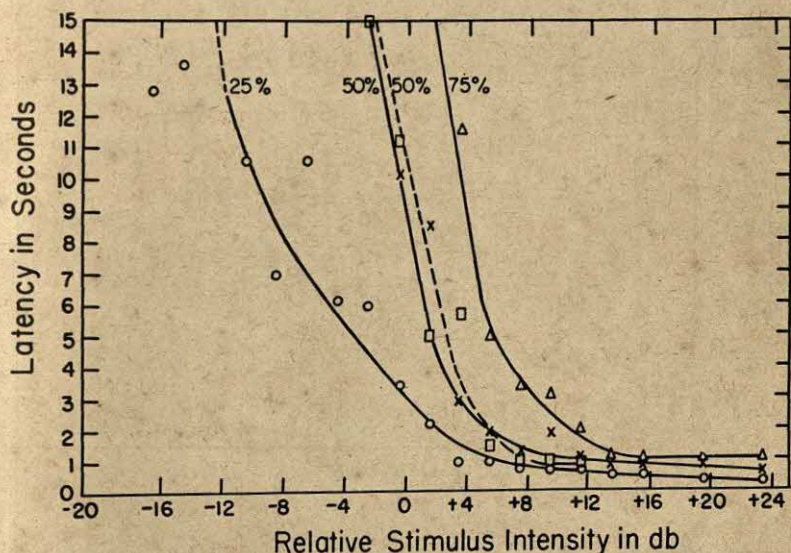


FIG. 4. LATENCY OF RESPONSE AS A FUNCTION OF THE RELATIVE INTENSITY OF THE STIMULUS

The parameter is the proportion of the trials at the particular relative intensity in which the latency was less than or equal to the time on the ordinate. The solid lines were obtained from the combined data for all stimuli for five pigeons. The points at the low relative intensity portion of the 25% curve for the pigeons represent noise. The dotted line was obtained from the human Ss.

might have been obtained if the distributions for the pigeons had not contained latencies contributed by responses due to noise.

The similarity of pigeon and human latency curves for the intensities close to threshold is without any particular significance. The very close correspondence between the latencies observed for pigeons and humans at higher intensities does, however, support the assertion, made previously in connection with stimulus-calibration, that the pigeon remained in a relatively fixed 'ready' position between trials. The humans can be assumed to have been always alert and ready to respond during the determinations. The fact that the pigeons took no longer than the humans to respond at



these higher intensities suggests that the pigeons behaved similarly.

Fig. 5 shows the cumulative distributions of latencies at various stimulus-intensities. Fig. 6, derived from Fig. 5, shows the proportion of the total trials in which the positive response occurred within each  $1/3$  sec. of the time of each trial. The curves for the lowest intensities, where the observed proportion of positive responses was down at the noise-level, indicate that the positive responses were, in general, evenly distributed over the trial period. This fact substantiates the assumption, made in the calculation of

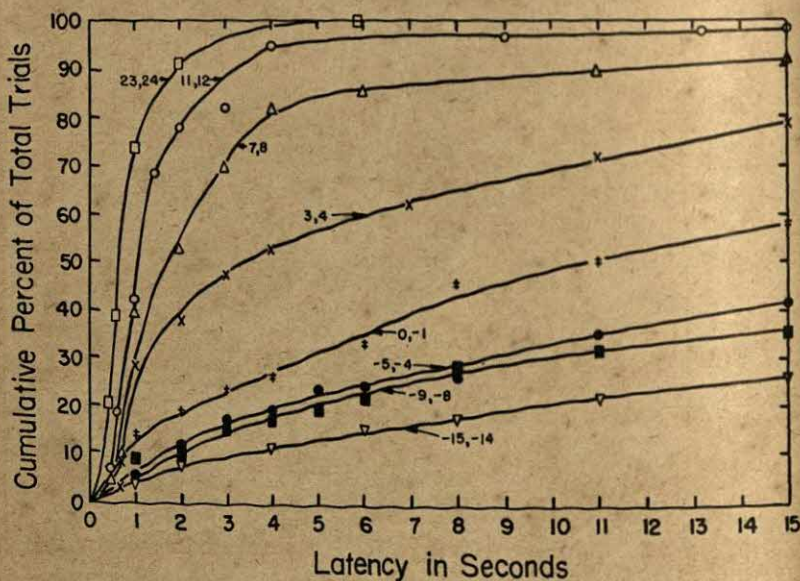


FIG. 5. CUMULATIVE DISTRIBUTIONS OF LATENCIES FOR FIVE PIGEONS

The parameter is relative stimulus-intensity. The latencies from adjacent pairs of intensities have been combined into one distribution.

noise-levels, that interval-responses were distributed evenly over a block of trials.

The data obtained from the threshold-measurements permit comparison of the variation with stimulus-intensity of two commonly used measures of response strength—'probability of response' and the latency. The 'probability of response' is defined conventionally as the proportion of the trials on which the pigeon responded to the stimulus. The relation between this probability and the intensity of the stimulus is given by the various psychophysical functions. The average probability of response, for all birds and stimuli, is shown as a function of relative stimulus-intensity in Fig. 3.

As a rough approximation, this probability changes from 1 to 0 as the intensity decreases from +7 to -7 db.

Figs. 4, 5, and 6 show how the characteristics of the distributions of latencies varied as a function of the relative stimulus-intensity. Fig. 4 shows that the median and quartile latencies increased rapidly over the range of stimulus-intensities for which the response-probability was decreasing. It

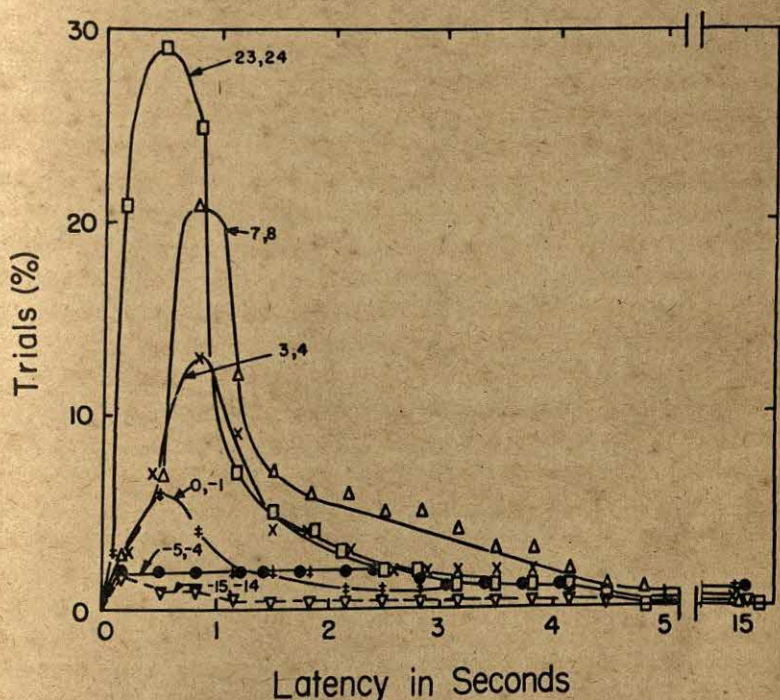


FIG. 6. THE DISTRIBUTION OF THE LATENCIES FOR FIVE PIGEONS

The parameter is relative stimulus-intensity. The points on each curve represent the proportion of the total trials at each intensity in which the onset of the response occurs within each  $\frac{1}{3}$  sec. of the trial period (cf. Fig. 5).

does not, however, establish a simple inverse relation between the intensity of the stimulus and the observed latency of response. The observed changes in the median and quartile latencies could result solely from the differing probabilities of the positive response to the stimulus at the various intensities. In other words, the increase in median and quartile latencies might be accounted for, not by any shift in the *distribution* of latencies toward longer time-delays, but rather by the smaller proportion of positive responses.



appearing within the trial-intervals. Figs. 5 and 6 support this interpretation. They show that the number of cases in the latency distributions decreased as the relative stimulus-intensity decreased, but that the modal latency remained essentially constant regardless of stimulus-intensity or response-probability. The bird's probability of response decreased as the intensity decreased; but when the bird did respond, it was still most likely to respond at a constant time-delay after the onset of the stimulus. A constant modal latency for different levels of response-probability has also been observed by Skinner.<sup>36</sup> In his experiment, the pigeon's probability of response was varied by altering the degree of deprivation or by withholding reinforcement rather than by changing the intensity of the stimulus.

If, however, the relation between mean (or median) latency and relative stimulus-intensity is determined only from those trials on which the birds actually responded, the result is consistent with the findings of Wendt for monkeys and humans,<sup>37</sup> and Piéron<sup>38</sup> and Chocholle<sup>39</sup> for humans. These investigators presumably computed mean latencies only from those trials on which a response was made. They found that the mean latency or reaction-time dropped off most rapidly as the intensity of the stimulus was increased just above threshold, and thereafter fell off more gradually. Both Piéron and Chocholle showed that mean reaction-time continued to decrease until the intensity of the stimulus was at least 30 db. above threshold.

Hull has cited that the inverse relation between mean latency or reaction-time and stimulus-intensity discussed in the preceding paragraph as evidence for a 'stimulus-intensity dynamism,' or the influence of stimulus-intensity *per se* upon response-strength.<sup>40</sup> It is questionable, however, whether this mean latency provides a satisfactory description of behavior when probability of response is varying. As the threshold is approached and responses appear on a smaller and smaller proportion of the trials, this mean latency becomes decreasingly representative of behavior. The change in this mean latency with stimulus-intensity itself derives from the changes in the characteristics of the latency distributions and in response-probability with stimulus-intensity. These measures—the characteristics of the latency

<sup>36</sup> Skinner, *op. cit.*, 1950, 197.

<sup>37</sup> Wendt, *op. cit.*, 22-23.

<sup>38</sup> Henri Piéron, Nouvelles recherches sur l'analyse du temps de latence sensorielle et sur la loi qui relie ce temps à l'intensité de l'excitation, *Année psychol.*, 22, 1922, 58-142.

<sup>39</sup> R. Chocholle, Variation des temps de réaction auditifs en fonction de l'intensité à diverses fréquences, *Année psychol.*, 41, 1940, 65-124.

<sup>40</sup> C. L. Hull, Stimulus intensity dynamism (V) and stimulus generalization, *Psychol. Rev.*, 56, 1949, 67-77.

distributions and the probability of the response—provide the fundamental and comprehensive description of the relation between stimulus-intensity and response-strength in the vicinity of the absolute threshold.

#### SUMMARY

Absolute thresholds for pure tones and 'white' noise were determined for six pigeons by an operant conditioning technique. After the animals had learned to peck at a small key for a food reward, absolute thresholds were measured by a modified method of limits. The entire procedure was recorded on moving tape, permitting measurement and tabulation of all responses as well as the correlation between responding and the onset and duration of the auditory stimuli. The psychometric functions obtained were essentially similar in form for the different pigeons and for the various stimuli used. The threshold-curve for pure tones was in close agreement with data obtained elsewhere by avoidance training. In general the pigeons' sensitivity to pure tones was similar to that of human *Ss* at low frequencies, but the threshold-curve for the pigeons rose abruptly above 4000  $\sim$ . The median latency of the response to the auditory stimulus increased as the stimulus-intensity approached the threshold, but the modal latency remained essentially constant.



## ECOLOGICAL CUE-VALIDITY OF 'PROXIMITY' AND OF OTHER GESTALT FACTORS

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Gestalt psychologists have stressed the influence of certain stimulus-factors upon figural unity in perceptual organization. Prominent among these factors are 'proximity,' 'equality' (or 'similarity'), 'symmetry,' 'good continuation,' and 'closure' in the sense of the closedness of a line or pattern in the stimulus-configuration.<sup>1</sup>

According to orthodox Gestalt theory, the effectiveness of these factors rests on dynamic processes inherent in the brain field, rather than on accumulated past experience; while occasioned by respective characteristics of the stimulus-configuration which acts as a set of 'topographical' factors at the boundary of the system, the dynamics themselves are in the nature of 'physical Gestalten,' that is, of a spontaneous physiological 'self-distribution' built into the organism prior to, and as a condition for—rather than as a result of—learning.<sup>2</sup> For this reason it is also said that the factors mentioned operate in an 'autochthonous' manner, that is, are indigenous to the organism so far as their organizational effect is concerned.

A more broadly functionalistic view of perception would suggest an alternative interpretation of the factors of perceptual organization which at the same time would be well in keeping with modern learning theory. According to this view these factors would be seen as guides to the life-relevant physical properties of the remote environmental objects, and thus as playing a part in adjustment; in more technical language, they would be conceived of as proximal 'cues' to the so-called distal bodily reality.

The possibility of such an interpretation hinges upon the 'ecological validity'<sup>3</sup> of these factors, that is, their objective trustworthiness as potential

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<sup>1</sup> Following an analysis by G. E. Müller, the classic presentation is by Max Wertheimer, *Untersuchungen zur Lehre von der Gestalt: II. Psychol. Forschung*, 4, 1923, 301-350; abridged translation in W. D. Ellis, *A Source Book of Gestalt Psychology*, 1938, 71-81.

<sup>2</sup> Wolfgang Köhler, *Gestalt Psychology*, 1947; esp. 107, 132 f. For the operation of the factor primarily under consideration in the present study, 'proximity,' a striking model from the physical chemistry of liquids (the diffusion phenomenon of 'cassiotaxis') is offered by Wolfgang Metzger, *Gesetze des Sehens*, 1936, 169; the example used is the frequently quoted case of the well-known perceptual organization of the star cluster, 'big dipper,' in accordance with the 'law' of proximity.

<sup>3</sup> Egon Brunswik, *Systematic and Representative Design of Psychological Experiments*, Univ. of Calif. Syllabus No. 304, 1947; 30, 34 ff.



indicators of mechanical or other relatively essential or enduring characteristics of our manipulable surroundings. This problem is analogous to that of the 'physiognomic' or other external cues offered to the organism for potential utilization in social preception. Here we know, for example, that height possesses statistically significant ecological validity with respect to intelligence; but we also know that this validity, which may be expressed in terms of a correlation coefficient, is of a very low order (perhaps 0.1 or 0.15). Thus the cue is a crudely 'probabilistic' one, and so would have to be any learning involved in its acquisition. Another analogy is with the so-called depth-cues in the perception of third-dimensional space, such as vertical position or subdivision of the field. For some of these cues, ecological validities somewhat higher than those typical of social perception, but still of definitely limited value, have been established by Seidner.<sup>4</sup>

Any study of ecological validity can be no more than propedeutic to psychology; concern is limited to a survey of statistical relations among variables as typical of the natural or cultural habitat of an individual or group while the question of the actual utilization of cues or of other aspects of organismic response is left untouched. In short, such studies deal with *potential* cues, not with cues actually employed. Yet ecological surveys are indispensable not only for an understanding and appraisal of responses but, as is especially true in our particular case, for general problems in psychological theorizing as well.

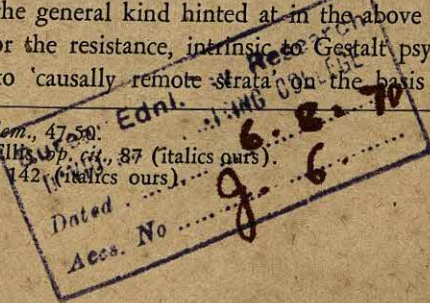
There can be no doubt that the ecological validity of the Gestalt factors, when seen as potential perceptual cues, could likewise be of no more than very limited value. In part perhaps for this reason, but certainly at least in part by virtue of their predilection for dynamic rather than learning-type explanations, Gestalt psychologists were prone to brush aside suggestions of 'generalized experience' as the possible source of the laws of perceptual organization. Thus they were pointing out that there are "in nature . . . *fully as many* obtuse and acute angles . . . [e.g. the branches of trees]" as there are right angles.<sup>5</sup> This statement was made to explain preference for rectangularity as a *prægnant* form of organization. Or they were pointing out that "my general experience is that, *as often as not*, similar members of a group are movable, and move, independently."<sup>6</sup> This in a broad presentation of similarity, proximity, and other organizational factors as allegedly autochthonous principles. In effect, statements of this kind atomize reality by playing down regularity in our surroundings or by asserting ecological zero-correlations.

Our aim is to take the guesswork out of the ascertainment of frequency relationships of the general kind hinted at in the above quotations. This is to make up for the resistance, intrinsic to Gestalt psychology, against extrapolations into 'causally remote strata' on the basis of probabilistic

<sup>4</sup> See Brunswik, *idem.*, 47-50.

<sup>5</sup> Wertheimer, in *Ellis, op. cit.*, 87 (italics ours).

<sup>6</sup> Köhler, *op. cit.*, 142 (italics ours).





cue validity,<sup>7</sup> and against the 'cue' or 'sign' concept in perception in general.<sup>8</sup>

### PRESENT STUDY

The present study thus endeavors to extend the purely ecological type of analysis from physiognomic and depth cues to some of the Gestalt factors mentioned at the beginning of this paper.<sup>9</sup> In the main, we will concentrate on 'proximity',<sup>10</sup> with a side-glance at 'symmetry' and 'closedness'.<sup>11</sup> The problem is, then, whether these factors, when present in the proximal stimulus-configuration on the retina or in a picture, possess some objective indicative value as to the unities in the underlying distal physical reality. In other words, in tending, as we are said to do, to unite closely adjacent (or symmetrical, or closed) sets of elements of the stimulus-configuration into a common 'figure,' are our chances improved that the figural units seen will correspond to the stable mechanical units we are able to manipulate behaviorally? This question can be answered in the affirmative if there is a statistically significant correlation, however low, between the picture factors and the structure of the geographic reality depicted in the picture in such a way that closely adjacent (or symmetrical, or closed) pictorial elements can be traced with greater frequency to a single mechanically coherent object, rather than to incidental configurations without lasting value or significance.

*Materials.* Our material thus must fulfill two conditions. First, it must be a representative sample of stimulus-configurations or situations and situational elements from a universe of conditions to which we are exposed.<sup>12</sup> Secondly—an automatic consequence of the fulfillment of the first requirement—the material must be clas-

<sup>7</sup> Brunswik, *Wahrnehmung und Gegenstandswelt*, 1934, 112, 228. For an answer to Metzger's criticism of the cue concept as defined in this book see Brunswik, Remarks on functionalism in perception, *J. Personality*, 18, 1949, 56-65, esp. 58. For a broader recent discussion of the 'non-distal' orientation of Gestalt psychology see Brunswik, *The Conceptual Framework of Psychology*, 1952.

<sup>8</sup> For Kurt Koffka's views see his *Principles of Gestalt Psychology*, 1935, 160 and elsewhere in the book. See further Metzger, *Psychologie*, 1941, 15-17; concerning the defense of the cue-concept see references in footnote 7.

<sup>9</sup> A preliminary report was given by the writers, under the title "Ecological validity of Gestalt factors as perceptual cues," at the 1951 meetings of the Western Psychological Association (abstracted in *Amer. Psychol.*, 6, 1951, 496).

<sup>10</sup> Also known as 'nearness' or 'adjacency' in the visual field, and not to be confused with the concept of proximal in contradistinction to distal in the regional definition of stimuli or responses.

<sup>11</sup> The term is used here in contradistinction to 'closure'; this latter term we should like to see reserved for the tendency of the perceiver to close up gaps when responding to non-closed, stimulus configurations.

<sup>12</sup> Concerning the methodological requirement of 'representative design' and its application to psychological ecology see Brunswik, *op. cit.*, 1947, Chap. II, and VI to IX.



sifiable both proximally, *i.e.* in terms of characteristics of the retinal projection or of a picture *per se*, and distally, *i.e.* in terms of an underlying geographical stratum merely represented but not actually present on the retina or in the picture.

In the gestalt psychologists early attempts to demonstrate the organizational effect of factors in the stimulus-configuration none of these requirements was fulfilled. With only scattered exceptions (such as the star clusters referred to in footnote 2), the stimuli considered are limited to standardized dots or simple lines; and these elements and patterns lack representational meaning, thus discouraging even the thought of an ecological analysis.

A convenient approximation to situational representativeness in the field of perception is the use of pictures from popular magazines. After some deliberation, we settled on a series of varied episodes from the current motion picture, "Kind Hearts and Coronets," as selected for reproduction in *Life* magazine.<sup>13</sup> There were seven situations, all of which we used; they are presented without omission of any part in Figs. 1 to 7. In the original, all reproductions are 4 in. wide; heights vary in proportion to those shown. As is to be expected, the description of detail in our figures falls considerably short of that in the magazine pictures. Our illustrations are merely to be taken as rough guides.

#### DEFINITION AND CLASSIFICATION OF PROXIMITY

The survey on proximity was based on all pairs of discernible adjacent parallel lines in the picture. As Koffka has lucidly explained, proximity is not to be understood as operating "between events of *any* kind"; proximity cannot be divorced from the other factors, it must be defined as "proximity of similars."<sup>14</sup> In our case the similarity was defined primarily as parallelism, and thus in terms of direction.

The criteria of selection of our sample were further specified as follows.

(1) The line-pairings had to be adjacent, that is, not separated by a third parallel. An example is, in Fig. 7, the separation between the boundaries of the tree trunk in the center (Item q); on the other hand, the separation between the left boundary of this tree and the left boundary of the tree at the extreme right (Item [s])<sup>15</sup> was not included because there was a third parallel between them.

(2) The two lines forming the pair had to be at least in part overlapping, that is, they had to share a common perpendicular. An example of a part-overlapping line-pairing included in our sample is, in Fig. 7, the separation between the inner boundaries of the two tree trunks mentioned above (Item r). Excluded was, for example in Fig. 5, the separation between the front edge of the box placed on the table between the two men and the lower edge of the picture partly visible in the extreme upper left corner (Item [n]); the two lines, although parallel, do not overlap.

(3) Near-parallel pairs with not more than 5° divergence were included in our

<sup>13</sup> *Life*, June 19, 1950, 79-84. Reproduced by permission.

<sup>14</sup> K. Koffka, *op. cit.*, 164-167.

<sup>15</sup> Examples of items not included in the sample are placed in brackets [ ] here and in the figures.





Fig. 4



Fig. 3



Fig. 2



Fig. 1



Fig. 7



Fig. 6



Fig. 5

sample. An example is, in Fig. 7, the separation between the center tree and the branch or tree extending upward in continuation of the profile of the man at left (Item p); the separation between the lines was in such cases defined as the approximate average distance.

(4) Curved or part-curved lines were also included when they could reasonably be called parallel. Included was, for example in Fig. 1, the visible part of the clerk's collar (Item c, which is at the same time an example for slight divergence); excluded was, for example in Fig. 4, the space between the brims of the straw hats of the woman obscuring the view of letters T and E in 'VOTES' and the woman standing in the left foreground of the picture (Item [j]).

Excluded were, furthermore, 'good' continuations of interrupted lines, the other part of which had already been considered. An example is, in Fig. 5, the picture frame after having been interrupted by the clock (Item [I]); only one side of the interrupted line pair was counted.

Excluded were, finally, all cases of distinctly poor photographic definition, for example, in Fig. 5 the frame of the picture at the top right (Item [m]).

In applying these stipulations, a total of 892 separations was obtained as our final sample. In terms of proximity as our potential cue-variable, the sample was divided into eight intervals in geometrical progression from the limen to 0.5 mm., 0.5 to 1 mm., 1 to 2 mm., 2 to 4 mm., and so forth, to 32 to 64 mm. Distribution and (geometric) means for the total sample are shown in the bottom part of Table I. The mode is at the Interval No. 2, 0.5 to 1 mm. Grouping all separations to coincide with the geometric mean between the limits of their respective intervals, their geometric mean is 1.37 mm. Also shown is the mean and sigma in terms of class interval numbers, 2.95 and 1.54; since intervals progress in a geometrical scale so far as the measured values are concerned, 2.95 is in effect likewise a geometric mean.

It seemed possible that the perceptual system may come to utilize not picture-proximity as such, but rather separations as seen in three instead of two dimensions.<sup>10</sup> Since this argument invokes principles of perceptual size- and shape-constancy, it introduces a distal aspect into the classification; this is, however, an aspect quite different from our main, mechanical-distal referent variable of 'coherence.' To meet the present argument, the sub-sample of separations for which both lines seemed to lie at (approximately) the same depth, or in a common frontal parallel plane, was considered separately from the total sample. Data relating to this sub-sample are shown in the tables in parentheses beneath the main figures. Our *N* for this sub-sample is 483; note that mode and mean as well as over-all distribution are in close agreement with those of the full sample.

#### DISTAL CLASSIFICATION OF PROXIMITY IN TERMS OF MECHANICAL COHERENCE

All classification in terms of distal variables, that is, of the realities depicted by the line-pairings, involves interpretation of picture content and thus implies a certain degree of uncertainty. In cases of doubt, which fortunately turned out to be fewer than anticipated, the two authors consulted with each other and, if necessary, with further observers unfamiliar with the purpose of the investigation.

<sup>10</sup> This point was made in a seminar discussion by Mr. Gordon Bronson.



The distal variable chosen as the referent variable in our analysis of possible signification of the cue variable may be labelled 'mechanical coherence.' The following schema of six discrete categories differentiating in terms of, or at least relevant to, mechanical coherence was arrived at after considerable trial and error and deliberation. The schema is exhaustive in that it makes it possible to place each of the 892 separations in one and only one category.

(1) *Fully exposed mechanical units.* This category comprises all separations in which both lines of the pair represent mechanical boundaries of one and the same mechanically coherent object.

Examples are, in Fig. 1, the width of the crown of the hat worn by the man in the center (Item a), or, in Fig. 7, the vertical tree trunk in the background between the two men (Item q). Of all our categories this is the most frequently represented, encompassing a total of 334 items (163 of them representing pairs in a common frontal plane).

(2) *Passages and overlapped recessions.* Under this category are subsumed separations representing holes, gaps, passages, or spaces *between* mechanical units; these separations traverse over a depth or recession, with the background fill overlapped on both sides by other objects.

Examples are, in Fig. 1, the dark doorway (Item b), or, in Fig. 6, the curved dark background area between arch and lamp (Item o). In all, there are 171 (73) such items in our sample.

(3) *One-sidedly covered mechanical units.* These are separations in which one of the lines represents the mechanical boundary of one object, the other the mechanical boundary of another object overlapping the first.

Examples of such protruding object parts are, in Fig. 7, the bright lower part of the coat of the man at left extending beneath the right coat sleeve (Item t), or, in Fig. 1, the visible end of the white cuff on the clerk's left wrist (Item f). There are only 56 (21) items of this kind in our selection of pictures.

(4) *Ornamental divisions.* This category comprises separations defined by color inhomogeneities on a coherent surface, such as stripes or flat moldings, so long as they are not caused by light and shade distribution.

Examples are, in Fig. 2, the vertical stripes on the object behind the officer's right upper arm (Item g), or, in Fig. 4, some of the letters on the large sign in the center (Item k). There are 204 such items in our pictures. While in all other categories the frontal sub-group is considerably smaller than the full group, in this case it is only about 10% less (182). This is due in part to the fact that the parallel edges of stripes or moldings are frequently flat, and in part to their better definition in the picture in those cases in which they are located frontally.

(5) *Shadow-bounded separations.* This category defines separations for which one line of the pair is a shadow contour and the other represents a mechanical boundary of the object *on which* the shadow is cast. Many items in this category do not represent shaded areas but rather normally illuminated areas "left over" after deduction of a shadow.

Examples are, in Fig. 3, the lighted expanse of the upper end of the ship's door above the head of the captain (Item i), or in Fig. 1, the separation between the



edge of the panel in the lower right corner of the picture and the right contour of the umbrella shadow mentioned below (Item e). This is the least populous category, with a total of only 33 (13) items.

(6) *Shadows*. As defined for our purpose, this category is limited to separations between two shadow contours (penumbras), or between one shadow contour and a mechanical boundary of the object *casting* the shadow.

An example of the former, bilateral shadow is, in Fig. 1, the shadow cast by the umbrella handle on the panel of the cabinet in front (Item d); an example of the latter, unilateral shadow is, in Fig. 2, the shadow of the knife blade on the table in the lower righthand corner of the picture (Item h). There are 94 (31) items in this category.

It can be readily seen that our classification system has bearing on its purpose of differentiating between mechanically enduring and essential, and mechanically less essential or unessential cases. Category (1), comprising more than one third of our separations, represents mostly relatively stable spatial relations, invariant even under dislocation of the object; on the other hand, many of the passages and recessions of Category (2)—with the exception of such items as fixed doorways—will easily change at the flick of a hand, and so will one-sided overlappings of Category (3). Shadow distribution, Categories (5) and (6), is altogether dependent on changing conditions of illumination, and ornaments, Category (4), are likewise in themselves mostly misleading as to behavioral orientation. It must be noted for later discussion, however, that shadows, as defined under Category (6), especially when narrow, are frequently instrumental in bringing to prominence the mechanical boundaries of manipulable physical bodies (as witness the shadows that help establish the identity of the door mentioned above in connection with Item i of Fig. 3).

#### ECOLOGICAL VALIDITY OF PROXIMITY

Results of the ecological analysis of the proximity-coherence relationship are presented in our two tables. In the six center pairs of rows of Table I, distributions and means are shown for the six categories in a manner described further above for the bottom row of totals. In Table II, critical ratios between these means are shown for a series of cogent juxtapositions of Category (1) with the other categories, single and combined. The ecological relationships involved are summarily expressed by the point-biserial  $r$ . The two variables correlated are degree of proximity as the 'proximal' or picture variable, and mechanical coherence as dichotomized in any one of the juxtapositions just mentioned as the 'distal' or significance variable.

Results confirm the hypothesis we had from the start, to the effect that smaller separations, that is, greater proximity between adjacent parallels, tends to go with greater frequency of mechanical coherence in the distal,



reality underlying our pictures. This is revealed by the fact that the relatively incidental separations subsumed under Categories (2) to (5) show larger means than does mechanical unity Category (1); this is especially true so far as Categories (2) and (5) are concerned. In line with this, the point-biserial correlation between proximity, on the one hand, and the dichotomized variable of exposed mechanical unity Category (1) *vs.* passages and overlapped recessions Category (2), on the other, is positive; the

TABLE I  
FREQUENCY DISTRIBUTIONS OF LINE-SEPARATIONS IN A  
REPRESENTATIVE STUDY OF PROXIMITY

(The top figures refer to the cases drawn from the over-all sample; the bottom figures (in parentheses) to those cases out of the total in which the two edges or contours represented by the lines may be assumed to be located in the same frontal plane.)

Distal realities represented by line-separations	Proximal cue-variable: 'Proximity' of adjacent parallels in the pictures											
	Class intervals of line-separations (in mm.)								N	Interval		Geom. mean (mm.)
	<0.05	0.5-1	1-2	2-4	4-8	8-16	16-32	32-64		Mean	SD	
(1) Fully exposed mechanical units	57 (24)	126 (73)	67 (24)	43 (18)	29 (17)	10 (6)	2 (1)	0 (0)	334 (163)	2.70 (2.71)	1.34 (1.38)	1.15 (1.16)
(2) Passages, overlapped recessions	16 (7)	31 (7)	36 (17)	30 (15)	20 (10)	16 (7)	15 (7)	7 (3)	171 (73)	3.88 (4.07)	1.92 (1.87)	2.60 (2.97)
(3) One-sidedly covered mechanical units	4 (2)	18 (9)	14 (7)	9 (1)	7 (1)	4 (1)	0 (0)	0 (0)	56 (21)	3.16 (2.67)	1.37 (1.16)	1.58 (1.12)
(4) Ornaments (e.g. stripes, flat moldings), letters	14 (8)	87 (80)	54 (46)	16 (16)	33 (32)	0 (0)	0 (0)	0 (0)	204 (182)	2.84 (2.91)	1.18 (1.19)	1.27 (1.33)
(5) Shadow-bounded separations	0 (0)	4 (2)	9 (1)	10 (7)	7 (3)	2 (0)	1 (0)	0 (0)	33 (13)	3.91 (3.85)	1.21 (.93)	2.66 (2.55)
(6) Shadows	46 (13)	27 (10)	8 (5)	7 (2)	5 (0)	1 (1)	0 (0)	0 (0)	94 (31)	1.95 (2.00)	1.23 (1.16)	.68 (.71)
Totals	137 (54)	293 (181)	188 (100)	115 (59)	101 (63)	33 (15)	18 (8)	7 (3)	802 (483)	2.95 (2.97)	1.54 (1.53)	1.37 (1.33)

specific value of  $r$  is 0.34 (0.37 for frontal separations). The critical ratio of the difference between the means for the two categories involved is 7.15 for an  $N$  of 505 lines pairings (5.53 for the 236 frontal items), and the relationship is thus statistically highly significant.<sup>17</sup> Expanding the second item in the dichotomy to a pooled category consisting of passages, overlapped recessions, and ornaments—Categories (2) and (4) combined—yields 0.20 (0.18); this is lower than the first figure(s) but still statistically highly significant.

The only incidental type of line-separation for which the mean is smaller than for mechanical units is 'shadows', as defined under Category (6). This indicates that in our sample shadows tend to be still narrower than

<sup>17</sup> Since the standard error of the point-biserial  $r$  could not be found in any of a number of current advanced statistical texts, the critical ratio of the means was used as the only test of significance.

exposed mechanical units. The proximal-distal correlation becomes significantly negative in this case,  $-0.22$  ( $-0.19$ ). It must be kept in mind, however, that our material is based on situations with relatively artificial illumination; therefore, the pattern of shadows may be less representative of our normal surroundings than are other features of our sample, although common observation would seem to confirm that shadows are relatively frequently narrow.

A further pair of correlations shown in Table II,  $0.12$  ( $0.13$ ), is that linking proximity with exposed mechanical unity vs. a pooling of all other

TABLE II

ECOLOGICAL VALIDITY COEFFICIENTS OF THE FACTOR OF PROXIMITY (NEARNESS) OF LINES IN THE PICTURES (AS THE PROXIMAL STIMULUS-VARIABLE), AND VARIOUS JUXTAPOSITIONS OF MECHANICAL UNITY WITH OTHER UNDERLYING CAUSES (AS THE REPRESENTED DISTAL STIMULUS-VARIABLE)

Correlations between proximity (nearness in line-separations in the pictures), and Category (1) vs:	N	r	CR
Category (2)	505 (236)*	.34 (.37)	7.15 (5.53)
Categories (2) and (4)	709 (418)	.20 (.18)	5.45 (3.54)
Categories (2)-(6)	892 (483)	.12 (.13)	4.00 (2.91)
Category (6)	428 (194)	-.22 (-.19)	5.10 (2.98)

\* For explanation of figures within parentheses see Table I.

categories, including shadows. The low values obtained reflect the fact, just discussed, that shadows countermand the trend in the rest of the mechanically relatively incoherent separations. On account of the large  $N$ , 892 (483), even these low over-all figures are statistically significant; the ecological validity of the proximity factor as a cue for mechanical coherence thus remains unchallenged by inclusion of the special category, shadows, displaying a contrary trend.

Since, as referred to above, shadows help to bring out mechanical units in perceptual figure-ground organization, they may be pooled with this category rather than with the others. The coefficient obtained by such juxtaposition of Categories (1) and (6) vs. Categories (2), (3), (4) and (5)—not shown in Table II—is  $0.26$  ( $0.21$  for the frontal sub-sample), with a  $CR$  of  $8.09$  ( $4.77$ ). This may be taken as an alternative, somewhat more liberal measure of the over-all ecological validity of the proximity cue.



## PRELIMINARY ANALYSIS OF SYMMETRY AND CLOSEDNESS

Much higher ecological validity than was obtained for proximity is foretold, by limited evidence, for the stimulus-factors of closedness and symmetry. Two proximal classifications were used in this context, both involving joint reference to these factors.

The first was a combined 'symmetry-closedness' factor. Only curved lines returning to their origin and showing symmetry about at least one axis were included in this class. A total of only 11 cases satisfying this criterion could be detected in our set of pictures. Most of them were in Fig. 5, for example, the outline of the bottle in the center (Item z), or the face of the clock in the right center background (Item x). All 11 items signify object units; thus the obtained ecological validity would be perfect for this limited sample; but statistical significance is of course not established, and no such ideal value should be expected upon closer scrutiny.

Secondly, a separate 'symmetry-without-closedness' classification was defined by curved outlines not closed but involving at least a half-turn, that is, possessing at least one pair of parallel tangents touching the curve in opposite directions, and with the added stipulation that the part of the curve between the parallels be symmetrical. An example is, in Fig. 5, the rim of the fruit bowl at the left front (Item y); in this case the incompleteness is due to 'interception' by other objects blocking out part of the rim, thus establishing a valid instance of the familiar perceptual depth-cue. Another example is, in Fig. 1, the outer circumference of the hat (the width of which constituted Item a). Of the total of 10 cases in this group all but one signify object unity. The lone exception is, in Fig. 5, the bowl-shaped object between the head of the man at the right and the picture referred to above as Item [m]; the outline of its counterpart on the other side of the clock was too hazy in the picture to be eligible for our sample.

## DISCUSSION

As all studies aiming at representativeness, the present analysis may in a strict sense be applied only to the specific natural-cultural universe from which our sample is drawn. Although we have made no effort to define this universe in a formal way, it can probably be taken as a first approximation to the universe to which most of us are perceptually exposed.

The successful demonstration, within any framework stipulated, of the ecological validity of a gestalt-factor does not automatically imply the legitimacy of its interpretation as a learned cue. It merely shows that an objective basis for probability learning is offered the individual within the framework chosen. Since, however, all ecological validities represent a challenge to the organism for utilization, and since probably many cues are actually being utilized roughly in proportion to the degree of their validity,<sup>18</sup> our findings lend plausibility-support to the *reinterpretation of*

<sup>18</sup> Brunswik, *op. cit.*, 1947, 41, 48 ff.



*proximity as a cue acquired by generalized probability learning.* If this should become possible for other gestalt factors also, they all could be seen as externally imposed upon, rather than as innately intrinsic to, the processes in the brain; they would then appear as functionally useful rather than as whimsically 'autochthonous.' It goes without saying that such an interpretation would lose much of its cogency if it would turn out that proximity has similar organizing effects in individuals, groups, or species in whose habitat or culture it has no (or opposite) ecological validity.

Actual utilization of the proximity factor on the part of the perceptual response system, although doubtless present in the above-mentioned type of artificial examples brought forth by the gestalt psychologists,<sup>19</sup> would seem to require further study by means of more representatively selected stimulus-situations. This is especially true when the problem is the relative weight given to this factor by the organism in comparison with the other factors of organization. 'Crazy worlds' in the sense of universes with artificially reversed validities could also be constructed to which animals could be exposed from birth on and their perceptions studied.

It should be noted that in the company of the other acknowledged gestalt factors the factor of proximity plays somewhat the rôle of a step-child. Proximity is brought in relation to 'association by contiguity,' for which it is said to furnish the perceptual underpinning allegedly indispensable in any learning; in turn, learning by space-time contiguity is notoriously minimized in gestalt psychology. Furthermore, and in line with this, proximity is seen as being at a disadvantage in the face of "parts which 'fit' each other, which jointly form a 'good curve.' [These] are more strongly unified than such as have no intrinsic relation and are linked by *mere* (*italics ours*) proximity. Such 'good continuation' distinguishes a meaningful text from a nonsense series; therefore the process corresponding to the apprehension of the meaningful material must be better organized than that corresponding to a nonsense series."<sup>20</sup> It is in part for reasons such as this that we intended to include a study of some of the more genuine

<sup>19</sup> These examples have in part been incorporated into experiments on animals, such as those on birds by M. Hertz, *Wahrnehmungspsychologische Untersuchungen am Eichelhäher*, *Zsch. f. vergl. Physiol.*, 7, 1928, 144-194; for discussion see Köhler, *op. cit.*, 142-149. In a second of two papers using rats, I. Krechovsky (An experimental investigation of the principle of proximity in the visual perception of the rat, *J. Exper. Psychol.*, 22, 1938, 497-523) has shown proximity to be effective, but only under certain motivational conditions. Concerning the purely cognitive aspects involved Koffka, *op. cit.*, 167, remarks, "How proximity and equality will work when no regular or simple pattern can be the result has not yet been investigated. In this, and in many other respects, our knowledge is still incomplete."

<sup>20</sup> Koffka, *op. cit.*, 569.



gestalt factors, such as symmetry and closure, in our ecological program. Since our pictures, well suited as they were for proximity, yielded only a most meager sample for those other factors; we must leave these additional problems for further investigation.

#### SUMMARY

In a selection of seven pictures of common perceptual situations, all the  $N = 892$  separations between discernible adjacent parallels were classified as to the degree of their 'proximity' (nearness, smallness of line-separation) in the picture. They were also classified as to mechanical object coherence in the geography depicted. Point-biserial correlations describing the 'ecological validity' of proximity (the 'proximal' stimulus-variable) as a potential indicator of mechanical coherence (the 'distal' variable) range from 0.37 to 0.18 for various dichotomous juxtapositions of mechanical units and more incidental separations such as passages or ornaments, but excluding most shadows. Since shadows tend to be still narrower than coherent objects ( $r$  about  $-0.2$ ), their inclusion lowers the validity coefficient to about 0.12; even this value is statistically significant in view of the largeness of the sample. High ecological validity is foretold, by limited and statistically not significant evidence ( $N = 21$ ), for the further factors of 'symmetry,' with and without 'closedness.' The frequently emphasized perceptual organization effect of proximity (leaving the other gestalt factors for further investigation) may thus well be rooted in a generalized probability learning of its over-all ecological validity in the natural-cultural universe to which we are habitually exposed, rather than in an 'autochthonous' gestalt dynamics of the brain field.

## LEARNING IN SIMPLE ONE-DIMENSIONAL TRACKING

By CHARLES W. SLACK, Princeton University

Tracking in one of its simplest forms consists of keeping a pencil on a segment of line, viewed through a slit, as the line varies its position con-

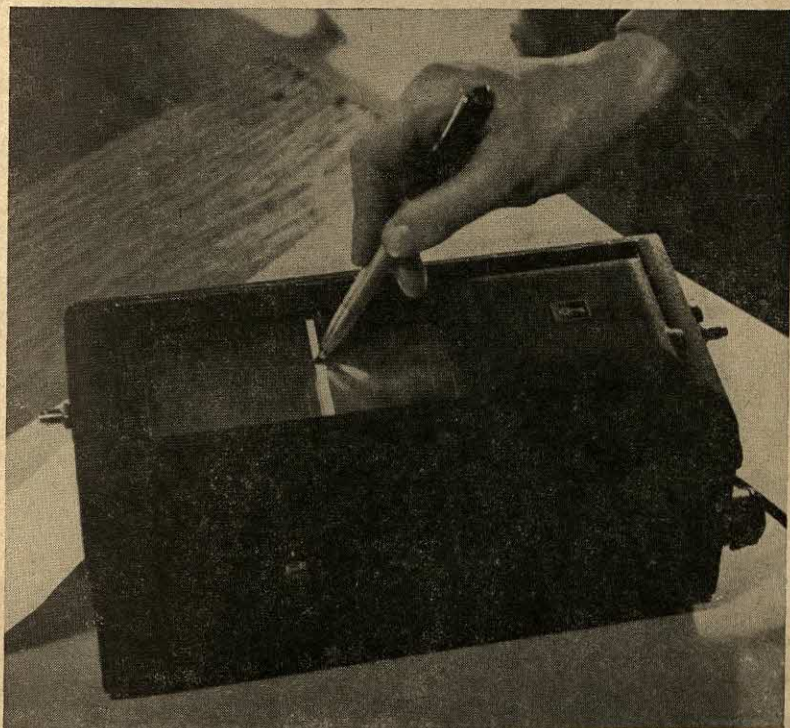


FIG. 1. THE TRACKING-BOX

Paper-speed is 4 in. per sec. The pen is equipped with a felt point.

tinuously or discontinuously (Fig. 1). An input-wave of desired frequency, amplitude, and complexity is presented on a strip of paper in such a manner that only a small segment is visible through the slit. The paper

\* Accepted for publication April 7, 1952. This study was one of several publications in preparation deriving from a larger project initiated by the Professional Division, Bureau of Medicine and Surgery, and subsidized by Office of Naval Research, contract N6onr27014.



moves behind the slit at a constant rate of speed and  $S$  is instructed: "Keep your pencil on the line, even when the line moves." It is possible with more complex apparatus to vary the ratio of hand-movement to display-movement, the load upon the hand, and the extremities used (singly or in combination) for tracking. We shall here consider only the simplest case in which the load upon the hand is constant and the control-ratio (ratio of hand-movement to display-movement) is unity. We shall deal

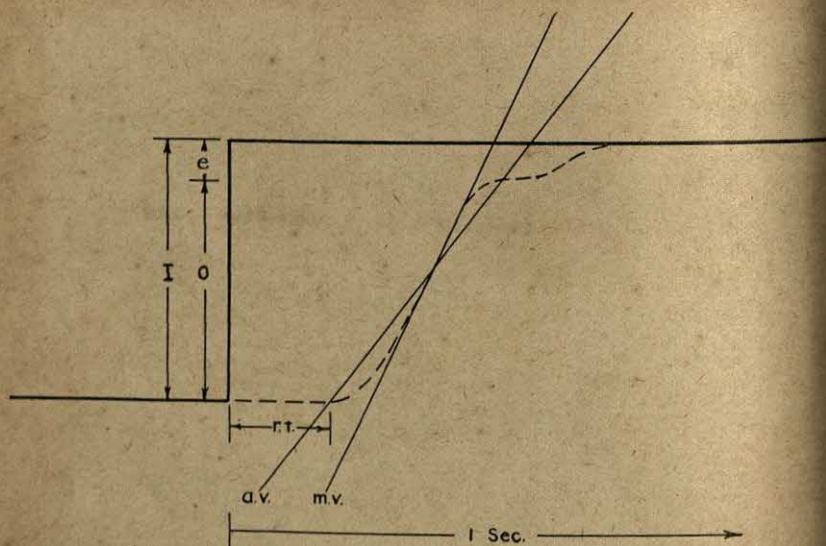


FIG. 2. DIAGRAM OF TYPICAL STEP-FUNCTION IN WHICH THE MEAN MAGNITUDE OF THE SERIES WAS LESS THAN THAT OF THE GIVEN STEP

Solid line = input

Dotted line = output

I = input magnitude

O = magnitude of initial response

r.t. = reaction-time

a.v. = average velocity

m.v. = maximal velocity

e = magnitude of error

only with step-function inputs, although many other types of input are feasible. Step-functions have the particular advantage of being easy to make and obvious in their components. Fig. 2 shows a typical step-function input and output (response). The step was presented as one of an unpredictable series.

Besides being an excellent approach to the study of motor behavior, the tracking experiment presents some interesting problems in learning. This paper describes some of the types of learning which occur in tracking. With apparatus such as is shown in Fig. 1, it is possible to study at

least four kinds of learning: (1) initial acquaintance with apparatus and task; (2) the range-effect or the establishment of a central tendency; (3) 'locking in' or the appearance of an assumption of regularity as evidenced by reductions in reaction-time; and (4) the assumption of regularity without change in performance. Although these four kinds of learning are considered separately from the standpoint of various behavioral criteria, they may be regarded as similar in their underlying mechanisms.

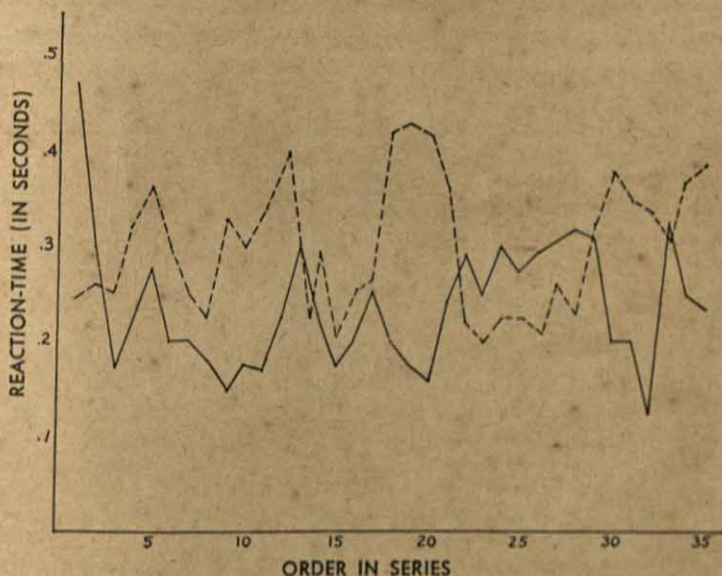


FIG. 3. ACQUAINTANCE-EFFECT AS INDICATED BY DROP IN REACTION-TIME FOR FIRST FEW RESPONSES

One S (solid line) shows the effect; another S (dotted line) does not.  
Among 10 normal Ss, 4 showed the effect.

(1) *Initial acquaintance with apparatus and task.* In many experimental situations some learning—that occurring before the actual experimental period—is not recorded since it does not appear in the measures used or, if recorded, is not expressed in the results. The initial acquaintance-effect may occur rapidly, as in the case of simple tracking, or it may extend into the learning period, as it does in complex tracking, *e.g.* driving a car. In simple tracking of step-function inputs, this kind of learning is readily observed in some Ss as an extra-long reaction-time, an extra-slow velocity in the initial responses, or both. It does not appear in all Ss nor is it likely



to appear in Ss who have recently served in such experiments. It is, moreover, typical only of the first few responses. Ss quickly settle down to a constant reaction-time and to a velocity which is independent of time of presentation or absolute position in the series. The curves of one normal adult who showed this *acquaintance-effect* in reaction-time and one who did not are given in Fig. 3. The curves of one feebleminded and one normal child who showed the effect in their average velocity to a 1-in. step are given in Fig. 4.

(2) *The range-effect.* As mentioned above, velocity is independent of time of presentation and of absolute position within the series presented.

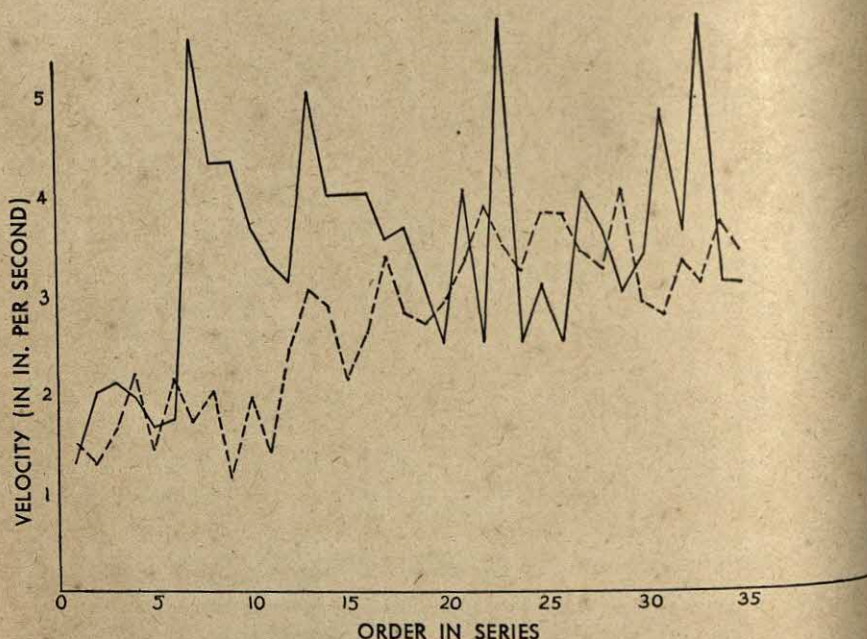


FIG. 4. ACQUAINTANCE-EFFECT AS INDICATED BY A RAPID RISE IN THE AVERAGE VELOCITY TO A STEP OF 1 IN.

Solid line, the results of a normal S; dotted line, of a feebleminded S.  
Among 10 normal Ss, 6 showed the effect.

It is, however, dependent upon the size of the step to which the response is made. A relationship, close to linear, exists between step-size and velocity (maximal and average).<sup>1</sup> This has been termed *linearity* in light of

<sup>1</sup>L. V. Searle and F. V. Taylor, Studies of tracking behavior: I. Rate and time characteristics of simple corrective movements, *J. Exper. Psychol.*, 38, 1948, 615-631.

techniques of operational analysis of transmission system and it represents one of the ways the human operator approximates a linear servo-mechanism.<sup>2</sup> Operational analysis is a technique used in engineering by the means of which a linear, continuous servo-mechanism may be described mathematically and a prediction of the response of the system to a complex input may be made on the basis of its response to the components of that input. It is evident from the studies of Searle and Taylor as well as others, however, that the human operator departs from true linearity with larger steps and that the time it takes him to make all the steps is not constant.<sup>3</sup> It should be clear, in connection with servo-analysis, that any learning which appears as a change in the velocity or duration or magnitude of responses over time or repetition, is itself a departure from linearity. This holds for the four kinds of learning mentioned here as well as for learning which occurs as a result of changing the display-to-hand ratio or altering the load.<sup>4</sup>

Velocity is, then, dependent upon the size of the given step. In a similar way, the magnitude of the initial response (before correction) is dependent upon the size of the given step. Initial magnitude, however, is dependent also upon other things. It has been demonstrated clearly to be dependent upon the size of the preceding steps. This has been called the *range-effect* and is a tendency to overshoot the smaller steps and undershoot the larger. The mean step, at least with grouped data, tends to be neither undershot nor overshoot. If the mean step is changed, the range-effect also shifts to the new mean, indicating that the effect is one of learning of the central tendency and not the result of absolute step-magnitudes.<sup>5</sup>

A second factor upon which the initial magnitude of response depends may be an individual tendency for under- or over-shooting which was obscured in the grouped data of Craig<sup>6</sup> and others.<sup>7</sup>

The nature of this individual tendency can be discovered in the following experiment. A series of seven steps of random size, duration, and direction were each

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<sup>2</sup> D. G. Ellson and D. Gilbarg, The application of operational analysis to human motor behavior, *A.A.F., Air Materiel Command, Wright Field*, 1948, Serial No. MCREXD-694-2S.

<sup>3</sup> Searle and Taylor, *op. cit.*, *loc. cit.*

<sup>4</sup> W. E. Hick and J. A. V. Bates, The Human Operator of Control Mechanisms, Ministry of Supply Permanent Records of Research and Development, Great Britain, Monog. No. 17-204.

<sup>5</sup> D. G. Ellson and Lawrence Wheeler, Jr., The range effect, *A.A.F., Air Materiel Command, Wright Field*, 1949, Technical Report No. 5813, Serial No. MCREXD-694-2P.

<sup>6</sup> D. R. Craig, Effect of amplitude range on duration of responses to step function displacement, *A.A.F., Air Materiel Command, Wright Field*, 1949, Technical Report No. 5913.

<sup>7</sup> Ellson and Gilbarg, *op. cit.*, *loc. cit.*



presented 11 times to a group of 10 Ss. In this manner the effect of S's past experience at each presentation of the  $i$ -step ( $i = 1$  to 7) is constant providing the contribution of previous experience is expressible as a mean or weighted mean of previous step-sizes. Having thus experimentally controlled the range-effect, the inter-individual variance is comparable to the intra-individual variance and we can see if the inter-individual variance is significant or if the slopes seem different for different

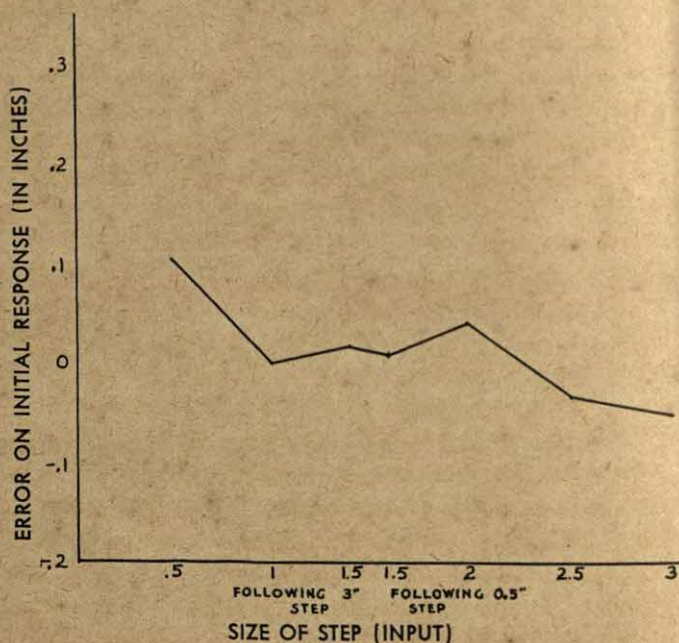


FIG. 5. THE RANGE-EFFECT

Order of steps in the series was: 1 in. left; 2 in. right; 0.5 in. left; 1.5 in. right; 2.5 in. right; 3 in. left; 1.5 in. right. In cases of true linearity, the curve would have zero slope.

Ss. If either of these conditions held they would indicate the operation of individual characteristic tendencies as a component of the response.<sup>8</sup>

This experiment was performed with 10 Ss—men, undergraduate students (see Fig. 5). The results indicate that individual differences are not a very important factor in the range-effect. The inter-individual variance, for all seven step-sizes (from 0.5 to 3 in.), is larger than the intra-individual variance, and the  $F$ -ratio, for six of the seven steps, is significant at the 1-% level (see Fig. 6). There is some indication that individual differences in the slopes of the error-curves might be found with a larger, less homogeneous population since the variance of the mean out-puts over all

<sup>8</sup> The author is greatly indebted to Mr. Robert Abelson for the design of this experiment.

steps for each individual is significantly larger than the variance of the group mean over all steps in 2 of the 10 cases. In one of the two cases, however, this is due to a low correlation between input and output.

Of course, it would be improvident to assume that individual differences do not exist because they were not demonstrated in this experiment. The value of the experiment, in view of the negative quality of the results, lies in the fact that it gives an indication of the magnitude of intra-individual variability, and that it provides us with a design for obtaining inter-individual differences in those instances where the search appears to be worth while.

It might be meaningful at this point to abstract from the individual's performance in the following way. *S* approaches the experiment and each step within the experi-

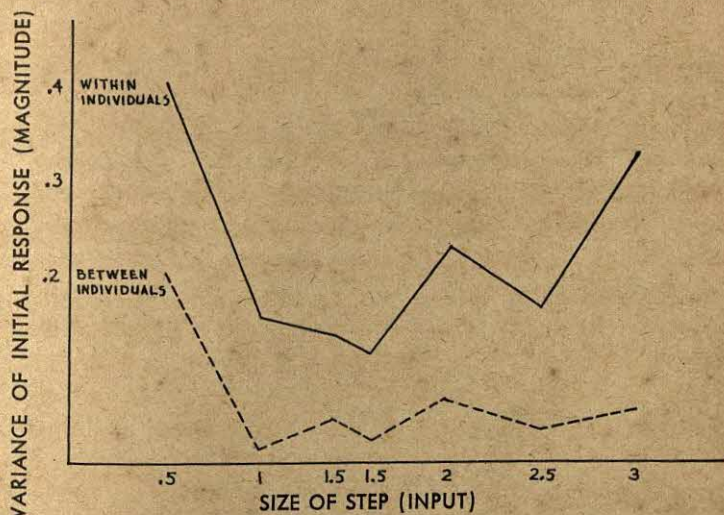


FIG. 6. COMPARISON OF THE INTER- AND INTRA-VARIANCE AMONG 10 Ss

ment with a set of expectancies resulting from previous experience. These affect his measurable performance in at least two respects: (1) his expectancies regarding the nature of the task and of his own abilities are corrected during the acquaintance-period as demonstrated by the reduction of velocity and reaction-time in the very first few responses; and (2) his expectancies regarding the size of upcoming-steps are regulated in part by his experience of the size of previous steps and in part, perhaps, by some kind of characteristic tendency (as yet undiscovered) for under-shooting or overshooting, which characteristic tendency may in turn be the result of past experience. The consequence of this is that *S* expects each step to be one which is just about mean size and makes his response accordingly. In any case, if we accept the initial response of *S* as an indication of what he initially 'perceives' (as is frequently done in psychophysics) we are forced to admit that what he 'perceives' is, in an important measure, dependent upon what he has previously 'perceived.'



(3) 'Locking in.' When dealing with step-function inputs, one quite parsimonious analysis of the stimulus is possible. Each step can be described as having a direction (in this case to the right or left of  $S$ ), a duration (length of time between steps) and a magnitude (in the studies reported in this paper, the sizes ranged from 0.3 in.).<sup>9</sup> The simple response or output, on the other hand, (aside from being a function of the factors outlined above) can be described as having a reaction-time, a magnitude, a duration, a maximal and an average velocity, and a rate of acceleration.



FIG. 7. 'LOCKING IN' RESPONSE TO A SQUARE WAVE IN WHICH AN ATTEMPT WAS MADE TO KEEP 'A' EQUAL TO 'B'

This performance should be contrasted with the usual anticipatory response in which phase-error is only intermittently corrected and is often out of phase by a considerable amount.

Until now, this paper has been concerned only with those series of steps where the input variables of magnitude, duration, and direction have been kept random or at least unpredictable. It is in this case that  $S$  behaves most like a linear servo-mechanism since he is unable accurately to predict the 'where' and 'when' of future steps. Having considered some of the ways in which the operator departs from linearity in a random or unpredictable series it now becomes of interest to consider what happens when certain aspects of the input are held constant or predictable.

Let us consider first the case where duration and magnitude are held constant and direction is predictable (first right, then left). The input in this case is a regular square wave of constant amplitude. Under these circumstances the  $S$ s often 'lock in' as is indicated by a regular reduction of reaction-time to zero or negative. There seem to be at least two rather separate behavior patterns observable in  $S$ s who 'lock in.' One of these seems to be characterized by an attempt (at times reported as almost a struggle) to improve performance. The reaction-time is reduced to zero or negative depending, perhaps, upon which is the subjective criterion of a 'good' performance. If negative, then  $S$  may attempt to achieve just that amount of anticipation which will make him cross the step at the half way point (see Fig. 7). Further, it may be reported that an attempt was made to minimize the time it takes to make a response without increasing the overshoot and the time it takes actually to get to

<sup>9</sup> Dr. Hadley Cantril has suggested to the author that two other variables might be added; namely, location (where the step is located on the slit) and nature (in this paper only right angle steps are discussed but other types are obviously possible).



the new input position. Since one of the effects of linearity is a particular small range of velocities for each initial magnitude of response which cannot be greatly exceeded, velocity can only be increased if overshoot is increased, and, reciprocally, overshoot can only be reduced if velocity is reduced. The struggle is then, a fruitless one and if *S* realizes this, he may fall into a second type of behavior pattern, characterized as follows: faced with the same repeating square wave of constant amplitude, *S* may relax into moving back and forth in time with the stimulus, watching but not weighting the information from his eyes heavily so that any slight error in timing of the movement is cumulative over a number of steps. *S* may notice now and then that he has moved out of phase and correct for this but then the error occurs again and the cycle repeated.

(4) *Assumption of regularity without change in performance.* When presented with a regular square wave, *S* need not 'lock in.' Indeed, his performance may not in any observable way be different from that which he would give to the mean step in an unpredictable series. What is more, reaction-time does not necessarily reduce; *S*'s performance may be independent of time and it may appear that, since there is no change in performance, no learning has occurred. It can frequently be demonstrated, however, that learning has occurred without a change in performance. This is done in the following way.

An *S* is selected who does not 'lock in' (or, as we shall see, any *S* can be prevented from 'locking in') and is presented with a regular square wave a number of times (*e.g.* 39), at the end of which he is given a surprise-step differing from the previous steps in magnitude, or duration, or direction, or any combination of these. *S* may then act as if this surprise-step were just like the others he has experienced and make an error in magnitude, duration, or direction of the response.<sup>10</sup> The error is an indication that learning has occurred (Fig. 8).

If one of the three aspects of the stimulus is varied in an unpredictable manner, *S* is prevented from truly 'locking in' although he may anticipate. For example, if the duration and direction are held constant and the magnitude varied *S* may anticipate with a response which will probably be in error by a certain amount. *S*'s performance is no quicker nor more efficient if he anticipates under these conditions than if he waits until the stimulus moves, since a corrective response cannot be made until approximately a reaction-time after the step has occurred and since the time it takes to make a response is almost a constant independent of the magnitude. Moreover, by anticipating, he leaves himself even more susceptible when presented with a surprise-step to the kind of error described above. Here again, though, it is not necessary that *S* anticipate or show any other sign of change in performance in order for learning to occur as indicated by the surprise-error.

With two of the three aspects of the stimulus varied and only one held predictable the author has never obtained truly consistent anticipation, although learning again without change in performance, is possible and has been obtained.

<sup>10</sup> If the stimulus-duration is greater than usual, the resulting response may occur in the absence of a stimulus.



This fourth type of learning—assumption of regularity without change in performance—is a rather tricky and flimsy affair. Once *S* has become aware of it, he is not likely to be fooled by it again and may therefore be

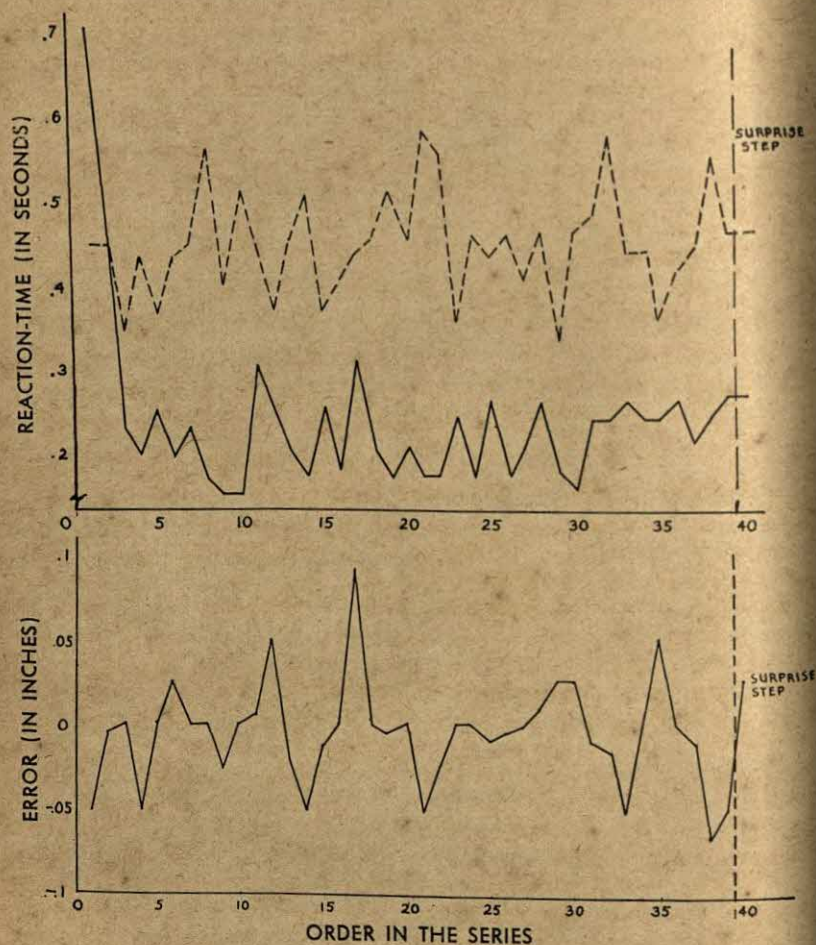


FIG. 8. GRAPHS DEMONSTRATING LEARNING WITHOUT A CHANGE IN ERROR, REACTION-TIME, NOR DURATION

The dotted line is response-duration; the two solid lines are reaction-time and error, respectively. The first 39 steps were made alternately left and right in steps of 1 in. and durations of 1.25 sec. The fortieth or surprise-step was the cessation of the stimulus. *S* gave a response to this step which was, in all measured characteristics, within the limits of errors of the other 39 steps. *S* had least learned three characteristics of the stimulus (direction, duration, and magnitude) without measurably changing his performance.

useless as an *S* in the future. As soon as the learning is allowed to show up in performance, it disappears.

This type of learning also illustrates some fundamental properties of all four kinds of learning presented in this paper; namely, their operational and psychological independence. One can easily see that in nearly all cases the experiment can be so arranged that one kind and not the others will show up. Thus, the kinds are almost operationally independent. Since no correlation between the kinds has been demonstrated to exist within individuals, it is probably best to assume that the kinds of learning are psychologically independent also; that is, that any particular *S* may show any combination of the four. It may be found to be otherwise but for the present it does not seem unreasonable to call them independent kinds of learning.

#### SUMMARY AND CONCLUSION

Four kinds of learning in simple one-dimensional tracking of step-function inputs are discussed along with experiments demonstrating properties of the various kinds.

(1) *The acquaintance-effect.* This effect occurs in some *Ss* as an extra-long reaction time and/or an extra-slow velocity in the initial response. The effect is diminished in a very few responses.

(2) *The range-effect.* A study was done to see if individual differences could be found in the range effect. Results were largely negative.

(3) *'Locking in.'* Two different behavior patterns were observed in *Ss* who 'locked in' (reduced reaction-time in a regular manner). One kind of performance includes a shift in the phase relationship of the stimulus and the response which is corrected only periodically, and the other kind does not exhibit such shift.

(4) *Assumption of regularity without change in performance.* It was discovered that if *Ss* did not 'lock in' or were prevented from 'locking in', that learning without change in performance measures used frequently occurs. An attempt was made to show that *S* gains no obvious advantage from learning in either the range-effect with unpredictable step sizes or in the assumption of regularity without change in performance.

It is obvious that much research must be done on learning in tracking before it can be stated with any surety that the facts of the situation are known. There is still much to be discovered regarding the departures from linearity. Each of the experiments described here gives rise to many questions as well as answering a few. If it is the intention of the experimenter



to arrive at a model or models of tracking performance, it is the personal belief of the author that certain things should be borne in mind:

(1) That if the human being is a servo-mechanism, it is in most instances a non-linear one and in some instances, at least, a discontinuous one.<sup>11</sup> As a matter of fact, the most important feature of the human operator may well be that it *learns*. If this is the case, our servo-models will only be as good as our learning-models.

(2) Just because in a particular experimental situation a human being acts like a servo-mechanism does not necessarily indicate the generality of the model. The human being is a very flexible thing in his behavior. He can be forced, under proper experimental conditions, to act like a great many things. If you tie a large enough weight to his arm so that muscular contractions are useless his arm will act quite like a pendulum. Similarly, perhaps, if you prevent him from anticipating accurately and discount his reaction-time, he will act like continuous linear servo, not quite, but pretty close.

(3) That learning models used cannot, most probably, be of the type which assumes either a necessary change in performance with learning or a necessary increase in efficiency in performance with learning. The learning which goes on in the simple one-dimensional tracking situation does not in these cases either increase the efficiency, speed, and accuracy or decrease the duration of the response. At present, the most accurate statement that can be made in regard to the functionality of much of the learning in tracking is that, like other types of non-linearity, it is a source of error.

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<sup>11</sup> W. E. Hick, Discontinuous functioning of the human operator in pursuit tasks, *Quart. J. Exper. Psychol.*, 1, 1948, 36-51.

## THE TRIGONOMETRIC RELATIONSHIP OF PRECISION AND ANGLE OF LINEAR PURSUIT-MOVEMENTS AS A FUNCTION OF AMOUNT OF PRACTICE

By W. J. BROGDEN, University of Wisconsin

Corrigan and Brogden have reported the results of a series of experiments on the effect of angle upon the precision of linear pursuit-movements of the right arm and hand.<sup>1</sup> Their data indicate that the relation between angle from the body at which the movement is made and the precision of movement may be given adequate mathematical expression by the following trigonometric equation:  $y = a + b \cos 2x + c \sin 2x$ , in which  $y$  = precision of right-arm movement in terms of group mean frequency of stylus contact,  $x$  = angle from the body at which the movement is made,<sup>2</sup>  $a$  = the constant that determines the base line of the curve (mean frequency of stylus contact for all angles), and  $b$  and  $c$  are the constants by means of which the amplitude ( $d$ ) and the phase angle ( $2e$ ) of the curve are determined [ $d = (b^2 + c^2)^{1/2}$ ;  $\cos 2e = c/d$ ].

The first three experiments differed from each other in that the  $S$ s were tested on different angles, with a total of seven angles involved in Experiments 1 and 2, and six angles in Experiment 3. These experiments were alike in that each  $S$  was given 10 trials on each angle followed by a second series of 10 trials on each angle. Different  $S$ s were given different sequences of angles, so that each angle was represented an equal number of times in each ordinal position. The initial analysis of these data was concerned with the effect of practice on the precision of performance. Group learning curves were constructed for each angle of each experiment by computing the mean group-precision of performance for each of the 20 trials. For each angle a practice-effect was noted and the difference in the mean performance of the last 10 trials (those of the second series) from that of the first 10 trials (first series) was reliable by the  $t$ -test at the 5-% level of confidence for all angles for each experiment. Examination of the learning curves showed that the maximal practice-effect took place during the first 10 trials and that precision in performance changed little during the 10 trials of the second session. Analysis of variance of the data

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<sup>1</sup> R. E. Corrigan and W. J. Brogden, The effect of angle upon precision of linear pursuit-movements, this JOURNAL, 61, 1948, 502-510; R. E. Corrigan and W. J. Brogden, The trigonometric relationship of precision and angle of linear pursuit-movements, this JOURNAL, 62, 1949, 90-98.

<sup>2</sup> Angle is designated by Cartesian coördinates.  $0^\circ$  represents the position in which the track is normal to the frontal plane of the body of  $S$ , and the arm movement is started with the stylus close to, and continued away from, the frontal plane of the body.



of the second session revealed no practice-effect between performance on the several angles tested in sequence, since in none of the three experiments was ordinal position of angle found to be a statistically significant variable. Analysis of the relation of angle and precision of movement was, therefore, confined to the data of the second series of trials. Experiment 4 made use of 24 angles, and 10 trials at each angle were obtained for both a first and second series as in the earlier experiments. Group learning curves were constructed for each angle as before and similar results were obtained. Because 24 angles were involved, a greater amount of practice occurred than in the earlier experiments. The performance of many Ss reached maximal precision for the apparatus (zero or near zero stylus-contacts) and the distribution of scores at the various angles for the second series was found to be skewed. An appropriate transformation, the logarithm (base  $e$ ) of the raw score  $+5$ , was applied to each score for all Ss in order to normalize the distributions. When analysis of variance of the transformed data was completed, ordinal position of angle was found to have a significant effect on precision of performance. A plot of mean precision of performance (transformed scores) as a function of ordinal position showed an increase in precision of performance to occur with increase in ordinal position. This effect of practice, more specifically a progressive positive transfer from angle to angle, does not affect the trigonometric relation of angle and precision of pursuit-movement, since each angle was represented in each ordinal position. One might expect, however, degree of practice to be an important parameter of the relation between angle and precision of pursuit-movement. If rate of acquisition varies as a function of angle, the relation between angle and precision of movement will vary as a function of practice. Also, with sufficient practice a maximal proficiency would be obtained for each angle. If maximal proficiency were equal for each angle, the relation between angle and precision of movement would then be a straight line, but the relation between angle and amount of practice to attain maximal proficiency of pursuit-movement might have a similar relation to that obtained previously between angle and precision of movement for a minimal amount of practice.

It is clear from the previous studies that there are at least two ways by which practice might affect the trigonometric relation of angle and precision of pursuit-movement: one by the amount of practice at each individual angle; and the other by the total number of angles at which practice is given. In the latter case, transfer from one angle to another is involved as well as a direct practice-effect in terms of the number of trials at each angle. The same experimental design could be used as in the earlier studies. Instead of using only two series of 10 trials at each of the angles, a large number of series would be used. This design would produce a confounding of transfer effects with those of direct practice. Individual differences, however, would be minimized, since every S would be tested at each of the angles selected for the experiment. If on the other hand, an experimental design is used in which different groups of Ss are given practice on different angles, only one angle for a given group, there is no possibility for transfer effects between angles to occur. The effect of practice upon the



relation between angle and precision of pursuit-movement would be obtained without contamination of a transfer effect between angles, even though the precision of measurement of the effect of practice on this relation will be affected by the individual differences between groups. The present experiment makes use of the latter design and involves eight groups of 5 Ss each, who received extended practice on linear pursuit-movement of the right arm with angle held constant for the duration of practice. Each group was given practice at a different angle, and these angles were 0, 30, 45, 60, 90, 120, 135, and 150°.

This experiment was planned also to provide answers to several additional questions. In the prior studies, the measure of precision used was the total number of contacts made by the stylus on each side of the track throughout its length. It is possible that the relationship obtained between angle and precision of movement depends upon an asymmetrical spatial distribution of the errors; change in the angle selects different axes of this distribution and, thus, merely reveals the asymmetry of the distribution. Obtaining separate counts of the errors for each side of the track should provide a test of this possibility, so separate records for each side of the track were recorded in the present experiment. It is also possible that errors at different angles occur with greater frequency in different parts of the track. To check on this, a polygraph record of contacts was obtained so that the spatial distribution of errors throughout the length of the track were obtained for each side. These additional measures provide means of testing the adequacy of the score of total error and for more precisely testing the relation between the precision of pursuit-movements and the angle at which the movements are made.

#### METHOD AND PROCEDURE

*Apparatus.* The apparatus used in the present experiment is the same as that previously reported.<sup>3</sup> It consists of a track formed by two brass plates resting on a piece of glass that S traverses with a metal-tipped stylus. Velocity of stylus-movement is controlled by instructions given S to match the rate of his movement to that of a small cylindrical target that travels beneath the glass plate of the track at a constant velocity of 3.0 cm. per sec. Control apparatus provides automatically for starting the target, stopping it at the end of the track, and returning it to the starting position when its direction is again reversed for the start of a new trial. The platform on which the track is mounted may be rotated about its center by means of a bearing in the vertical plane. The angle 0° is represented by the track normal to, and the target parallel to, the frontal plane of S. A disk attached to the bearing of the central vertical axis is notched every 15° to permit rapid and secure selec-

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<sup>3</sup> Corrigan and Brogden, *op. cit.*, 502 f. and 90 f.



tion of the appropriate angle. The stylus and each side of the track constitutes an open switch in parallel with the counting switch of a Potter Electronic Counter, Model 67, and an ink-writing signal marker on the polygraph. Each contact of the stylus with a track side is registered cumulatively on the counter for that side and on the paper tape of the polygraph.

*Subjects.* The Ss, 40 in number, were volunteers from an elementary course in psychology. The criteria of selection were that they be men and be right-handed.

*Procedure.* The initial step was to seat S in a tank-driver's seat and to fasten a harness about his trunk that kept the right shoulder in a relatively fixed position. The height of the seat and its closeness to the track-platform were so adjusted to body stature that S was both comfortable and able to reach the end of the track with the stylus when the right arm was fully extended. The standardized instructions already reported verbatim in a previous paper were modified only slightly to satisfy the conditions of the present experiment.<sup>4</sup> These modifications refer to the single angle at which the S practiced for the duration of the experiment and to the number and location of rest periods. Three practice trials were given with the track in the angular position for the group to which S had been assigned in random fashion through the use of a table of random numbers. When S had completed his practice trials and thoroughly understood the task, the experiment then began. Each individual trial lasted approximately 15 sec., and the interval between trials was approximately 5 sec. There was a rest period of 1 min. between each series of 10 trials. Every S was given a total of 160 trials. The duration of the experimental session was, therefore, on the order of 2 hr. for each S. Group I practiced the linear pursuit-movement at 0°; Group II at an angle of 30°; Group III at 45°; Group IV at 60°; Group V at 90°; Group VI at 120°; Group VII at 135°; and Group VIII at 150°. These particular angles were selected because they appear to be the critical ones in determining the trigonometric relationship previously obtained between precision and angle of linear pursuit-movements.

During the course of the experiment, many Ss achieved a precision of performance at very nearly a zero error-level. The distributions of scores were found to be so markedly skewed that the transformation applied to the scores in the previous experiment was applied to each score of the present experiment.<sup>5</sup> The logarithm (base *e*) was obtained for the sum of the raw score + 5 for each S on every trial. Measures were available on each trial for each side of the track separately, both by means of the Potter Counters and the polygraphic record. Three raw scores were obtained from the Potter Counters: number of contacts on the left side of the track, number of contacts on the right side of the track, and total number of contacts (sum of the scores for the two sides of the track). Similar scores were taken from the polygraph record. In addition, measures of errors for each side of the track and for both sides combined were obtained for each fifth of the linear distance of the track; thus, these three measures of precision were obtained for every fifth of the linear distance of the track. After the transformation had been made for each trial for every S and for each of the measures, group means were computed for each measure on each trial. Group mean performance was then plotted for each trial for every one of the several measures. These learning curves were so irregular that

<sup>4</sup> *Ibid.*, 502 f.

<sup>5</sup> *Ibid.*, 90 f.



means for successive blocks of 20 trials were computed for every measure for each group. Learning curves plotted from this data are more regular than those for the single trials. For each group, the individual curves for the left and right sides of the track and for the sum of the two sides of the track were plotted on the same graph. In all cases, the curves for the left and right sides of the track overlapped with each other and showed the same general form. The form of the curve for total contacts per trial was very much the same as those for the two sides of the track taken separately. The learning curves for these three measures, obtained for every fifth of the track length, did not differ significantly from each other. No evidence was obtained that the distribution of errors is asymmetrical for either side of the track or throughout its length. The result for each of these measures is essentially the same, except for differences in the absolute number of contacts produced by the fractionation of the data. Use of the total number of contacts made by the stylus with both sides of the track, therefore, provides as precise a measure of proficiency of linear pursuit-movement as do any of the other measures. The results, therefore, will be reported only in terms of the total error score for both sides of the track from the Potter Counters. This is the measure of precision that was used in the earlier experiments.

### RESULTS

An analysis of variance was completed on these data.<sup>6</sup> Mean precision of performance, in terms of the transformed score for total error per trial

TABLE I  
RESULTS FOR ANALYSIS OF VARIANCE

Source	Degrees of freedom	Sum of squares	Mean square	F	F at 5% level
(1) Angles (Groups)	7	7.8368	1.1195	2.33	2.33
(2) Blocks of practice trials	7	1.4071	0.2010	9.62	2.01
(3) Angles $\times$ blocks	49	0.0380	0.0008		
(4) Individual differences of Ss within angles (Groups)	32	15.3563	0.4799	23.07	1.46
(5) Error	224	4.6697	0.0208		

from the Potter Counters, for every S for each of the 8 blocks of 20 practice trials, was entered into the work table. The results of the analysis are summarized in Table I. Since individual differences are significant when tested against the error mean square, the mean square for individual differences must be used as the error-term in the  $F$  to test the significance of the mean square for angles. This latter  $F$  is significant at the 5% level of confidence. The mean square for blocks of practice trials is tested against the mean square for error and is significant at better than the 5% level.

<sup>6</sup> G. W. Snedecor, *Statistical Methods*, 4th ed., 1946, Chap. 11.



The mean square of the interaction between angles and blocks is so small that no *F*-test is necessary.

The results of analysis of variance demonstrate that both angles and practice (in blocks of 20 trials) have a significant effect upon the proficiency of linear pursuit-movements. A more precise analysis of these effects can be obtained by examination of the mean precision of performance as a function of angles and of practice. One way of viewing these relations is to plot the learning curves for each group (angles). This is done in Fig. 1. Examination of these curves shows an increase in precision of performance with practice for all groups except Group V and Group VI, the *Ss* of which received practice at the angles  $90^\circ$  and  $120^\circ$  respectively. There are differences between groups in the gains in proficiency produced by practice, as well as differences between groups in the level of precision through all stages of practice. For each group the differences between the mean group performance on the first 20 trials and trials 71 through 90, between trials 1 through 20 and 141 through 160, and between trials 71 through 90 and trials 141 through 160 were subjected to the *t*-test. Tests for evidence of learning between the initial and the middle stage of practice, between initial and terminal practice, and between the middle and terminal stages of practice are, thus, obtained. The differences between the initial and middle stages of practice and between the initial and terminal stages of practice were reliable at better than the 5-% level of confidence for all groups except Groups V ( $90^\circ$ ) and VI ( $120^\circ$ ). No reliable differences between the different stages of practice were obtained for Groups V or VI. The gains in precision of performance from the middle to the terminal stage of practice were much less than those between the initial and middle stages and between the initial and terminal stages of practice. Gains in precision of performance between the middle and terminal stages of practice were reliable at the 5-% level of confidence for Groups I ( $0^\circ$ ), II ( $30^\circ$ ), and IV ( $60^\circ$ ), and decrements for Groups III ( $45^\circ$ ) and VIII ( $150^\circ$ ) were reliable at the same level. No reliable differences were obtained for Groups V ( $90^\circ$ ), VI ( $120^\circ$ ), and VII ( $135^\circ$ ).

It is doubtful that 160 trials of practice produced maximal learning for Groups I, II, and IV, with the angles respectively of  $0^\circ$ ,  $30^\circ$ , and  $60^\circ$ . The learning curves for these groups appear still to be descending at the termination of practice. For Groups III, V, VI, VII, and VIII, however, 160 trials of practice are more than adequate for the attainment of maximal proficiency.<sup>10</sup> No evidence of learning was obtained for Group V at  $90^\circ$ , and little if any for Group VI at  $120^\circ$ . Groups III, VII, and VIII, with angles of  $45^\circ$ ,  $135^\circ$ , and  $150^\circ$  respectively, attained a maximal level of proficiency

in linear pursuit-movements after about 100 trials. In the case of Groups VII and VIII, the maximal proficiency obtained after 100 trials is nearly at the level of zero error, since zero error equals 1.70 in terms of the transformed error score.

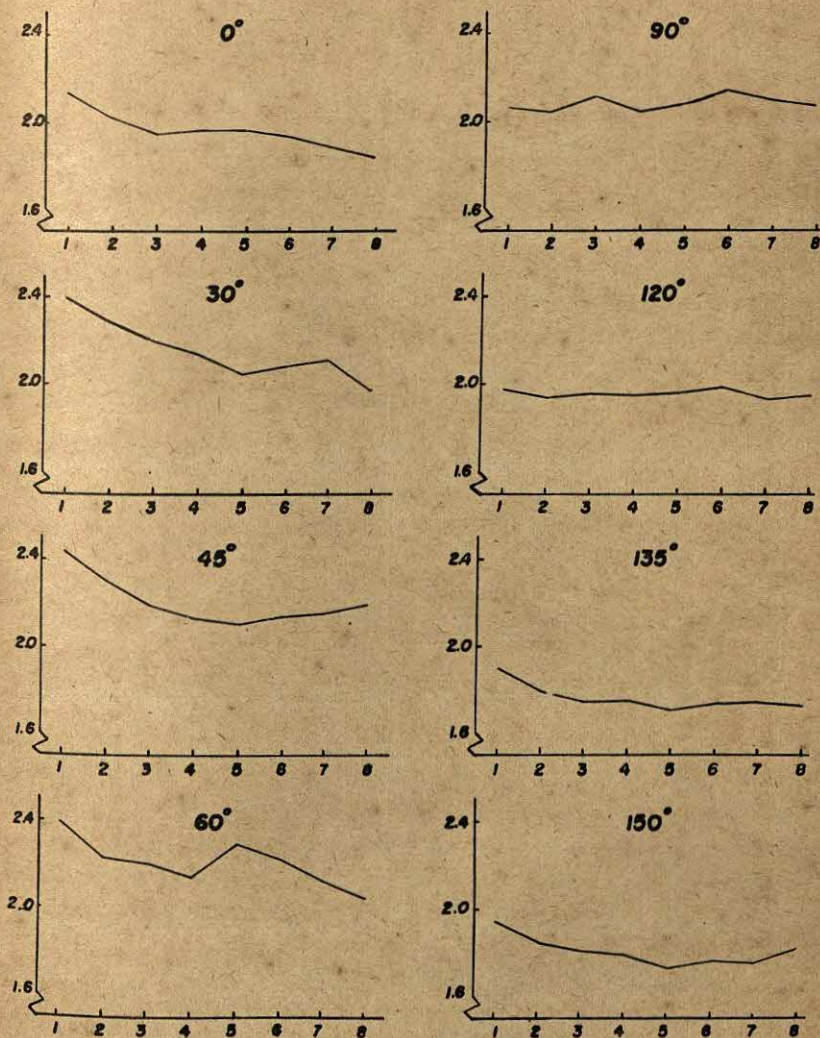


FIG. 1. LEARNING CURVES OF PRECISION OF CONSTANT VELOCITY PURSUIT-MOVEMENTS AT DIFFERENT ANGLES FROM THE BODY

For each curve, the ordinate is the logarithm (base  $e$ ) of the raw error score + 5 and the abscissa represents successive blocks of 20 practice trials. Every point is the group mean of the S's mean precision of performance for 20 trials. The angle at which each group practiced is shown in the figure.



There is another way by which the relation of angle and practice to precision of linear pursuit-movements can be presented. This is by fitting the trigonometric equation,  $y = a + b \cos 2x + c \sin 2x$ , to group means (angles) for each of the 8 blocks of 20 practice-trials. Figure 2 presents a

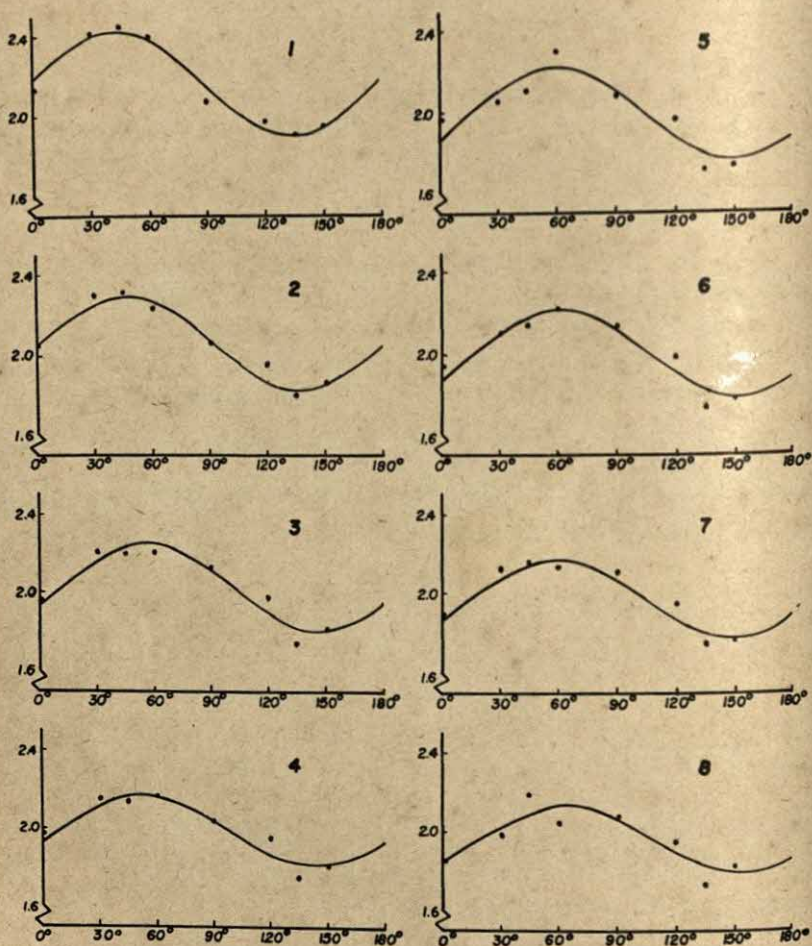


FIG. 2. CURVES OF REGRESSION EQUATIONS FOR PRECISION AND ANGLE OF PURSUIT-MOVEMENTS FOR SUCCESSIVE BLOCKS OF TWENTY PRACTICE-TRIALS

For each curve, the ordinate is the logarithm (base  $e$ ) of the raw score + 5 and the abscissa represents the angle at which performance was measured. Every point is the group mean of the  $S$ 's mean precision of performance for the 20 trial block indicated in the figure. A separate group of  $S$ s practiced on each of the different angles. The smooth curves are plots of the trigonometric regression equation,  $y = a + b \cos 2x + c \sin 2x$ , fitted to the empirical data by the method of least squares.

plot of these results, and Table II gives the constants of the regression equations fitted by the method of least squares to the empirical data. Examination of Fig. 2 and Table II indicates that the regression equations show progressive changes as a function of increasing amounts of practice. These changes appear to be an increase in overall precision of performance, a decrease in the amplitude of the curves, and a progressive increase in the

TABLE II  
CONSTANTS OF REGRESSION EQUATIONS FITTED BY METHOD OF LEAST SQUARES  
TO DATA FOR EACH BLOCK OF 20 PRACTICE TRIALS

Blocks of practice	a	b	c	d	2c
1	2.16	0.0150	0.2587	0.2590	-3°
2	2.06	-0.0167	0.2287	0.2293	4°
3	2.03	-0.0867	0.1991	0.2172	23°
4	1.99	-0.0567	0.1675	0.1768	19°
5	1.99	-0.1167	0.1943	0.2266	31°
6	2.00	-0.1267	0.1790	0.2193	35°
7	1.97	-0.1017	0.1773	0.2043	30°
8	1.96	-0.1100	0.1318	0.1717	40°

TABLE III  
RESULTS OF ANALYSIS OF COVARIANCE

Source	Degrees of freedom	Sum of squares	Mean square	F	F at 5% level
(1) Deviations among regressions, a alone varying	7	0.1120	0.0160	3.87	2.25
(2) Deviations among regressions, b and c only varying	14	0.2206	0.0143	3.47	1.95
(3) Deviations among regressions	21	0.3326	0.0158	3.84	1.84
(4) Deviations within blocks (about block regression)	40	0.1652	0.0041		
(5) Deviations about mean regression	61	0.4978			

phase angle of the curves. An analysis of covariance was completed in order to determine whether the variation among regression equations for different stages of practice is significant statistically.<sup>7</sup> It was possible in this analysis to break down the sum of squares for deviations among regressions into deviations among regressions with the coefficient *a* alone varying and into deviations among regressions with the coefficients *b* and *c* only varying. The results of this analysis are shown in Table III. The mean square for deviations within blocks was used as the denominator in the *F*-ratios, shown in Column 4 of Table III. All three *F*s are significant at

<sup>7</sup> *Ibid.*, Chap. 13.



better than the 5-% level of confidence. There are significant differences between the regression equations for the different blocks of practice, between coefficient  $a$  of these equations and between coefficients  $b$  and  $c$  of these equations.

### DISCUSSION

The results of the present experiment indicate clearly that the trigonometric relation between precision and angle of linear pursuit-movement is altered as a function of practice. Significant alteration of the regression equations occurs in terms of coefficient  $a$  (mean frequency of error) and coefficients  $b$  and  $c$  (constants in the equation by means of which the amplitude ( $d$ ) and the phase angle ( $2e$ ) are determined). From Table II, it is evident that there is a progressive decrease in the magnitude of  $a$  as practice increases, but that the decrease is irregular and there is one inversion. Coefficient  $b$  shows a progressive decrease, irregular in magnitude and there are two inversions. Coefficient  $c$  shows a progressive decrease, irregular in magnitude, and there is one inversion. Changes in these two coefficients are not directly meaningful, but the changes in the amplitude  $d$  and the phase angle  $2e$ , each derived from  $b$  and  $c$ , are meaningful. The amplitude  $d$  decreases in magnitude progressively through the first four blocks of practice-trials, shows a sharp increase between the fourth and fifth blocks of practice, and then decreases progressively through the remaining blocks of practice. The phase angle  $2e$  shows a progressive increase in size with increase in practice. This change is irregular in magnitude and there are two inversions. The shift in phase angle indicates that the relative difficulty of angle shifts as a function of practice. Early in practice the most difficult angle is of the order of  $45^\circ$  and the easiest angle is on the order of  $135^\circ$ . As practice increases, the size of the most difficult and the easiest angle increases in magnitude, until late in practice it is on the order of  $70^\circ$  and  $160^\circ$  respectively. The relative difficulty between other angles also changes.

It is clear that effect of practice on the trigonometric relation between angle and precision of linear pursuit-movements is complex. The base line, the amplitude, and the phase angle of the curve are all affected, but no precise quantification of these changes is possible from the data of the present experiment. The irregularity in the changes of the coefficient of the fitted regression equations together with the fact that the coefficients are derived from least square fits to empirical data expressed as group means of the individual means of blocks of 20 practice-trials, makes it unwise to attempt quantitative expression. An increase in the size of  $N$  and an increase in the number of practice trials would undoubtedly provide more reliable data. Whether it would be possible to give quantitative expression



to practice as a parameter in the trigonometric regression equation is unknown, but in any case the relation of practice to precision and angle of performance would be a complex function.

It is interesting to consider the relative importance of practice and of anatomical structure in the trigonometric relation of precision and angle of linear pursuit-movements of the arm. Initially, that is with little practice and with the positive transfer that comes from the experience of Ss in using the arm to manipulate stylus-like objects, the trigonometric relation is symmetrical. The plot of the regression equation is quite similar to plots of equations that represent the motion of objects over time (*e.g.* the motion of a pendulum or the vibrating motion of objects). At this stage of practice, one might suppose that the anatomical characteristics of the arm and shoulder are primarily responsible for the obtained relation. As practice increases, the difficulty of performance at different angles is changed, partly in terms of a general improvement in performance, partly in terms of a decrease in the degree of difficulty between the most difficult and the easiest angle, but also in terms of change in the relative difficulty of angles. With increasing practice, the most difficult angle becomes a less difficult angle, and the angle that is initially the easiest becomes relatively more difficult. Practice has overcome some of the limitations imposed on the proficiency of performance by anatomical characteristics. Maximal proficiency in terms of absolute measurement by the apparatus (zero error score) is, however, approached only at two angles,  $135^{\circ}$  and  $150^{\circ}$ . It appears doubtful that practice can eliminate completely the limitation imposed upon performance by anatomical characteristics. At some angles further improvement in performance appears possible, but at others maximal proficiency appears to have been obtained. It is unlikely, therefore, that a plot of precision versus angle will ever become a straight line, no matter what the amount of practice.

In addition to a demonstration that practice does have a significant effect on the trigonometric relation of precision and angle of linear pursuit-movements, the present experiment also provides important information about the measurement of precision of linear pursuit-movements. Since no significant differences were found between measures of precision in terms of side of the track or in terms of linear distance throughout the track, total error score (both sides of the track throughout the whole length of the track) is as satisfactory as any other measure. Thus, no support was found for the hypothesis that the relation between precision and angle of movement depends upon an asymmetrical spatial distribution of errors, or for the hypothesis that at different angles, errors occur with greater frequency in certain parts of the track.

#### SUMMARY

The present experiment was designed to provide information about the effect of practice upon the trigonometric relation of precision and angle of linear pursuit-movements. It was designed also to determine whether errors



within the track are distributed asymmetrically between the two sides of the track or throughout the linear distance of the track. Eight groups of 5 Ss each practiced linear pursuit-movements for 160 trials at one of the following angles: 0, 30, 45, 60, 90, 120, 135, and 150°. Group learning curves were constructed in terms of a transformed error score—logarithm (base  $e$ ) of raw score + 5—for total errors per trial, for total errors per trial on the left side of the track, for total errors per trial for the right side of the track, and for total errors per trial for each fifth of linear distance of the track for both sides and for each side of the track separately. No significant differences were found between these different measures except those of absolute magnitude brought about by fractionation of the data. Therefore, all further analyses of the data were made in terms of the total error score.

Learning curves were constructed and practice was found to have a differential effect for angles. No learning was obtained at the angles of 90° and 120°. Evidence of learning was obtained for the other angles, but there were marked differences in the amount of learning and the terminal levels of performance. The trigonometric equation,  $y = a + b \cos 2x + c \sin 2x$ , was fitted by the method of least squares to the group means for successive blocks of 20 trials of practice. Early in practice, the regression equation was found to be symmetrical and of fairly large amplitude. With an increase in practice, asymmetry increased with an increase in the size of the phase angle  $2e$ , and the base line decreased as did also the amplitude. Although these changes were significant statistically, they were not regular in progression. The effect of practice on the trigonometric relation of precision and angle of linear pursuit-movements is complex. Because of this complexity and because of the lack of precise quantification of the effect of practice, it was not possible to introduce a mathematical expression of practice as a parameter in the trigonometric equation that describes the relation of precision and angle of linear pursuit-movements.



## THE EFFECT OF RANDOM AND ALTERNATING PARTIAL REINFORCEMENT ON RESISTANCE TO EXTINCTION IN THE RAT

By D. W. TYLER, E. C. WORTZ, and M. E. BITTERMAN,  
University of Texas

The greater resistance to extinction which follows partial as compared with consistent reinforcement may be understood in terms of what Mowrer and Jones have called the *discrimination hypothesis*.<sup>1</sup> On the assumption that rate of extinction is inversely related to the similarity between acquisition- and extinction-situations, and on the assumption that the two situations are more similar for partially reinforced than for consistently reinforced animals, it follows that extinction should be more rapid after consistent reinforcement. If this quite reasonable interpretation is accepted, there remains the question of how the similarity between conditions of acquisition and extinction should be assessed. Two solutions to this problem have been proposed, one based on the principle of *stimulus-generalization* (or stimulus-compounding) and a second based on the concept of *serial patterning*.

The principle of stimulus-generalization leads to a trial-by-trial analysis—it directs attention to the similarity between the stimulating conditions present on individual extinction-trials as compared with reinforced training trials. For consistently reinforced animals, the after-effects of previous reinforcement are assumed to be part of the stimulus-compound present on each training trial. After-effects of reinforcement are absent during the extinction series, and this change in stimulating conditions results in relatively rapid extinction. For partially reinforced animals, however, responses to a stimulus-compound which does not contain the afferent consequences of reinforcement are frequently rewarded during training, and these animals are, therefore, expected to extinguish less rapidly.<sup>2</sup> Evidence supporting this interpretation has been provided by Sheffield, who found no difference in the extinction of partially and consistently reinforced groups of rats under conditions of spaced training—a result which follows from the assumption that the after-effects

\* Accepted for publication March 28, 1952.

<sup>1</sup>O. H. Mowrer and Helen Jones, Habit strength as a function of the pattern of reinforcement, *J. Exper. Psychol.*, 35, 1945, 293-311.

<sup>2</sup>The stimulus-compound present on trials following non-reinforcement may differ, not only in that after-effects of reinforcement are absent, but also in that certain after-effects of non-reinforcement (e.g. interoceptive stimuli resulting from frustration-responses) are present. Since the same consequences follow from both differences, exposition in terms of after-effects of reinforcement alone achieves simplicity without doing violence to the theory.



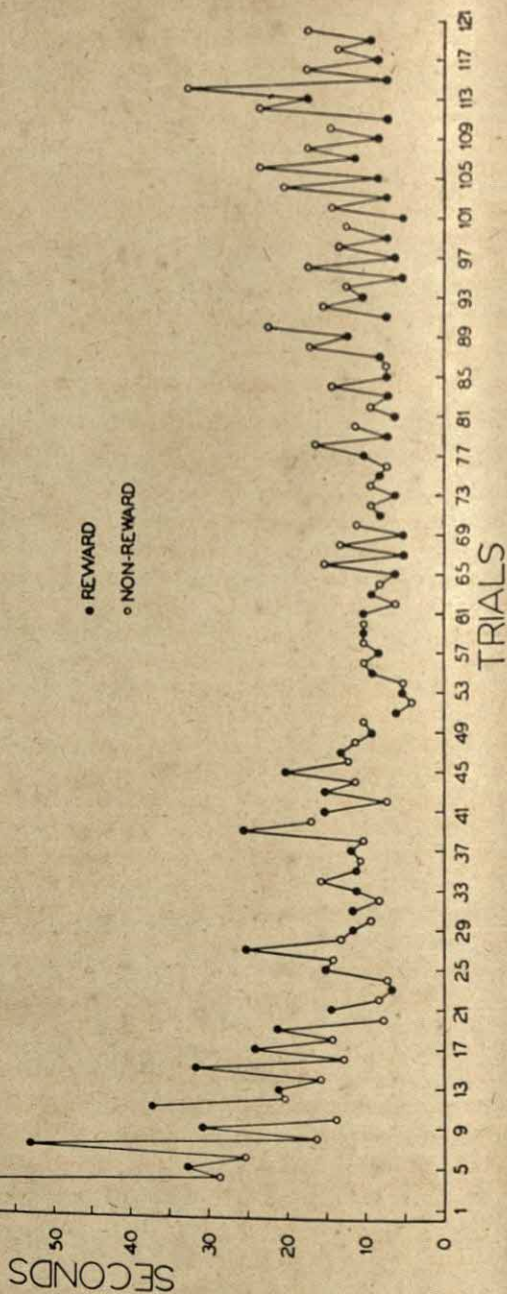


FIG. 1. THE DEVELOPMENT OF PATTERNING IN THE ALTERNATE GROUP  
Median running-time is plotted for each trial of the training series.

experiment, to maximize carry-over of the effects of reinforcement.) On each trial the time between the elevation of the door of the starting box and the jump from the end of the runway was measured with a stopwatch. If on any training trial the animal had not reached the goal-box in a period of 180 sec., it was guided in the direction of the goal and manually encouraged to jump from the end of the runway.

All animals were reinforced (10 sec. of feeding with wet mash) on 5 of the 10 trials given on each training day. The *alternate group* ( $N = 13$ ) was reinforced on odd-number trials—reinforcement and non-reinforcement were regularly alternated. For the *random group* ( $N = 12$ ) reinforcements and non-reinforcements were administered in accordance with the following four Gellerman orders:<sup>9</sup> RRRNNRRN, RNNRRNNRRN, RNNRRNNRRN, and RNNRRNNRRN. An examination of these orders shows that 60% of the reinforced trials followed non-reinforcement (including reinforced initial trials), while 40% of the reinforced trials followed reinforcement. For the alternate group, of course, all reinforced trials followed non-reinforcement, and responses following reinforcement were never reinforced. Each group was given 12 series of training trials, with an interval of 48 hr. between series.

*Extinction.* Extinction-trials also were given in series of 10, with a 48-hr. interval between series. As on unreinforced trials of the training series, no food was present in the goal-box in which the animals were confined for a period of 10 sec., and the 20-sec. interval between trials was spent in the waiting cage. Runs were timed as before, with one exception: if an animal did not reach the goal-box in 90 sec., it was removed from the runway and taken directly to the waiting cage for the usual 20-sec. period. The criterion of extinction was two successive incomplete trials of this kind.

## RESULTS

The course of learning in the alternate group is shown in Fig. 1 which is a plot of median time per trial for the entire training series. The development of patterning can be seen clearly. Early in the series the animals tended to run faster on the unreinforced trials (which in every case followed reinforced trials). In the intermediate stage of training of difference in running speed on trials following reinforcement and trials following non-reinforcement disappeared. Finally, the initial difference was reversed—the animals ran more rapidly after non-reinforcement than after reinforcement.

In Fig. 2, the two groups are compared with respect to differences in speed of running on trials following reinforcement and on trials following non-reinforcement. For each animal in each group the sum of running times for the five trials following reinforcement was subtracted from the sum of running times for the five trials following non-reinforcement on

<sup>9</sup> L. W. Gellerman, Chance order for alternating stimuli in visual discrimination experiments, *J. Genet. Psychol.*, 42, 1933, 356-360.



each day of training. Mean difference-scores for each group are plotted in Fig. 2. Both curves are positive at first, showing more rapid running after reinforcement, and then fall to zero. The curve for the random group remains at the zero level, while the curve for the alternating group becomes reliably negative. On the final day of training, for example, the mean of the random group does not differ significantly from zero, while the difference from zero of the mean for the alternating group is significant well beyond the 1-% level (Wilcoxon's non-parametric test for paired replicates).<sup>10</sup> These results are precisely what are expected in terms of an

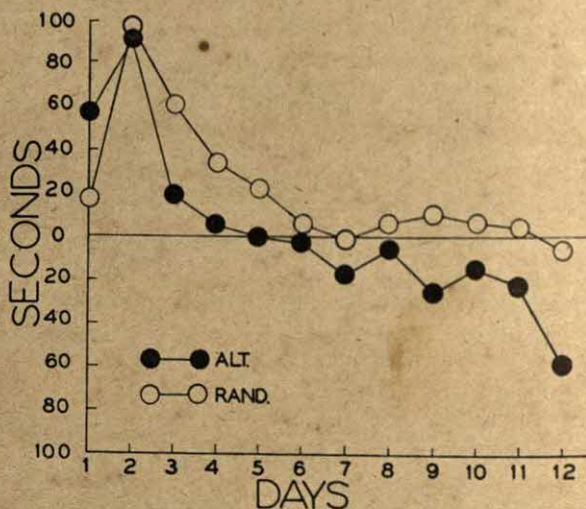


FIG. 2. MEDIAN DIFFERENCES IN RUNNING-TIME FOLLOWING REINFORCEMENT AND NON-REINFORCEMENT

The measure employed was the sum of running-times for the five trials following reinforcement minus the sum of running-times for the five trials following non-reinforcement on each day of training.

analysis of after-effects of reinforcement. In the random group, responses following non-reinforcement (to a stimulus-compound which does not contain after-effects of reinforcement) are not rewarded very much more often than responses following reinforcement (to a stimulus-compound which does contain after-effects of reinforcement). For this reason, runs to the two compounds should be equally rapid. In the alternating group,

<sup>10</sup> Frank Wilcoxon, *Some Rapid Approximate Statistical Procedures*, American Cyanamid Co., Stamford, Conn., 1949, 1-16.

responses in the presence of after-effects are never reinforced and responses in the absence of after-effects are always reinforced. A significant difference in running times should, therefore, appear. If the delay in extinction which follows partial reinforcement is explained in these terms, however, the alternating group should be expected to extinguish less rapidly than the random.

In Fig. 3, the course of learning and extinction in both groups is shown

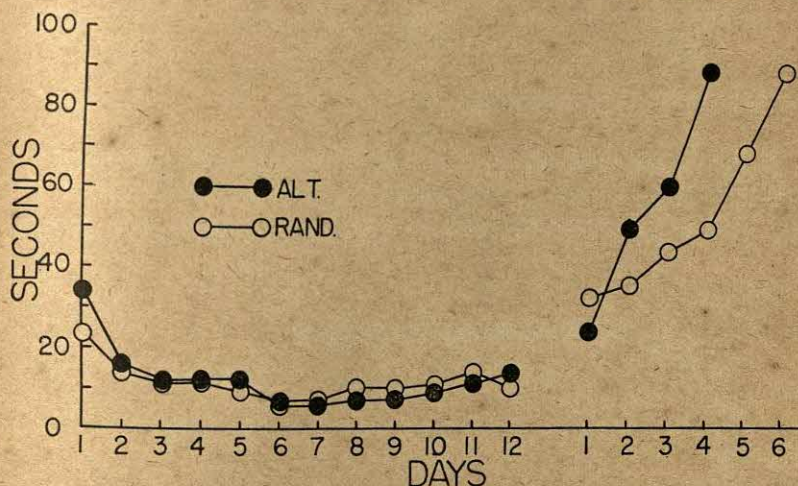


FIG. 3. MEDIAN RUNNING-TIMES DURING TRAINING AND EXTINCTION

The measure employed was the median of the ten running-times on each day. When each animal reached the criterion of extinction, it was assigned time-scores of 90 sec. for each subsequent 'trial.'

in terms of median running times. For every animal the median of the 10 time-scores for each day was computed and the median of these medians plotted for the two groups. After each animal had met the criterion of extinction, it was assigned a score of 90 sec. for each subsequent trial of the seven-day extinction-series. The extinction-curves reach 90 sec. before the end of the seven-day period because more than half the animals in each group had extinguished before that time. The curves for the two groups are quite similar during the training period, but they diverge during extinction—the alternating group tending to extinguish more rapidly.

The performance of each animal during the training series was expressed in terms of the median of the 12 daily medians. Similarly, its performance during extinction was expressed as the median of the seven daily medians. Group medians for training and extinction are compared in Table I. The



two groups did not differ significantly during training, but during extinction the median running time for the alternating group was significantly higher than that for the random group (Festinger's test<sup>11</sup>).

TABLE 1  
MEDIAN RUNNING-TIMES IN TRAINING AND EXTINCTION

	Training	Extinction
Alternate	10.75	90.00
Random	10.63	50.25
Diff.	0.12	39.75*

\* Significant at the 5% level (Festinger's test.)

In Fig. 4, the course of learning and extinction for the two groups is plotted in terms of mean percentage of response-times below 90 sec. per day. (It will be remembered that the criterion of extinction was two suc-

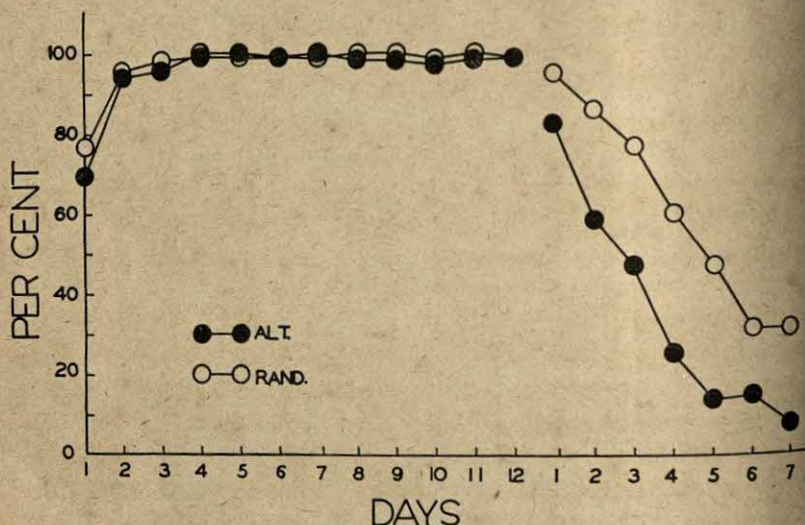


FIG. 4. MEAN PERCENTAGE OF RUNNING-TIMES GREATER THAN 90 SEC. DURING TRAINING AND EXTINCTION

cessive time-scores greater than 90 sec.) Again the curves are similar during training but diverge during extinction. In Table II, mean values for

<sup>11</sup> Leon Festinger, The significance of difference between means without reference to the frequency distribution function, *Psychometrika*, 11, 1946, 97-105.

training and extinction series are given for both groups. No significant difference appears during training, but during extinction the mean frequency for the random group is significantly greater than that for the alternating group (Festinger's test).

In terms of both measures, then—median running time and mean frequency of response-times below 90 sec.—extinction was found to be more rapid in the alternating group than in the random. The results are in

TABLE II  
MEAN PERCENT OF RUNNING-TIMES BELOW 90 SEC.  
IN TRAINING AND EXTINCTION

	Training	Extinction
Alternate	95.96	35.62
Random	96.87	61.31
Diff.	0.91	25.69*

\* Significant beyond the 5% level (Festinger's test).

agreement with those obtained by Longenecker, Krauskopf, and Bitterman for the conditioned galvanic skin response to shock in human subjects.<sup>12</sup> Although in both experiments that kind of patterning developed in training which should have been expected in terms of stimulus-generalization to lead to less rapid extinction in the alternating groups, the differences obtained were in the opposite direction. These results provide support for the concept of serial patterning (which suggests that similarity between conditions of training and extinction must be evaluated in terms of sequences of events), on the reasonable assumption that there was greater disparity between training- and extinction-sequences for the alternating than for the random group. Further tests of this interpretation would seem to require the use of a method for the independent evaluation of sequential similarity.

#### SUMMARY

Two groups of rats were trained on a runway under conditions of partial (50%) reinforcement. For one group (random) reinforced and non-reinforced trials were given according to a haphazard order, while for the second group (alternating) odd-numbered trials were reinforced and even-numbered trials non-reinforced. Significantly greater resistance to extinction was found in the random group. The results are opposed to predictions based on the concept of stimulus-generalization and support the conception of serial patterning.

<sup>12</sup> *Op. cit.*, 580-587.



## FIGURAL AFTER-EFFECTS UTILIZING APPARENT MOVEMENT AS INSPECTION-FIGURE

By R. J. CHRISTMAN, West Virginia University

According to the field theory proposed by Köhler and Wallach, any figure process sets up stresses in the brain which tend to prevent the reoccurrence of subsequent figures in the same location.<sup>1</sup> Thus, for example, with prior satiation by the rectangle in Inspection-Figure I-A in Fig. 1, stresses are set up which prevent the right hand square of the Test-Pattern, shown in Fig. 1 from being seen in its true physical position. The right hand square appears displaced downward and is seen lower in space than would be the case had satiation not previously occurred. According to Köhler and Wallach the mechanism behind this stress-distortion is probably an electrotonic effect in the neural substrate, induced by figure currents acting in a volume electrolyte, presumably in the visual cortex, or possibly some other visual center.<sup>2</sup>

If the cortical projection of a static figure induces an electrotonic effect, as Köhler and Wallach suggest, then a moving figure should also produce electrotonus. Furthermore, if apparent movement results from a short-circuiting of the underlying brain process, as Wertheimer suggested,<sup>3</sup> then the pathway taken by the short-circuit should undergo an electrotonus perhaps greater than that induced by the usual gradient of a figure contour, and equal to that induced by a physically moving object. This is to say, we should expect a greater figural after-effect if satiation were produced by apparent movement, than if it were produced by a motionless inspection-figure.

It was proposed to measure the amount of figural after-effect using the conventional Inspection-Figure (I-A in Fig. 1) and to compare these results with those obtained when using Inspection-Figure I-B in Fig. 1 in which the I-object was produced by apparent movement. On the basis of the Köhler-Wallach theory we should expect the following results.

(1) Satiation with Inspection-Figure I-A should produce a greater figural

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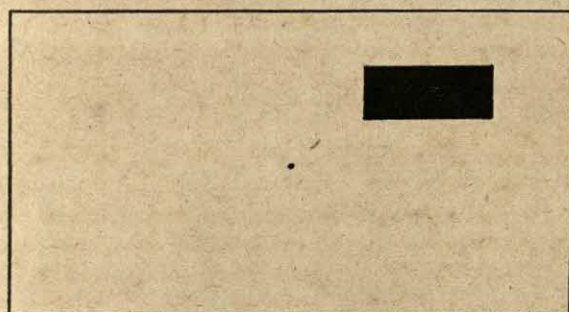
<sup>1</sup> Wolfgang Köhler and Hans Wallach, Figural after-effects, an investigation of visual processes, *Proc. Amer. Philos. Soc.*, 88, 1944, 269-357.

<sup>2</sup> Köhler and Wallach, *op. cit.*, 315-341.

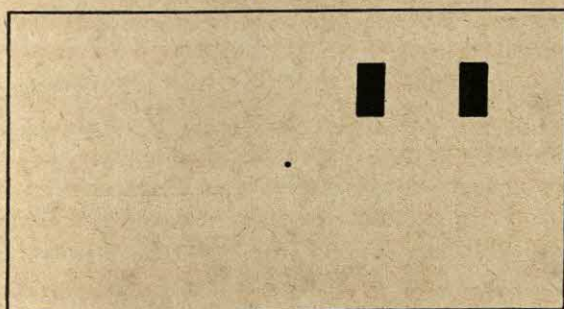
<sup>3</sup> Max Wertheimer, Experimentelle Studien über das Sehen von Bewegungen, *Zsch. f. Psychol.*, 61, 1912, 161-265.

after-effect (more judgments 'left high') than a control series with no inspection-figure present.

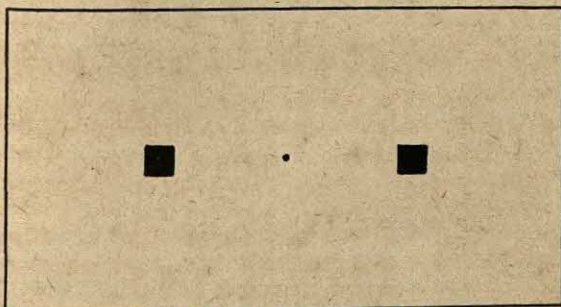
(2) Satiation with I-A should produce a greater figural after-effect than satiation with Inspection-Figure I-B. This follows because the area of I-A is greater than that of I-B.



I-A



I-B



TEST-PATTERN

FIG. 1. LANTERN SLIDES USED IN THE EXPERIMENTS  
All patterns used were actually the opposite of the above,  
being white on a black background.



(3) Inspection-Figure I-B in which the two parts of the figure appear to move back and forth (apparent movement) should produce more effect than either I-A or a motionless I-B.

(4) Satiation with I-B in which the alternation is too rapid for apparent movement, being seen only as two flickering bars, should produce less effect than when seen as apparent movement.

In the following pages are presented the results off an experiment designed to test these propositions.

#### METHOD AND PROCEDURE

The equipment consisted of a  $2 \times 2$  in. slide projector, a translucent screen, three slides (see Fig. 1), an episotister, a variable speed motor for producing apparent movement and flicker with I-B, a timing apparatus, and a signal by means of which *O* could indicate which square of the test-pattern appeared higher in space. *O* sat 3 ft. away from the translucent white screen and fixated a red dot painted on the center of the screen. The Inspection-Figures were about  $4^\circ$  from the line of regard and subtended a visual angle of  $3^\circ$ . Transmitted light was used for presentation of all patterns.

The five conditions of satiation used in the experiment were as follows.

Condition 1. Control series. *O* fixated red dot on bare screen for 1 min.

Condition 2. Satiation I-B. Conventional procedure for figural after-effect. *O* fixated red dot on screen for 1 min. while pattern was projected thereupon.

Condition 3. Satiation I-B. Same as Condition 2 except that the two vertical portions of I-B were alternated at a rate of about five complete cycles per sec. with a duration of 70 m.sec. for each bar. The phenomenal appearance was similar to that in Condition 2, but with the pattern appearing to flicker.

Condition 4. Satiation I-A. Conventional procedure for figural after-effect. *O* fixated center dot in I-A for 1 min.

Condition 5. Satiation I-B. Same as Conditions 2 and 3 except that the two vertical bars were alternated at a slower rate, about 66 cycles per min. with a duration of about 350 m.sec. and an interval of 100 m.sec. This rate produced excellent movement, all *O*s seeing a single vertical bar moving back and forth in a horizontal direction.

The duration of all fixations was 1 min. and in every case *O* fixated the central dot and not the figure projected upon the screen.

Immediately upon withdrawal of the Inspection-Figure, or at the end of 1 min. in Condition 1, *O* was presented the Test-Pattern for 1 sec., to which he indicated whether the left or right square appeared higher in space. The same Test-Pattern was used throughout, but the lantern slide was reversed for half the exposures. Counterbalancing was complete. Actually, a slight, unplanned assymetry was found in the Test-Pattern, causing one square to be called higher in space without satiation more often than the 50% expected by chance alone. By using an ABBA and BAAB order of presentation for the position of the slide the chance of getting an even number of left and right judgments was somewhat enhanced. Actually, with the control group, the percentage of judgments 'left high' and 'right high' corrected for



equals was 47.8% and 52.2% respectively, not significantly different from chance expected frequencies.

The exposure of the Test-Pattern was repeated 10 times after each satiation, the successive 1-sec. exposures being separated by intervals of about 4 sec. each. Thus, the entire 10 presentations required about 50 sec., while the first 5 following satiation fell within the 25-sec. period in which the figural after-effect is maximal.<sup>4</sup> Each of 75 *O*s had 4 satiations, each making 40 judgments.<sup>5</sup>

The naïve *O*s, students in elementary psychology courses, were given standardized written instructions which explained the procedure, encouraging them to make positive judgments wherever possible, and to avoid equal judgments unless absolutely necessary.

### RESULTS

Of the 3,000 comparisons, only 3.4% were judged equal. These 104 judgments were divided proportionately to facilitate statistical handling. The total number of judgments 'left high' was calculated and also the number for the first five judgments following satiation under each condition.

Fig. 2 shows the average number of judgments 'left high,' corrected for equals, both for total judgments and for the first five comparisons after each satiation. Under Condition 1 the fifteen *O*s made 287 judgments 'left high' out of 600 comparisons, thus averaging 19.1 for each *O*. This figure is not significantly different from the chance expected 20. Similarly, the first 5 judgments of these same *O*s averaged 9.3 with a chance expected frequency of 10. At the other extreme, under Condition 5, these fifteen *O*s averaged 27.9 judgments 'left high' out of their 40 comparisons, and 15.4 for their first five judgments after each of the four satiations.

The arrangement of Fig. 2 is such as to clearly indicate the increasing amount of the effect according to the original hypothesis. Apparent movement produces the greatest amount of effect, while Condition 4, utilizing the largest satiation-area and total amount of satiation, ranks second. There is little to choose between the motionless I-B (Condition 2) and that in which the alternation is too rapid for apparent movement. (Condition 3) It is also apparent from Fig. 2 that the effect is greater if only the first five judgments after satiation are used.<sup>6</sup>

<sup>4</sup> Elaine R. Hammer, Temporal factors in figural after-effects, this JOURNAL, 62, 1949, 337-354.

<sup>5</sup> In the final statistical analysis only the first 5 judgments after satiation were used, thus yielding 20 judgments per *O* instead of 40.

<sup>6</sup> If the first judgments after satiation only, had been used, the differences would have been even greater. The estimated values based on the few judgments available (60 per condition) would be: 18.7 for Condition 1, 26.0 for Condition 2, 25.3 for Condition 3, 26.7 for Condition 4, and 32.7 for Condition 5. The total *N* of 300 would have been much too small, however, to yield statistically reliable differences.



An analysis of variance showed a between conditions variance, significant beyond the 0.001 level of confidence. ( $F = 9.23$  with 4 and 70 d.f.) The  $t$ -ratios for all possible comparisons are shown in Table I. The de-

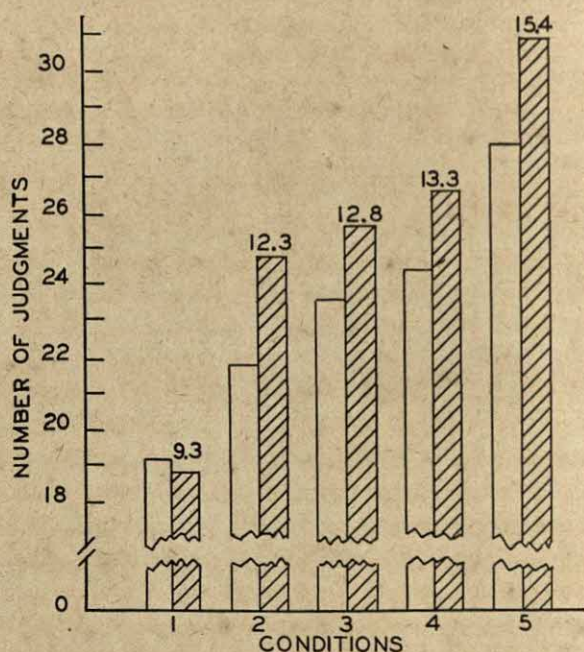


FIG. 2. AVERAGE NUMBER OF JUDGMENTS 'LEFT HIGH,'  
CORRECTED FOR EQUALS  
Shaded bars indicate first five judgments after satiation,  
drawn to same scale.

TABLE I  
THE  $t$ -RATIOS FOR THE DIFFERENCES FOR ALL POSSIBLE  
COMBINATIONS OF THE FIVE CONDITIONS

Conditions	1	2	3	4
5	5.98	3.03	2.55	2.06
4	3.92	.99	.49	
3	3.43	.49		
2	2.94			

nominator for all the  $t$ -ratios was based on the total within group variance according to a method suggested by McNemar.<sup>7</sup>

An examination of Table I suggests that all four conditions of satiation

<sup>7</sup>Quinn McNemar, *Psychological Statistics*, 245-246.

produce results different from those found under the control condition—all differences involving Condition 1 being significant beyond the 0.01 level of confidence. Thus, figural after-effects were produced under each of the conditions of satiation used. While all four of the primary hypotheses appear to be supported by Fig. 2, some of these differences are not statistically significant.

The second hypothesis, while seemingly true, is not established, since the *t*-ratio between Conditions 2 and 4 is only 0.99. In fact, all comparisons among Conditions 2, 3, and 4 show differences, which, while all in a direction which would be predicted, are not statistically significant.

With respect to the last two hypotheses, both seem to be supported by the data. Condition 5 produces a figural after-effect significantly greater than under Conditions 1, 2, 3, and 4 at levels of confidence of 0.01, 0.01, 0.02, and 0.05 respectively; certainly large enough to be highly suggestive if not entirely conclusive.

#### SUMMARY AND CONCLUSIONS

It has been demonstrated that satiation with a two-part figure, alternated to produce apparent movement, produces a greater figural after-effect than the same figure when motionless, or when alternated too rapidly for apparent movement. Furthermore, the apparent movement produces an effect greater than that produced by a solid figure of the same physical size as that represented by the apparent movement.

Köhler and Wallach,<sup>8</sup> Alexander,<sup>9</sup> Hammer,<sup>10</sup> and others have disclosed some of the factors influencing the amount of after-effect, including duration of satiation, brightness, contrast, and strength of figure. Condition 5 presents less reason to expect a large figural after-effect than any of the other conditions, except perhaps, Condition 3 which is equivalent to Condition 5 but lacking the apparent movement. The total time of stimulation for Condition 5 was much less than for Condition 2, due to the alternate blocking of the two halves of the stimulus-figure in order to produce apparent movement in the former. Compared to Condition 4, both the area and duration of satiation were considerably less favorable for producing a large figural after-effect under Condition 5. Yet, this is exactly what happened. Apparently the alternating of the two halves of the inspection-

<sup>8</sup> Köhler and Wallach, *op. cit.*, esp. 269-312.

<sup>9</sup> L. T. Alexander, The influence of figure-ground relationships on binocular rivalry, MA Thesis, Ohio State Univ. Library, 1948.

<sup>10</sup> Hammer, *op. cit.*, 337-354.



figure enhanced the effect in some manner. The averages of 12.3 for Condition 2 and 15.4 for Condition 5 ( $t = 3.30$ ) certainly point to something highly favorable in the condition involving apparent movement.

Furthermore, it is not simply *any* action in the pattern which produces the enhancement of the figural after-effect, since alternation too rapid for apparent movement was significantly inferior to apparent movement in producing the effect (12.8 to 15.4 with  $t = 2.55$ ).

The only reasonable conclusion is that apparent movement does produce an unusually large figural after-effect. If we can admit to at least the plausibility of the Köhler-Wallach explanation for figural after-effects and Wertheimer's short-circuit explanation for apparent movement, the experimental findings take on added significance.

At least one serious inconsistency still remains. Should not the rapid alteration also produce a short-circuiting, perhaps too rapid to be observed as apparent movement, but still capable of establishing electrotonus? The answer to this question is not immediately apparent. One possible explanation can be derived from the basic theory, however. Köhler and Wallach emphasize both an immediate polarization of cell surfaces and a more gradual change in polarizability as a function of satiation.<sup>11</sup> It is further stated that ordinary figural after-effects are primarily a result of the latter. It seems possible that the former may be involved in determining the optimal rate for apparent movement. Thus, with very rapid alternation, immediate polarization effects at the neural loci which correspond to the two halves of the stimulus-pattern might not be dissipated rapidly enough to permit the short-circuiting necessary for both perceived apparent movement and the subsequent changes in polarizability which allegedly result in figural displacement.

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<sup>11</sup> Köhler and Wallach, *op. cit.*, 329.

## THE DISTRIBUTION OF SCOTOPIC SENSITIVITY IN HUMAN VISION

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The purpose of the present investigation was to compare absolute scotopic thresholds in different portions of the retina. Although a number of studies deal with the relationship between threshold and distance from the fovea, the unpublished study of Sloan is the only one that has come to our attention which compares thresholds on more than two meridians.<sup>1</sup> From studies on single meridians it has been generally concluded that the scotopic threshold is lowest in the neighborhood of 20° from the fovea,<sup>2</sup> although when corrections for the decrease in effective pupillary area with increasing eccentricity of stimulation are made<sup>3</sup> this band of maximal sensitivity displays an increase in extent.<sup>4</sup>

The studies in which comparisons have been made between two or more meridians indicate that the sensitivity gradient varies with the meridians tested.<sup>5</sup> Individual differences in gradients and variations in size of test-stimulus from experiment to experiment preclude, however, the construction of a map of the distribution of sensitivity for the entire

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<sup>1</sup> L. L. Sloan, *Personal communication*.

<sup>2</sup> Sloan, Rate of dark adaptation and regional threshold gradient of the dark-adapted eye: Physiologic and clinical studies, *Amer. J. Ophthalmol.*, 30, 1947, 705-720; W. J. Crozier and A. H. Holway, Theory and measurement of visual mechanisms: I. A visual discriminometer; II. Threshold stimulus-intensity and retinal position, *J. Gen. Physiol.*, 22, 1939, 341-364; W. S. Stiles and B. H. Crawford, The effect of a glaring light source on extrafoveal vision, *Proc. Roy. Soc. London*, B122, 1937, 255-280; H. A. Wentworth, A quantitative study of achromatic and chromatic sensitivity from center to periphery of the visual field, *Psychol. Monog.*, 40, 1930, (No. 183), 1-192; Nathaniel Kleitman and Henri Piéron, Contribution à l'étude des facteurs régissant le taux de sommation des impressions lumineuses de surface inégale, *Année Psychol.*, 29, 1928, 57-91; M. H. Pirenne, *Vision and the Eye*, 1948, 45-47; Rods and cones and Thomas Young's theory of color vision, *Nature*, 154, 1944, 741.

<sup>3</sup> Sloan, The threshold gradients of the rods and cones: In the dark-adapted and partially light-adapted eye, *Amer. J. Ophthalmol.*, 33, 1950, 1077-1089; K. H. Spring and Stiles, Apparent shape and size of the pupil viewed obliquely, *Brit. J. Ophthalmol.*, 32, 1948, 347-354.

<sup>4</sup> J. Ten Doesschate, Extra-foveal scotopic absolute threshold and the distribution of retinal rods, *Ophthalmologica*, 117, 1949, 110-115; Crozier and Holway, *op. cit.*, 354; Sloan, *op. cit.*, 1950, 1084.

<sup>5</sup> Stiles and Crawford, *op. cit.*, 261; Wentworth, *op. cit.*, 148; Sloan, *op. cit.*, 1947, *Personal communication*.



retina through combining the results of these investigations.<sup>6</sup> The impossibility of such a combination of data is further suggested by the finding of Kleitman and Piéron that the area-intensity relation depends upon the retinal location where it is investigated.<sup>7</sup> Accordingly, our aim was to determine thresholds on a number of meridians under a uniform set of conditions that a map of scotopic sensitivity may now be constructed.

*Apparatus.* In the present study it was necessary to provide for stable fixation, control of stimulus-intensity, and total dark adaptation. These specifications were met in the apparatus to be described. Light from an automobile headlamp was led through a neutral wedge and balancer, a ground glass and finally through an opal glass stimulus-patch. Calibration of intensity was done photometrically with the aid of a Macbeth illuminometer.

S was seated in a light-tight cubicle at a distance of 17 in. from the stimulus-patch. Fixation was controlled by small lights attached to a perimeter. These lights could be independently illuminated by S and kept at a brightness consistent with easy fixation.

Eight fixation-lights were located on each arm of the perimeter, providing eccentricities of stimulation at approximately 4, 8, 12, 16, 24, 32, 48, and 56°. The perimeter, mounted on a short piece of metal pipe, could be rotated about its axis to permit observations along a number of meridians. A small hole drilled through the perimeter and centered on the point of rotation defined the limits of the 1° test-patch.

Sixteen radii spaced at intervals of 22.5° were investigated. Thresholds were thus taken at 128 points of the retina in addition to the fovea. To investigate foveal sensitivity without requiring foveal fixation, four fixation-lights were placed to form the corners of a square at 4° from the center of the test-patch. S was required to look at the center of the four lights when foveal thresholds were determined.

*Subjects and procedure.* Eight men and women, students at Emory University, participated as Ss. All were carefully instructed as to the nature of their task and all had undergone at least four practice sessions which were procedurally identical with the experimental sessions.

Every experimental session lasted about two and one-half hours. Four experimental sessions were required, during each of which two meridians selected at random were investigated. Half of the radii were tested from periphery to fovea and the remaining half from fovea to periphery to balance out any intra-session threshold trends.

S was permitted to dark adapt for 40 min. prior to testing during which time a number of practice thresholds were taken to provide further adjustment to the test-situation. On all occasions the following method of threshold determination was employed. First, two dim but easily visible flashes were given. Then the intensity of the flash was decreased to below threshold and slowly increased by discrete steps

<sup>6</sup> Sloan, *op. cit.*, 1947, 708; Crozier and Holway, *op. cit.*, 341-364; Stiles and Crawford, *op. cit.*, 261.

<sup>7</sup> Kleitman and Piéron, *op. cit.*, 57-91.

until S indicated seeing two successive flashes. Five thresholds were taken at each retinal location, the threshold intensities in log micromicrolamberts being recorded as the median of these five readings. All observations were monocular and were made with the natural pupil of the right eye. The left eye was occluded for fixation as well as for testing, thus providing for accommodation and convergence at a

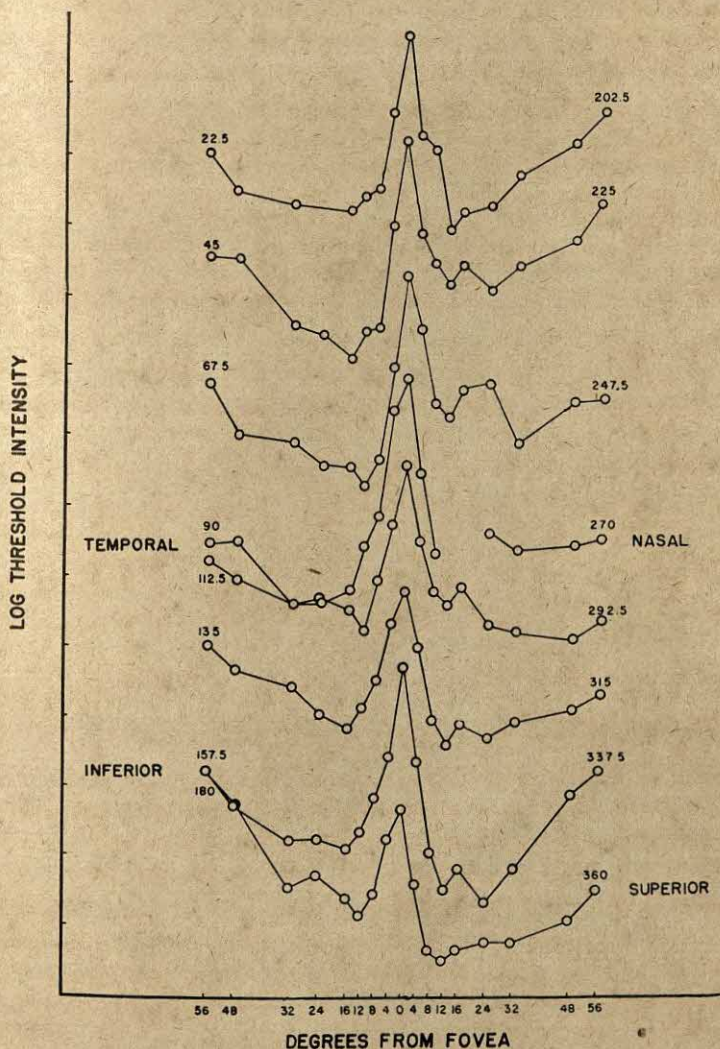


FIG. 1. RELATIVE MEAN THRESHOLD AS A FUNCTION OF LOCUS OF STIMULATION  
The curves are separated by intervals of 0.375 log units.



distance. Flash durations, controlled manually by a drop-leaf shutter, varied randomly between one-half and three-fourths second, well beyond the range where duration is a significant factor in threshold determination.<sup>8</sup> Frequent rest-intervals were allowed during testing to prevent excessive fatigue.

*Results.* The thresholds of the eight Ss were averaged at each point. These values are portrayed graphically in Fig. 1. This figure, containing eight curves, one for each meridian, demonstrates the threshold levels at varying eccentricities. The parameters of the curves are the azimuthal

TABLE I  
MEAN THRESHOLD INTENSITIES IN LOG MICROMICROLAMBERTS FOR  
VARIOUS POSITIONS OF THE RETINA

Retinal quadrant	Azimuth of Radius	Eccentricity								
		0°	4°	8°	12°	16°	24°	32°	48°	56°
Superior Temp.	22.5°	4.54	4.26	4.00	3.97	3.92	3.92	3.94	3.99	4.13
	45°	4.55	4.24	3.88	3.87	3.77	3.86	3.89	4.14	4.14
	67.5°	4.44	4.11	3.78	3.69	3.76	3.77	3.85	3.89	4.07
Temp.	90°	4.45	4.33	3.96	3.85	3.70	3.66	3.65	3.88	3.87
Inferior Temp.	112.5°	4.51	4.30	4.11	3.93	4.00	4.05	4.03	4.12	4.19
	135°	4.45	4.33	4.13	4.04	3.96	4.01	4.11	4.17	4.26
	157.5°	4.55	4.23	4.09	3.97	3.91	3.95	3.94	4.06	4.19
Inferior	180°	4.43	4.32	4.12	4.04	4.11	4.19	4.15	4.45	4.56
Inferior Nasa	202.5°	4.54	4.19	4.12	3.84	3.90	3.93	4.04	4.15	4.26
	225°	4.55	4.21	4.11	4.03	4.10	4.00	4.09	4.18	4.31
	247.5°	4.44	4.25	3.98	3.94	4.03	4.05	3.84	3.98	3.99
Nasal	270°	4.45	4.11	3.82	—	—	3.89	3.84	3.85	3.87
Superior Nasal	292.5°	4.51	4.25	4.06	4.02	4.08	3.95	3.92	3.89	3.96
	315°	4.45	4.24	3.99	3.89	3.97	3.92	3.98	4.02	4.07
	337.5°	4.55	4.22	3.90	3.76	3.83	3.71	3.83	4.10	4.18
Superior	360°	4.43	4.15	3.92	3.88	3.92	3.95	3.95	4.03	4.14

angles of the meridians measured from the vertical. When referred to retinal location that segment of the curve between the fovea and 360° denotes the superior portion of the retina; that along the fovea-90° path, the temporal part of the retina. The abscissa indicates eccentricity from the point of fixation, the ordinates being log threshold intensities. Although absolute values are not specified for this latter dimension the interval between marks on the ordinate equals 0.25 log unit. The curves are separated by an interval of 0.375 log units. The actual values on which these curves are based may be obtained from Table I.

<sup>8</sup> C. H. Graham and R. Margaria, Area and the intensity-time relation in the peripheral retina, *Amer. J. Physiol.*, 113, 1935, 299-305.



A number of facts may be derived from an inspection of the curves. First, the fovea has the highest threshold of any region within the range of our measurements except for the blind spot and the extreme inferior retina. Secondly, gradients of sensitivity are not identical. Thirdly, the lowest thresholds are found about  $20^\circ$  from the fovea on the temporal side of the horizontal meridian. Fourthly, regardless of the direction from the fovea, a point is reached beyond which the threshold rises. Finally, a small increment in threshold can be seen occurring on the nasal retina at eccentricities equalling that of the blind spot.

The data are summarized in a somewhat different manner in Fig. 2. This figure is a polar projection of the retinal surface divided into narrow bands by contours of equal threshold. It may be taken to represent a contour map of scotopic sensitivity. The band widths are indicated on the figure. The pole is placed at the fovea, with radial distances from it representing degrees eccentricity of points of stimulation. The radii along which measurements were made are also indicated in the figure. These radii,  $22.5^\circ$  apart, are designated in a counterclockwise direction from the vertical radius of the superior retina. Since measurements were made only within  $56^\circ$  eccentricity of the fovea, contours more peripheral than this were estimated by extrapolation. The amount of light required to stimulate the eye is indicated by the density of the dots within each band. Regions of low sensitivity are represented as dark areas, those of high sensitivity as light areas.

It is obvious that the various gradients of sensitivity are not equal, those along the horizontal meridian showing more extended regions of low threshold and thus providing the generally ovoid shape of the contours. It is also to be noted that sensitivity is somewhat deficient in the greater portion of the inferior retina, a finding in agreement with the results of Stiles and Crawford,<sup>9</sup> Sloan,<sup>10</sup> and Riopelle and Hake.<sup>11</sup>

In addition to this general picture, a few of the lesser systematic variations can be noted. One appears to be a recurring maximum threshold in the nasal portion of the fovea at a distance of  $16^\circ$  eccentricity (cf. Fig. 1). This phenomenon was noted previously by Stiles and Crawford and was attributed by them to the presence of a blood vessel radiating from the blind spot and passing on either side of the fovea.<sup>12</sup> Secondly, a number

<sup>9</sup> Stiles and Crawford, *op. cit.*, 255-280.

<sup>10</sup> Sloan, *Personal communication*.

<sup>11</sup> A. J. Riopelle and H. W. Hake, Area-intensity relations in scotopic vision using annular stimuli, *J. Exper. Psychol.*, 42, 1951, 54-58.

<sup>12</sup> Stiles and Crawford, *op. cit.*, 255-280.



of small 'fingers' of sensitivity can be seen radiating from the fovea. It is not known, however, to what extent these effects are the results of day-to-day fluctuations or to real local variations in sensitivity. On the one

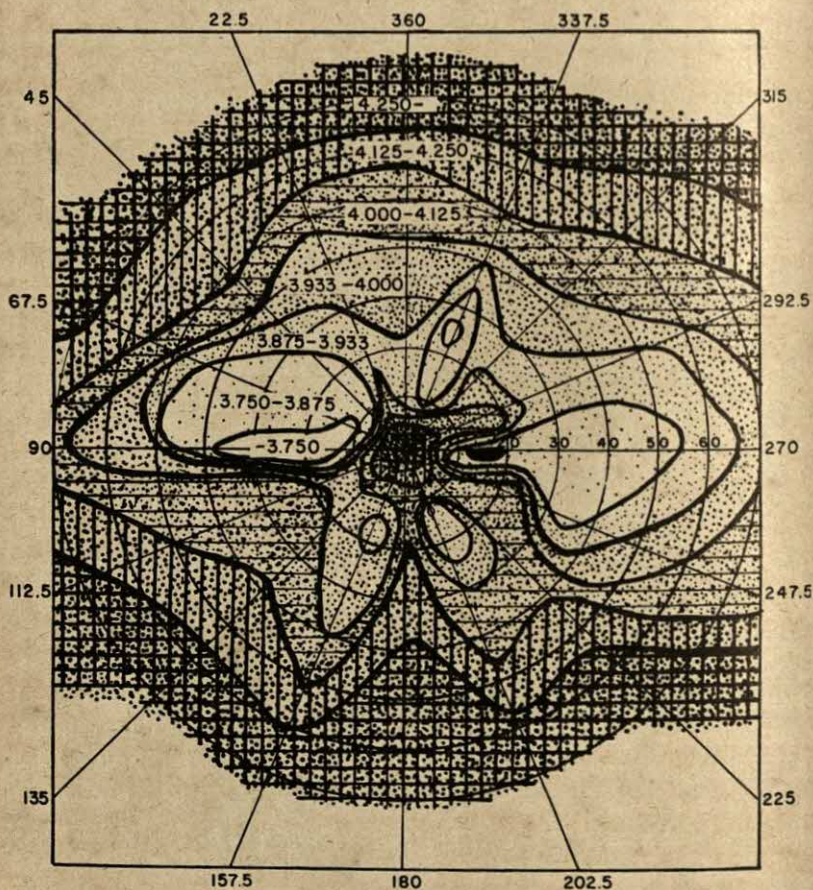


FIG. 2. CONTOURS OF EQUAL SENSITIVITY FOR THE DARK ADAPTED EYE AS REPRESENTED ON A POLAR PROJECTION

Azimuthal angle is measured in a counterclockwise direction from the vertical radius of the superior retina. Contours beyond 56° eccentricity are estimated by extrapolation. Band widths are indicated on the figure in terms of log micromicrolamberts.

hand, if contours such as these adhere faithfully to every fluctuation of the data, sampling fluctuations will be emphasized. Meanwhile, on the other hand, excessive 'averaging' may obscure real variation. It is certain, at least, that the random sequence of meridians investigated did not favor any particular dimensions. Furthermore, the variations in meridional



gradients obtained in this experiment are in the same direction as those previously reported by Stiles and Crawford<sup>13</sup> and by Sloan.<sup>14</sup>

*Discussion.* The general distribution of scotopic sensitivity is fairly clear; maximal sensitivity is found on either side of the fovea on the horizontal meridian in the neighborhood of 20-30° eccentricity. Thresholds are generally lower for the superior retina than for the inferior retina, but the range of low thresholds is not as extensive as for the horizontal meridian. Bands of equal sensitivity appear to be roughly elliptical in shape with the horizontal meridian as the major axis.

A number of reports<sup>15</sup> have compared the gradient of sensitivity along the horizontal meridian with Østerberg's density gradient of rods and cones. Although Østerberg's counts were most nearly complete along this meridian ( $\pm 30^\circ$  of azimuthal angle), he made additional counts at a number of other locations which led him to conclude that "the number of rods increases rapidly and regularly in all directions from the centre—with the exception of the neighbourhood of the papilla where the increase is less uniform. . . . While thus the distribution of the rods was found to be about the same in all directions from the centre to the maximum, so that the delineated rod belts appeared like circles around the centre, the rod belts peripheral to the rod maximum were oval or ovoid in outline coming near the centre inferiorly and temporally, and indicating a lesser density of the rods in these parts of the peripheral retina than in the corresponding parts of the upper and nasal quadrants."<sup>16</sup>

Before comparisons between the threshold and the neuroanatomical data can be made, the various estimates of threshold must be corrected for the changes in effective pupillary area with increasing eccentricities of stimulation. Empirical determinations of these corrections have already been made by Spring and Stiles<sup>17</sup> and by Sloan.<sup>18</sup> The results of such treatment, which changes estimates less than 0.10 log unit within 40° of the optic axis, would appear in Fig. 1 as a decrease in threshold in the periphery. The effect on a contour map, such as Fig. 2, however, would be little more than to further emphasize the oval characteristic of the contour.

<sup>13</sup> Stiles and Crawford, *op. cit.*, 255-280.

<sup>14</sup> Sloan, *Personal communication*.

<sup>15</sup> Sloan, *op. cit.*, 1950, 1083 ff.; Ten Doesschate, *op. cit.*, 112; Crozier and Holway, *op. cit.*, 341-364.

<sup>16</sup> G. Østerberg, Topography of the layer of rods and cones in the human retina, *Acta Ophthalm., Suppl.*, 6, 1935, 76.

<sup>17</sup> Spring and Stiles, *op. cit.*, 352.

<sup>18</sup> Sloan, *op. cit.*, 1950, 1083.



Thus, Fig. 2, although constructed from uncorrected data, is believed to portray adequately the general topography of retinal sensitivity.

A comparison of these threshold contours with the anatomical contours of Østerberg reveals both similarities and differences. The major agreement lies in the finding of regions of maximal sensitivity and of maximal rod density. Correspondence in location among these two sets of maximum, however, is not perfect. The receptor density gradients are uniform within  $20^\circ$  eccentricity, but variations occur in the threshold gradients in this same region.

Other important differences are that the location of the region of minimal threshold is found in the temporal rather than in the nasal retina as would be predicted from the rod counts, and that the major axis of the oval contours of sensitivity in the far periphery is inclined differently from that described by Østerberg.

None of the previous comparisons between visual sensitivity and Østerberg's counts has yielded perfect correlations. Consideration of possible individual differences in sensitivity gradient, individual differences in receptor density gradient, and possible summation and other factors, permits the reconciliation of even marked discrepancies. However, since independent estimates of the contributions of such variables are yet to be determined, any complete and confident specification of such differences is at present impossible.

#### SUMMARY

Estimates of monocular scotopic brightness sensitivity were obtained from 8 Ss for 8 positions on each of 16 equally-spaced radii using a modified method of limits. Isometric sensitivity contours constructed from these estimates are generally ovoid in character with the major axis in the horizontal dimension. Maximal sensitivity is found on either side of the fovea on the horizontal meridian in the neighborhood of  $20\text{--}30^\circ$  eccentricity. While thresholds are generally lower for the superior than for the inferior retina, the range of low thresholds is not as wide as for the horizontal meridian. These results are discussed in terms of available information on the distribution of retinal structure.



## A TEST OF THE VALIDITY OF THE ELSBERG METHOD OF OLFACTOMETRY

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In the Elsberg or blast-injection method of olfactometry, the absolute threshold is determined by finding the magnitude of blast of odorous air injected into the subject's nostril or nostrils which will give rise to an odor sensation.<sup>1</sup> This is accomplished by the very simple device of releasing, through a pinchcock, odorous air which has been compressed by a known amount by means of a hypodermic syringe. Although the measurements so obtained are ordinarily expressed in terms of volume, Jerome has shown that the effective variable is actually pressure.<sup>2</sup> The importance of pressure is further emphasized by Wenzel, although she further attempted to translate her stimulus-magnitudes into number of odorous molecules in the blast.<sup>3</sup> Jerome's discovery that volume as such was not a variable would suggest, however, that the number of odorous molecules in the blast is not important so long as the concentration of odor is kept constant, as it always is in either the original Elsberg method or in Wenzel's adaptation of it.<sup>4</sup> We may conclude, on the basis of the above comments, that an Elsberg or blast-injection threshold has a meaning considerably different from a threshold obtained by changing the concentration of odorous molecules in the inspired or injected air. In fact, it becomes a matter of moment to decide whether or not the blast-injection threshold as ordinarily used is an olfactory threshold at all. That is, does the threshold depend upon the sensitivity of the receptors, or does it depend upon the aerodynamics of the nose?

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<sup>1</sup> C. A. Elsberg and I. Levy, The sense of smell: I. A new and simple method of quantitative olfactometry, *Bull. Neurol. Inst. N.Y.*, 4, 1935, 5-19.

<sup>2</sup> Cf. F. J. Hammer, The relation of odor, taste, and flicker-fusion thresholds to food intake, *J. Comp. Physiol. Psychol.*, 44, 1951, 403-411; Franz R. Goetzl, M. S. Abel, and Ann J. Ahokas, Occurrence in normal individuals of diurnal variation in olfactory acuity, *J. Appl. Physiol.*, 2, 1950, 553-562; and E. A. Jerome, Olfactory thresholds measured in terms of stimulus pressure and volume, *Arch. Psychol.*, 1942, No. 274, 1-44.

<sup>3</sup> Bernice M. Wenzel, Differential sensitivity in olfaction, *J. Exper. Psychol.*, 39, 1949, 129-143.

<sup>4</sup> Jerome, *op. cit.*, used citral floating on water; Wenzel, *op. cit.*, used phenyl ethyl alcohol, the conditions making for a saturated vapor; Goetzl, *op. cit.*, 554, used "ground coffee of a standard brand." In all cases, the odorous air would be at maximal saturation at the given temperature.



The questions proposed in the preceding paragraph can be answered by varying the concentration of odor in the injected air, and determining the effect thereof upon the blast-injection threshold. The reasoning here is that if the concentration of odorous molecules, or their number in the blast, is of importance in determining the blast-injection threshold, then blasts of different magnitude should be necessary to reach threshold with odors of different concentration. For example, if one bottle contains air saturated with a given odor, and another contains air with only one-third the saturated concentration, then a proportionally larger blast of the less concentrated air should be required to reach threshold. If, on the other hand, the aerodynamics of the nose is the determining factor, and it is further assumed that the weaker concentration is above threshold olfactorily, then the concentration should have no effect. Needless to say, we may also encounter various intermediate possibilities, in which the obtained blast-injection threshold is a function of both variables, plus, perhaps, still others. The experiment reported here attempts to decide the issue presented above.

#### APPARATUS AND PROCEDURE

The design of the six stimulators followed essentially the Elsberg pattern.<sup>5</sup> Since Jerome's work showed that the volume of the container was of importance only insofar as it affected the pressure obtained from a given volumetric compression, no attempt was made to duplicate the Elsberg bottles exactly. The bottles used in the present study had a capacity of about 250 ml., and were as alike as modern mass-production methods could make them. Each was provided with a rubber stopper through which two glass tubes were inserted. One of these tubes was attached during experimentation to the hypodermic syringe used for compressing the contents, and extended to within about 2 cm. of the odorous liquid within the bottle. The exit tube, attached, when the bottle was in use, to the nose-piece, extended only a millimeter or two into the bottle. The stoppers were carefully covered with aluminum foil, and the connection of tube to syringe and tube to nose-piece was effected with silicone rubber tubing.<sup>6</sup> There were, therefore, absolutely no odorous, foreign materials introduced at any point in the system.

The odorous material in all cases equaled 20 ml. in volume. To obtain different concentrations of odor, advantage was taken of the fact that in solution the vapor pressure of a substance is an inverse ratio of its molecular concentration. The solvent used was heavy mineral oil, USP, which is odorless and with which the two odorous substances used are completely miscible. The odorous substances were n-octane, obtained from the Eastman Kodak Company, and amyl acetate, obtained from the Mefford Chemical Company. Three concentrations of each were prepared as follows:

<sup>5</sup> Elsberg and Levy, *loc. cit.*

<sup>6</sup> This tubing was obtained from the Connecticut Hard Rubber Company, New Haven, Connecticut.



(1) pure; (2) half odor and half mineral oil; (3) one part odor to nine parts mineral oil. If we assume the molecular weight of the mineral oil to be 414, and further assume ideal conditions, we should expect the concentrations of each odor to be available, therefore, (1) at the level determined by the vapor pressure of the substance; (2) at a level of 76% of the pure substance in the case of amyl acetate, and 74% for octane; and, (3) at a level of 26% of the concentrated level for amyl acetate and 24% for octane. When the unstoppered bottles were sniffed, the differences in concentration were clearly evident, as would be expected from the approximate DL for odor.<sup>†</sup> The n-octane was very much weaker in odor than the amyl acetate.

For reasons of simplicity, it was decided to use monorhinal stimulation. Hence, single nose-pieces were constructed for each odor which consisted of a straight tube, 3 mm. in inside diameter, sealed through a flaring tube which served to close off the nostril. To keep the jet as constant as possible, the same nose-piece was used for each level of a given odor, but was purged with air at the level of concentration each time the bottles were changed. The varying degrees of compression were obtained with a standard 50 ml. hypodermic syringe. All experimentation was done in the same room, which was located in the basement, without outside opening, and which was kept at a relatively constant temperature by a thermostatically controlled flow of washed air from the central ventilating system. There was never more than a 0.5° F. change during any one session (each session required about 40 min.), nor more than a 1° F. change from one day to the next.

In all, 10 Ss were used, including E, who was tested by MHJ. Except for E, the design and purpose of the experiment were known to none of the Ss, and, since E's results appear to be of the same order as those for the other Ss, they have been retained in the calculations. The other 9 Ss were staff members and graduate students in psychology. None was experienced in olfactory work but no difficulty in making the required judgments was encountered by any of them. Each was used during only one relatively brief session, hence problems of diurnal and day to day variation were not involved.

Every S was tested with every odor at one sitting. To minimize systematic effects, several different orders of presentation were used for the different Ss, followed immediately by the same order in reverse (a 'balanced order,' in other words). The one limitation placed upon the orders was that all three levels of the same substance were always given in succession, although, of course, sometimes starting with the lowest concentration, sometimes with the highest. This arrangement had the advantage of simplicity in manipulation of the apparatus, and inspection of the data reveals no particular trend or systematic bias. Every threshold reported in the results thus represents two measurements for every S on each odor. This was deemed sufficient for two reasons, first, the variability of results was far less than the expected variation from level to level if the technique were an actual test of odor threshold, and, secondly, it was desired not to fatigue the Ss. As is standard practice with the blast-injection method, all thresholds were obtained by an ascending series, and each blast exceeded the previous one by 0.5 ml. It should be emphasized that the utmost care is required for S not to breathe during or immediately after a blast.

<sup>†</sup> Wenzel, *op. cit.*, 129-130, discusses the Weber fraction for odor.



Fortunately, *S* can almost always distinguish between an immediate odor, and one arising after a breathing movement.

### RESULTS

The results are summarized in Table I. In keeping with the usual practice, thresholds are given in terms of milliliters of blast, rather than in terms of pressure. An approximate translation to pressure would place the thresholds at 5 to 7 mm. of Hg, which is of the order of magnitude of the thresholds obtained by Jerome for a different substance, and with birhinal stimulation. To check on the statistical significance of the differences obtained, *t* was calculated for the largest difference, that between

TABLE I  
ABSOLUTE THRESHOLDS STATED IN MILLILITERS FOR 10 *Ss*  
Percentages refer to calculated vapor pressure of concentrated vapor.

	n-octane			Amyl acetate		
	100%	74%	24%	100%	76%	26%
Mean (ml.)	1.85	2.23	2.15	1.75	1.83	1.75
SD	0.64	0.59	0.68	0.54	0.88	0.80

100% and 50% n-octane, using a distribution of actual differences. This *t* was only 0.73, which is not at all significant, and so no others were calculated. It is true, of course, that if a very large number of *Ss* were used, a difference of the magnitude found would be statistically significant. The differences are small, however, compared to those which would be predicted on an 'equal molecule' basis, and the trend from more to less saturated is not consistent.

It would appear from these results that thresholds obtained by use of the Elsberg or blast-injection technique are not understandable in terms of molecular concentration. Over a wide range of concentrations the obtained thresholds are independent of the concentration of odor in the injected air, which would preclude any translation into molar or similar terms referring to molecular per unit volume. If the blast injection thresholds are not true olfactory thresholds in the usual, that is, molecular concentration sense of the term, then we must look for some other way in which to understand the effect of increasing pressure. This leads at once to considerations of the aerodynamics of the system. Since the situation is actually very complicated, it is possible here only to outline the general problem.<sup>8</sup>

In the blast-injection technique we are dealing basically with a jet flow

<sup>8</sup> Mr. Ernst R. Letsch of the College of Engineering at UCLA is attempting to analyze the stimulus-situation in aerodynamic terms.



phenomenon, modified by the size and conformation of the nasal cavity into which the jet is discharged. It is obvious, therefore, that the distance which the injected air will carry through the nasal passage, and the amount of mixing with the air already in the passage, will depend upon the pressure and duration of the blast, the size of the orifice in the nose-piece, the width and shape of the nasal passage, and, to a minor extent in this case, the density of the vapor.<sup>9</sup> In addition, the 'pulse' would have to be taken into account, which further complicates matters. Considerable aerodynamical research, using models of the nose, would be necessary before the generalities stated above could be made more specific. Obviously, such a problem is of no direct interest in the present context.<sup>10</sup>

It would certainly seem to be good practice to utilize, in olfactory work, a technique which varies concentration rather than pressure. If one uses a blast of odorous material which is of sufficient pressure and volume to eliminate them as effective variables, and finds odor thresholds in terms of molecular concentration, the task of working out the relationships between stimulus-effectiveness and physical and chemical characteristics of the odor is very much simplified. In the usual blast-injection technique, unfortunately, one really does not know exactly what he is measuring, except that it is certainly not molecular concentration, and therefore he cannot compare his results with olfactory thresholds obtained by other methods.

#### SUMMARY

(1) Blast-injection olfactory thresholds were obtained for 10 Ss using three levels of concentration of each of two substances—*n*-octane and amyl acetate.

(2) No significant effect of concentration was found.

(3) It was concluded that the blast-injection threshold is not comparable to thresholds found in terms of odor concentration, and is not translatable into molecular terms.

<sup>9</sup>For 100% *n*-octane, the total mass of air has less than 6% octane vapor by weight. Thus, changing the weight by dilution has a very small effect.

<sup>10</sup>If the dilution were carried further, there would probably be a point at which both concentration and pressure would affect the threshold, the limiting case being an odor which would be below threshold at any pressure. This is not, however, relevant to the present discussion of the Elsberg technique as generally used.



## THE PERCEPTUAL MODIFICATION OF COLORED FIGURES

By ROBERT S. HARPER, Knox College

The past decade has seen a renewed interest in the study of perception, and some of the problems considered at the turn of the century are being attacked again. One of the problems of yesterday—formulated in the language of the time—was whether the elements that theoretically make up a percept are discernible in it. Today, in spite of attempts to avoid it, the same problem is posed by the results of experiments on the effects of motivation and past experience on perception. In a recent examination of some of these experiments, Pratt has concluded that there is no evidence for the direct influence of motivational and experiential variables on perception—that the reported relationships can be explained, in effect, in terms of response-generalization.<sup>1</sup> Here is a significant question concerning the characteristics of the 'mechanism' which intervenes between stimulus and response: although it may be avoided by an operational or behavioral logic, it is capable of experimental analysis.

In an experiment by Bruner, Postman, and Rodrigues, each of a number of selected figures (physically equal in color) was matched with a differential color-mixer.<sup>2</sup> Some of the figures (such as a lobster-claw or a lemon) had a characteristic red or yellow color, while others (such as geometrical forms) had no characteristic color. The results indicated that more red was added to the mixture for characteristically red figures than for neutral figures, and that more yellow was added to the mixture for characteristically yellow figures than for neutral figures. Unfortunately, however, these results are ambiguous; they can be interpreted either in terms of perceptual modification or in terms of response-generalization. From Pratt's point of view it might be maintained that *O* responded to a particular red color on the mixer *as if* it were the same color as that of characteristically red figures and *as if* it were redder than a neutral figure.

In the present experiment, a more adequate method was employed. A

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<sup>1</sup> C. C. Pratt, The rôle of past experiences in visual perception, *J. Psychol.*, 30, 1950, 85-107.

<sup>2</sup> J. S. Bruner, Leo Postman, and John Rodrigues, Expectation and the perception of color, this JOURNAL, 64, 1951, 216-227.



figure was superimposed on a background the color of which was varied until the figure was no longer detectable. The criterion of 'undetectability' makes it possible to distinguish between variation in perception and variation in response. If *O* is unable to detect a colored figure superimposed on a colored background, then the figure and ground are not merely being *responded* to as the same but are being *perceived* as the same. As in the experiment by Bruner et al, two classes of figure were used to study the effect of organizational factors in perception.

*Procedure.* Three pairs of stimulus-figures were made. One member of each pair was a 'meaningful' figure (*i.e.* one with a characteristic color). The other member of each pair was a 'non-meaningful' figure (*i.e.* one with no characteristic color) which had the same area and the same general contour characteristics as did its meaningful correlate. The three meaningful figures, all of them characteristically red, were an apple, a heart, and a lobster. Their respective 'non-meaningful' correlates were an oval, an isosceles triangle, and the letter 'Y.' All six figures, cut from the same sheet of orange paper (a scrap of which was, for *E*, indistinguishable from a background composed of 45° red and 315° orange), were mounted on cardboard grounds attached to small wire hangers.

A reflecting differential color-mixer was used to vary the background color.<sup>3</sup> *O* sat 3 ft. in front of a 3 × 4 ft. screen, in the center of which was a 2-in. aperture completely filled with the reflected background color. A small semi-cylindrical holder was so fitted into this aperture that the cut-out figures could be suspended in the middle of the patch of variable colored background. A black-cardboard shield was attached to the front of the holder, and the 2-in. opening in the shield was covered with a thin sheet of frosted plastic which blurred the sharp contours of the stimulus-figures. The apparatus was set up in a darkened room, with the figures being illuminated by an incandescent light placed above the holder, and with the variable colored background being illuminated by lights symmetrically placed to the left and right of the aperture. The two color-disks of the mixer were light orange and dark red, respectively.

Five *O*s, of varying degrees of psychological sophistication, were asked to tell *E*, as *E* continuously varied the background color, when the figure was no longer distinguishable from the background on which it was superposed. When the *O*s entered the experimental room, they were given the following instructions.

*Instructions.* I am going to place some small figures in this opening, and then I will gradually change the color of the background. I want you to tell me when the figure can no longer be distinguished from the background—when it merges with the background.

*E* then placed, successively, each figure in the holder and, starting either from a background with 0° of red or from a background with 180° of red, made two determinations for each figure. Three *O*s were shown the three non-meaningful figures first and then the meaningful ones. The other two *O*s were shown the

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<sup>3</sup> R. S. Harper and C. R. Oldroyd, An inexpensive differential color-mixer, this JOURNAL, 65, 1952, 614-616.



meaningful figures first. The figures were concealed from *O* until they were in the holder. As *E* stepped back from inserting the figure, he said, if it was a meaningful figure, "There is a reddish apple," ["heart" or "lobster"]; if it was a non-meaningful figure, "There is a yellowish-orange oval," ["triangle" or "Y"].

Varying the amount of red in the background not only changed the hue of the background, but, since the red was darker than the orange, it also changed the brightness of the ground. The presence of the shield in front of the holder for the stimulus-figures prevented absolutely uniform illumination on the figures, since the light source could not be placed perpendicular to them. The *Os* experienced some difficulty in getting a setting of the background which would make the whole figure, with its differential brightness at top and bottom, completely merge with the ground. When the *Os* reported this difficulty, *E* suggested that they attend to the upper central portion of the figure, since this was the area in which existed the defining characteristics of the figures.

TABLE I  
BACKGROUND COLOR REQUIRED FOR INDISTINGUISHABILITY  
Degrees of red in the background-color required by *O* to  
cause the figure to merge with the background.

<i>O</i>	triangle	heart	oval	apple	letter 'y'	lobster
M	31	121	6	153	124	158
	51	105	63	136	136	171
L	111	90	58	157	111	165
	66	105	81	161	117	156
C	84	123	30	129	117	170
	85	130	42	121	66	132
S	63	105	36	156	117	156
	45	110	15	137	91	153
H	57	75	33	144	117	157
	15	92	46	125	126	154

*Results.* The *Os* reported the merging of the figure with the ground with a very confident "Gone," sometimes preceded by a tentative "Going, going. . . ." Two sophisticated *Os* were used to evaluate the criterion-effect. While they were fixated on a particular part of the figure, and without stopping to record their settings in order not to influence their fixation, *E* changed the amount of red in the background fairly rapidly. In as many as seven 'passes' through the color-range, these two *Os* consistently reported the figure as "Gone" within a range of only 3°. These incidental observations simply serve as evidence that, perceptually, the figures truly disappeared, and that the results obtained were not due to some unknown artifact of the situation.

The results are given in Tables I and II. Table I shows the two settings of each *O* for each figure. Table II shows the average number of degrees

of red that each *O* added to the background to make the non-meaningful figures and the meaningful figures merge with the background. The mean difference in the number of degrees of red required for the two sets of figures to become undetectable ( $63.56^\circ$ ), evaluated by *t* for paired scores, was highly significant ( $t = 13.27$ ,  $P < 0.001$ ). Since, in this situation,

TABLE II  
BACKGROUND COLOR REQUIRED FOR INDISTINGUISHABILITY  
OF EACH CLASS OF FIGURES

<i>O</i>	Non-meaningful	Meaningful
M	68.50	140.67
L	90.67	139.00
C	70.67	134.17
S	61.17	136.17
H	65.67	124.50
Mean	71.34	134.90

$M_{diff} 63.56$ ;  $SE_D 4.79$ ;  $t 13.27$ ;  $P < 0.001$ .

when the figure merges with the background, a reasonable assumption is that *O* experiences both the figure and the ground as the same color, the conclusion can be drawn that the two classes of figure were *perceived* as being of different colors.

*Summary.* In this experiment the effect of past experience on the perception of color was studied. The method employed was based on the assumption that if a colored figure is indistinguishable from a colored background, the two colored surfaces are perceived as identical and not merely 'responded to' as identical. Significant perceptual changes were found.



## APPARATUS

### THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF TEXAS

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During the past summer, the Department of Psychology of The University of Texas occupied its new building, Mezes Hall, which had been in the process of construction since the spring of 1950. This building, the middle of three built in the same style of architecture on the east side

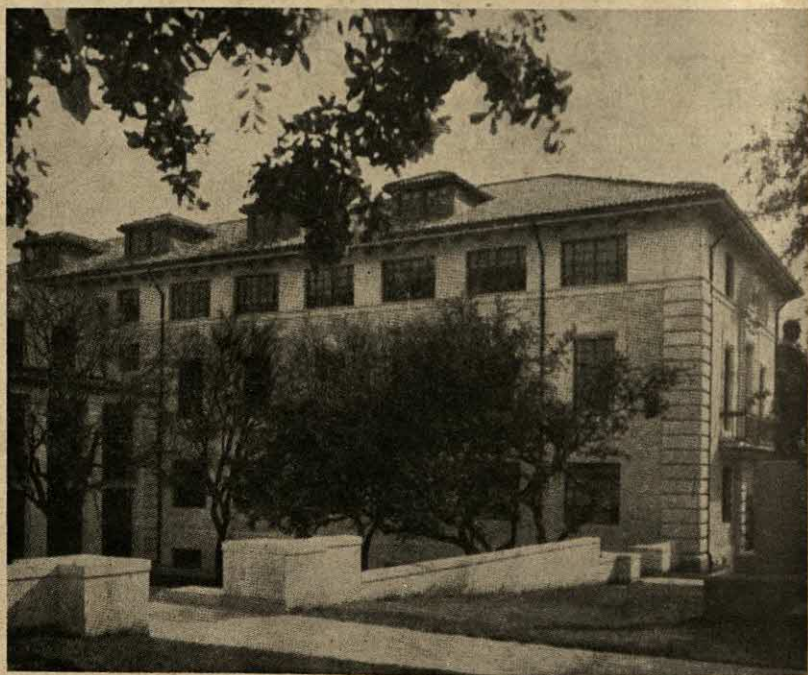


FIG. 1. MEZES HALL

of the central Mall of the campus, was planned and constructed with the needs of the Department of Psychology in mind.

Mezes Hall (Fig. 1) is a five-story building, 150 ft. in length and 57 ft. in width. It is built on a slope which is so steep that the basement is underground at one end—the end toward the Mall—and several feet above

ground at the further end. All the rooms of the basement, except those given over to utilities and mechanical equipment, are consequently above ground and have outside windows (see Fig. 2).

The building is constructed of native Cordova shell limestone with Cordova cream limestone trim. It is of modern, fire-proof, reinforced concrete construction and is air-conditioned throughout. The floors of the offices and rooms are covered with asphalt tile; of the halls with rubber tile; and of the entrances with travertine marble. The walls of the entrances and corridors are marble and glazed tile. The stairways are of stone. An automatic passenger elevator, operated by key, serves the first four floors. The corridors of every floor have a drinking fountain, several bulletin boards, a glass display case built in the wall near the side entrance, and, except for the first and fourth floors, a telephone booth.

The rooms and halls of the building are illuminated by fluorescent light and every room is amply supplied with 110-v. AC wall plugs. The laboratories are supplied, in addition, with 28-v. DC and 400 cycle, 110-v. AC, from special generators and motors in the rooms on the ground floor devoted to mechanical equipment (Rooms 1 and 2). An inter-laboratory wiring system, 12 intercommunicating No. 12 wires all of which are electrically shielded, connects the rooms of every floor.

Although the intercommunicating system of every floor is independent of those of other floors, any room of any floor may be connected with any room of any other floor merely by plugging into a vertical wiring system which connects the various floors. The horizontal division of the wiring system by floors makes for greater flexibility and greatly reduces the number of wires necessary for the intercommunicating system. The use of a set of wires on one floor does not interfere with the use of the same set on another floor. The need for electrical connections between rooms of different floors is small in comparison with that between rooms of the same floor.

*Ground floor.* In addition to mechanical equipment and utilities and rest-rooms for men and women, the ground floor (see Fig. 2) is devoted to specialized rooms: (a) a balance room; (b) the comparative laboratory; (c) the auditory suite; (d) the constant temperature rooms; (e) the demonstrational laboratory; and (f) the olfactorium.

(a) *Balance room.* The balance room (Room 3) contains scales and weights. Its floor, which rests on piers anchored in bed rock, is free from vibration hence delicate weighings are possible.

(b) *Comparative laboratory.* The comparative laboratory consists of six rooms: an operating room (Room 6A); two small inner rooms (4A and 8) which may be used as darkrooms; a small outside room (4B); an animal or specimen room (4);





The Department's mammalian laboratory is housed in a separate building on the campus and the anthropoid laboratory is at the Off-Campus Research Station, eight miles from the University.

(c) *Auditory suite.* The auditory suite consists of three rooms: an anteroom (Room 9) which services a large anechoic room (9A) and a small soundproof room (9B). The anechoic and soundproof chambers are rooms within rooms. The outer rooms in which they are built have walls of various thicknesses; the outer wall of the building is 20 in. thick—4 in. of stone veneer, 12 in. of reinforced concrete, and 4 in. of rockwool; the inner walls along the corridors are 10 in. thick—2 in. glazed brick, 4-in. hollow tile, and a 4-in. layer of rockwool; the connecting cross walls are 8 in. thick—4-in. hollow tile and a 4-in. layer of rockwool. The rockwool layer, which adds to the soundproofing of the outer rooms, is held in position by hardware cloth.

The inner rooms, which are separated from the outer by air-spaces and passageways varying in width from 8 in. to 3 ft., have walls, ceilings, and floors 12 in. thick—8 in. of reinforced concrete and 4 in. of rockwool. They rest upon large vibro-isolator spiral springs which are anchored to piers sunk to bed rock. The large 'concrete boxes' do not come into contact with the building. There is a 2-in. space between them and the floor of the outer rooms which is filled with felt. The rooms are without heating, cooling, and ventilating conduits and the electrical currents and interlaboratory wiring system run into them through rubber conduits. They are, therefore, structurally isolated from the building. No sounds nor other vibrations are communicated to them by conduction.

The rooms are ventilated between experimental sessions by means of a large tubular, rubberoid conduit that is carried into them. The conduit, which has an electrical fan in the outer end, extends from the room being ventilated, through the doors, and out into the corridor. The fresh air within the corridor is drawn through the open doors into the room. Heating and cooling the rooms are, therefore, no problems.

The rockwool linings of the inner rooms are held in place by copper screening which also serves as an electrical shield. The screening is extended across the floor and doors and is soldered together along its joints and, at 4-ft. intervals, to large copper wires which completely encircle the rooms. These wires are united in a cable which is attached to a copper plate sunk in the ground below the water line. Electro-encephalographic and other recordings of like nature requiring electrical shielding may, therefore, be made within them.

Three heavy soundproofed doors, one to each of the outer rooms and two to each of the inner rooms, isolate the inner rooms from the anteroom. These doors do not lock; they are held shut by levers which draw them tightly into the beveled door-frames. They may be opened, however, from either side.

The likeness of the construction of Rooms 9A and 9B ends here. The anechoic room in addition to being sound-proofed is 'echo-proofed.' Heavy felt drapes, 1 in. thick, hang down from the ceiling at 16-in. intervals. Wings of the same material, but 2 ft. wide, protrude into the room at similar intervals from the walls. The floor is covered with 1-in. steel grating, the interstices of which are filled with rockwool.

Except for the floor, over which a half-inch ozonite mat and thick carpet are laid, the copper screening in the small soundproof room is uncovered. This room, due probably to the open screening and the rockwool behind it, is remarkably free from standing waves.



The anteroom is connected electrically to the two sound-proof rooms and to the other rooms on the ground floor through the horizontal, interlaboratory wiring system. Since the vertical wiring system, mentioned above, rises from this room, it, or either one or both of the soundproof rooms, may be connected with any laboratory in the building.

The anteroom is also electrically shielded and soundproofed. Its copper screening is covered, however, with perforated sheets of hardboard, which reduce the noise-level of the operations performed within the room.

(d) *Constant temperature rooms.* Room 11 contains two constant temperature cubicles, each 7 ft. high and 8 ft. square. These little rooms, constructed of 4-in. tile, are insulated with two layers of cork 5 in. thick and lined with tile—the walls and ceilings with white, glazed tile and the floors with brown tile. Copper screening, soldered at its joints and to grounded wires, was placed between the cork and the tile to shield the rooms electrically.

The rooms have heavy ice-box doors which close, and are hermetically sealed under the pressure of strong spring clasps, against 4-in. strips of ½-in. sponge rubber. The doors, since the rooms are relatively soundproof, cannot be locked; they may be opened from within as well as from without.

Each room has a laboratory with running hot and cold water; its own refrigerated air-conditioning unit and thermostats which control humidity and temperature; and, mounted on the outside of the cubicle, its own thermographic register which automatically records, for periods capable of being varied from a few hours to a week, the wet- and dry-bulb temperatures of the air within it. The temperatures of the rooms may be varied between 0°-70°C. and controlled within a tolerance of 0.5° C.

The hum of the air-conditioning apparatus, when in operation, serves as a sound-screen which completely obliterates all other noises within the cubicles.

(e) *Demonstrational laboratory.* The demonstrational laboratory (Room 12) contains apparatus that is devoted to classroom demonstration—particularly in elementary psychology which is taught in the adjoining auditorium. The apparatus is set up on wheel-tables and conveyed, by means of the elevator in the next room, to the stage of the auditorium or wheeled to other places in the building as occasion and need demand. The apparatus in this laboratory is used only for purposes of demonstration. It is classified and arranged upon the shelves under the categories for which it is used.

(f) *Olfactorium.* Room 13 is to be devoted to the study of smell and taste. It is to contain an olfactorium, similar to the one described in a recent number of this JOURNAL.<sup>1</sup> Dark rooms, soundproof rooms, and constant temperature rooms are utilized in visual, auditory, and cutaneous research to insure constant environmental conditions but little has been done to date to obtain these conditions in the study of smell and taste. Since smell plays such a predominant rôle in our gustatory perceptions, and since the persistency of the olfactory stimulus not only alters the smell of subsequent stimuli but also reduces the observer's sensitivity by way of adaptation, the need for an 'odorless room'—for constant environmental conditions—is greater in the study of smell and taste than in any other modality of sense. The olfactorium is the obvious answer.

<sup>1</sup> Dean Foster, E. H. Scofield, and K. M. Dallenbach, An olfactorium, *op. cit.*, 63, 1950, 431-440.



As the floor plans of Room 13 show (see Fig. 2), a small room, containing a lavatory and a cabinet-shower, was built in one corner of this laboratory. This little room is for *O*'s use that he may remove the last vestige of body odor before he dons an odorless, plastic parka and enters the odor free environment of the olfactorium.

(*g*) *Vision*. On first inspection of the laboratory, vision seems to have been neglected—due probably to the fact that no rooms are specifically designated as 'visual.' This impression, however, is grossly in error as more space is available for visual experiment and research than for any other modality of sense. Every dark-room is potentially a visual research room. The anechoic and soundproof rooms are darkrooms as are the constant temperature rooms and two of the rooms in the Comparative Laboratory. The room devoted to smell and taste may also be made dark by drawing its light-proof shades. Two rooms in the undergraduate laboratory are, moreover, especially equipped for visual work, and five rooms in the graduate laboratory are dark rooms and all the others in it may be made dark by placing shades over the windows. In view of these possibilities for visual work, it seemed both unnecessary and wasteful of space to set aside a room and to label it specifically for vision.

*Batts Auditorium*. Mezes Hall and Batts Hall, the building to the north, are connected by a large auditorium. Fig. 1 shows the front half of Mezes Hall and the end of the Auditorium abutting it. This auditorium, named Batts Auditorium, was designed as a little theater but it was also equipped for large lectures. It seats about 450. The seats, which are comfortable and well upholstered, are equipped with removable tablet-arms which fold back and out of the way when not in use. The stage (see Fig. 2) is large and well illuminated. It has a large projection screen which can be exposed or covered automatically from the projection booth at the rear of the auditorium. The projection booth is equipped with projectors—16-mm., 32-mm., and lantern-slide—of the most modern design. The floor of the auditorium slopes gently from the main entrance or lobby, which is on the ground floor of Batts Hall at approximately the level of the first floor of Mezes Hall, down to the stage, to about the level of the ground floor of Mezes.

Psychology uses this auditorium for its larger elementary classes. The demonstrations, with which its lectures are richly illustrated, are set up on tables in the demonstrational laboratory adjoining, wheeled through the connecting doors, and brought to the level of the stage by the elevator.

The acoustics of the auditorium are excellent. A loudspeaker system, which was about to be installed, was found unnecessary since a speaker can be heard distinctly from any point within the auditorium without raising his voice. Indeed, he need speak, before a full or empty auditorium, with no greater intensity than he would use in speaking to a class of 15-20 students.

The second floor of the auditorium contains three classrooms and nine offices.



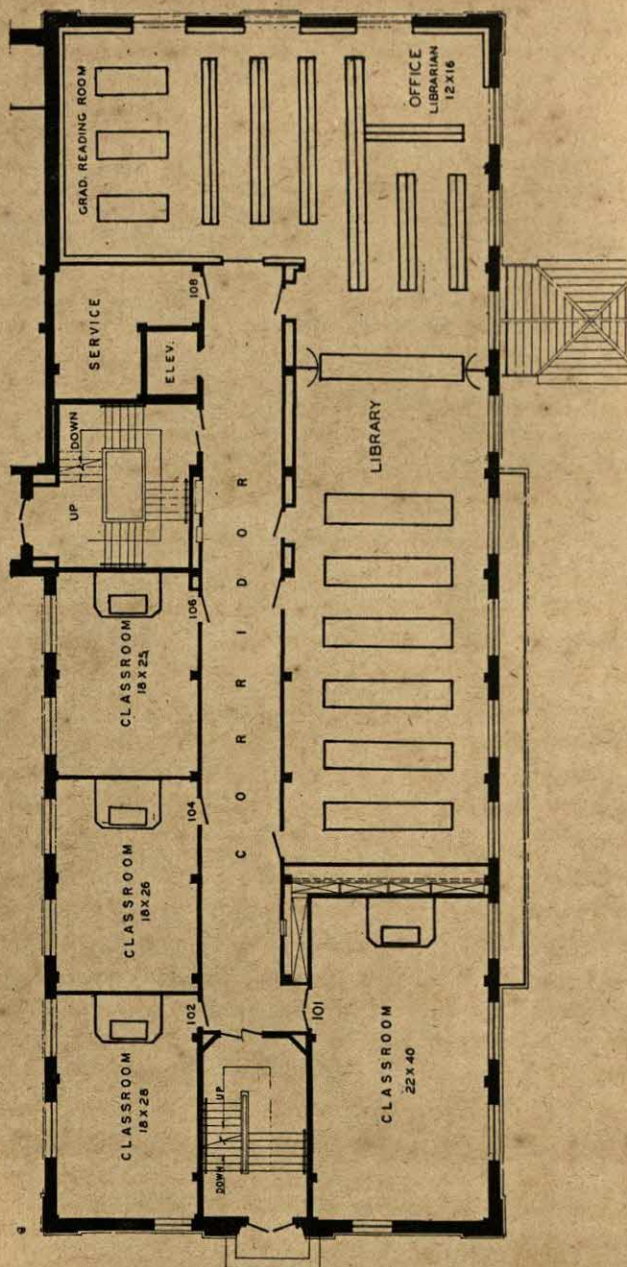


FIG. 3. PLAN OF FIRST FLOOR

Along the length of the auditorium, facing the court between the two buildings, there is a double-deck arcade connecting the ground and first floors of Batts Hall, respectively, with the first and second floors of Mezes Hall.

*First floor.* The principal entrance to the building is on the first floor at the end which is at the ground level of the Mall (see Fig. 3). This floor contains (a) classrooms and (b) the Library of Education and Psychology. The corridor on this floor is broad (10 ft.) since traffic through it is heavy.

(a) *Classrooms.* There are four classrooms on the first floor; one (Room 101) is large, holding 88 students; and three (102, 104, and 106) are small, holding from 38-42 students. The chairs in these rooms are of the pedestal-type. They have broad tablet-arms, 14 in. wide, with 6-10% of them being placed on the left side for left-handed students. They are spaced in rows, 46 in. back to back, and in files, 26 in. center to center. The 'blackboards' within the rooms are green as are all of them throughout the building. Every room has a projection-screen, 3 x 4 ft., which is permanently placed at an angle on the wall behind the instructor that lantern-slides may be projected by him during his lecture and from his desk.

(b) *Library.* The library, which is supervised by a trained librarian and a staff of seven assistants, has two reading rooms (one large and the other small) and stacks which have approximately 2500 ft. of shelving (see Fig. 3). The large reading room is for undergraduates and casual users, and the small room within the stacks is for graduates and others engaged in research. Admission to the inner reading room is by card. Admission cards, good for one year, are issued by the librarian upon recommendation of a member of the teaching staff.

The library contains complete files of all the psychological periodicals in English, German, and French, and runs of some journals in other languages, and all the text- and hand-books that are apt to be called for. Rare books and periodicals and those in cognate fields are borrowed, as occasion and need demand, from the main and other departmental libraries on the Campus or, through library exchange, from libraries of other universities and institutions.

*Second floor.* The second floor contains (a) offices, (b) the elementary laboratory, and (c) a seminary room (see Fig. 4).

(a) *Offices.* There are 11 offices on the second floor—10 open from the main corridor and 1 from the hall of the laboratory. The offices of the Departmental Chairman (Rooms 211 and 211A) are at the far end of the floor, near the elevator and the side entrances. To the left of this suite is the Department's store and supply room (212) which serves also as a mimeographing room. Adjoining the administrative suite, to the south, are the Editorial and Business Offices of this JOURNAL (Rooms 209 and 209A).

The rooms along the main corridor are divided into two suites of four; three staff offices with a central room, which serves as a waiting room and secretarial office. Besides these offices there are two on the south side of the building (Rooms 203 and 207) that open from the branches of the corridor.

The corridors on this floor were made narrow (5 ft. wide) that the space saved



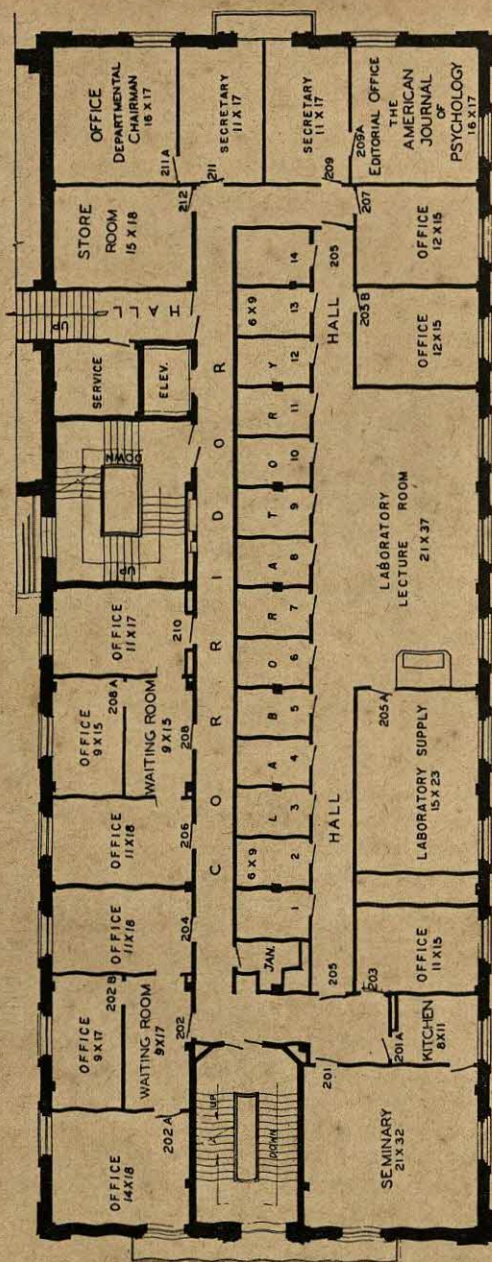


FIG. 4. PLAN OF SECOND FLOOR

could be added to the elementary laboratory. This construction was permissible since the doors to the offices were made to open in and traffic through the corridor was calculated to be light—there being never more than 28 students (the capacity of the laboratory) and a few casual visitors on the floor at one and the same time.

(b) *Laboratory.* The laboratory (Room 205) has entrances at each end of the floor. It contains 17 rooms: an office (205B) for the instructor in charge, a large lecture room, an apparatus room (205A) and 14 small (6 x 9 ft.) experimental rooms.

The experimental rooms (Rooms 1-14) are each furnished with a table, two chairs, and double, steel lockers. The upper parts of the doors to these rooms are glazed that the instructor may, without entering the rooms, observe the students working within them.

The laboratory students, who work in pairs, draw the apparatus and supplies needed for the particular experiment they are undertaking from the laboratory supply room (205A), and retire to one of the experimental rooms to perform it. Two of these small laboratory rooms (Rooms 1 and 2) are equipped for special work in vision. Their ceilings are completely filled with 12 pairs of 40-w. fluorescent tubes which may be turned on singly or in any combination.

(c) *Seminary.* The seminary room (Room 201) is furnished with two large tables, 40 well upholstered chairs, and book cases which extend around the room and cover all the walls except the north one, which carries a blackboard. The book shelves contain *The American Journal of Psychology's* exchanges and the Department's collection of offprints which are alphabetized by author.

A well-appointed, modern kitchen (Room 201A) with electric range and refrigerator, and amply supplied with dishes, flatware, and kitchen utensils, adjoins the seminary.

(d) *Auxiliary classrooms.* A hall opens from the corridor of the second floor, near the elevator, up a half-story flight of stairs to three classrooms (large, medium, and small) above the auditorium. These rooms may be shared by psychology when and as the need and occasion arise.

*Third floor.* The third floor contains (a) offices, (b) the clinical laboratory, two class rooms, a seminary, an experimental room and rest rooms for men and for women (see Fig. 5).

(a) *Offices.* There are eight offices on the third floor, only one of which is used at present by psychology. This office (Room 313) is occupied by the instructor in charge of the clinical laboratories that he may be near them. The other offices on this floor are occupied by the Department of Philosophy.

When the building was being planned, the Department of Psychology was too small to lay claim to all the space within it. Though the Department's growth since then has been rapid, it was not enough to fill the entire building. Sharing the space with another Department was, therefore, indicated. (This is one building at least that was not outgrown by the Department for which it was planned during the process of its construction!) Since the building had to be shared, Philosophy was selected, from among the Departments of a size to occupy the free space, as the other occupant of this floor. This choice was made for historical reasons. Psychology and Philosophy were at one time in the history of the University conjoined. More-







over, Dr. Sidney Edward Mezes, after whom the building was named, was formerly Professor of Philosophy and Chairman of the Department.<sup>2</sup> Philosophy's occupancy of these rooms can, however, be regarded as only temporary, as the time is coming, surely within the next 10-12 yr., when Psychology will be pressed for space and a just claim can be made for all the rooms within the building. At present, however, the space is ample and the two Departments occupy this floor jointly in an amicable relationship.

The plan of the offices along the main corridor is similar to that of the second floor: two suites of three offices with a central waiting room. The seventh office used by Philosophy (303) is on the corridor next to the Seminary (301) which is used solely by Philosophy. The experimental room (311) at the opposite end of the building is also assigned to Philosophy, being at present used as a study room for graduate students.

(b) *Clinical laboratory.* The clinical laboratory contains 15 rooms: the office of the instructor in charge, mentioned above; a small class room (305), seating 25 students; a suite of two rooms (307A and 307B) for the observation and study of children; a waiting room (307C) with a secretary-receptionist in charge; a demonstrational testing suite (307D and 307E); a suite of seven individual testing rooms (1-7); and a storage room (309) for test-blanks and other equipment and supplies.

The suite devoted to the study of children (307A and 307B) is divided by a one-way screen. The children's room of this suite has a lavatory in one corner and a green blackboard upon one wall, both being placed at heights appropriate for children. It is furnished with children's furniture (chairs and a table), with a sand-box, and with cases containing children's books, games, toys and other play things. The one-way glass screen, through which the children are observed, is protected from the children's side by hardware cloth. The viewing room, which is darkened by means of a venetian blind and a black, roller shade, is furnished with a large table and chairs like a seminary room—for which it is also used. Persons seated here are able, by means of a loudspeaker attached to a microphone in the children's room, to hear as well as to see the subjects being observed.

The demonstrational testing-suite is similarly divided by a one-way screen into a viewing and a testing room. The viewing room, which is a darkroom, has an elevated floor with four tiers of seats. Every seat gives a clear and unobstructed view of the room in which the testing is done. The testing room (8 x 8 ft.) contains a table and two chairs. The instructor gives a test to the subject in this room, showing the members of the class concealed in the viewing room the proper method of administering it. Since the subject is not aware of the group behind the mirror (*i.e.* the one-way screen) which he sees in his room, his performances are not affected by the embarrassing knowledge that he is being tested before a group, hence the demonstration of the method of giving the test proceeds in a normal and natural fashion.

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<sup>2</sup> Dr. Mezes joined the faculty of the University of Texas in 1894 as Adjunct Professor of Philosophy. He was promoted to Associate Professor in 1897 and to Professor of Philosophy in 1900. In 1902 he became Dean of Faculty, an office he retained until 1908 when he was appointed President of the University. In 1914, he went to the College of the City of New York.



The individual testing rooms, six small (6 x 7 ft.) and one large (7 x 13 ft.), are each equipped with a table, two chairs, and with what appears to be a large mirror upon the rear wall but is in reality a one-way screen. The student and the subject he is testing may consequently be observed by the instructor from the runway behind the one-way screens (see Fig. 5), without either of them being aware that they are under observation. Small openings between runway and the testing room—covered on the room-side by louvers and on the runway-side by small doors—permit the instructor, when he opens one of these doors, to hear as well as to see the work being done within the room and also, as occasion warrants, to converse with and to instruct the students regarding the work they are doing.

The corridor on this floor is 6 ft. wide—1 ft. wider than on the floor below. The increase in width was required because this floor has two class rooms. The corridor is, however, narrower by 4 ft. than the one on the first floor. This width was approved nevertheless because it was sufficient to carry the traffic of this floor and also because the space saved was needed for the individual testing-rooms of the clinical laboratory.

*Fourth floor.* The graduate research laboratory is on the fourth floor. This laboratory consists of 13 rooms: (a) a large apparatus room; (b) three shops; (c) a statistical room; and (d) eight rooms for research (see Fig. 6).

(a) *Apparatus room.* The apparatus room (415) on this floor contains only apparatus used in research. The various pieces in this room are indexed and catalogued and each has its own proper place upon the shelves. Every piece is checked out and in and is kept in good repair by the instrument maker who is in charge of the Department's shops. A liberal appropriation for apparatus has recently been received and new pieces are rapidly being added.

(b) *Shops.* A full-time instrument maker is in charge of the Departmental shops, of which there are three: a students' shop (407), fairly well equipped with smaller tools and machines, in which graduate students may do the simpler work incident to the construction and setting up of their own research apparatus; and two shops, machine and wood, which are used only by the instrument maker. These shops (411 and 413) are well equipped with the larger as well as the smaller tools and with heavy machinery; e.g. milling machine, lathes, drill presses, grinder, band and jig saws, sander, welding equipment, and the like.

(c) *Statistical room.* The Department's statistical machines have been brought together in one room (409). These machines, to discourage theft as well as their 'migration' to staff offices and to other laboratories within the Department, are chained to their tables.<sup>3</sup> This room is never locked. Students as well as staff have

<sup>3</sup> The machines were fastened to their tables by 30-in. chains. One end was securely fastened to the machine and the other end, after passing through a small hole bored in the top of the table, has a padlock inserted in its last link. This method is more secure and flexible than the method of bolting the machines to their tables. It is more secure, unless the bolts are peened and the threads destroyed—in which case it is difficult to remove the machines in case repairs are needed. It is more flexible as the machines may be moved about upon their desks (within the length of their chains) to positions of greater convenience to their users, or be turned on their sides or faces and cleaned and repaired, or if they must be returned to the shop for reconditioning, the locks are removed and the machines made free simply by the turn of a key.



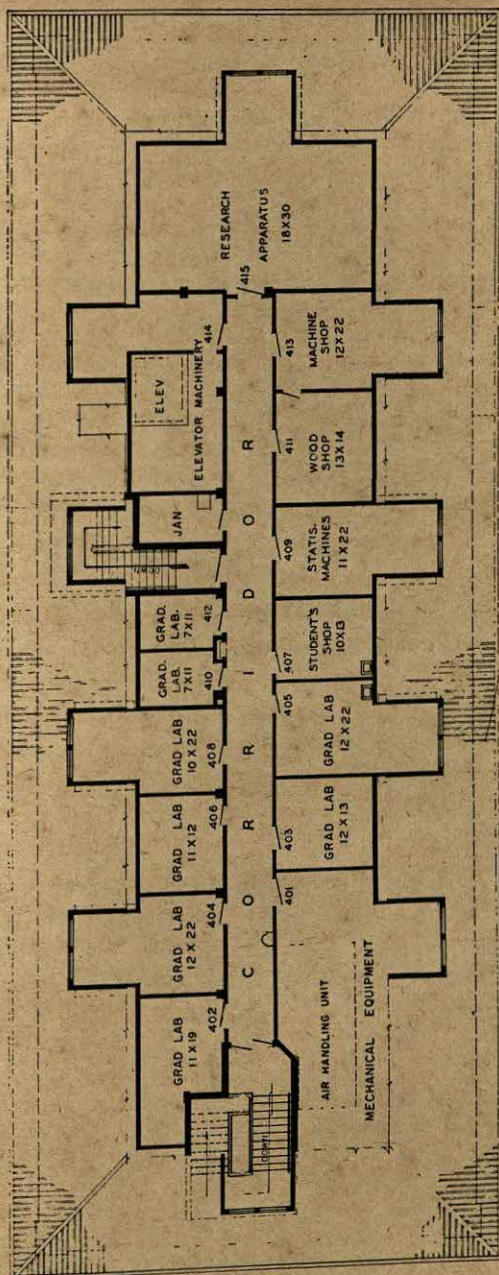


FIG. 6. PLAN OF FOURTH FLOOR



free access to it. It is the busiest room in the laboratory as its machines are used by classes in statistics and psychophysics and by graduate students in the computations involved in the preparation of their dissertations.

(d) *Research rooms.* There are eight rooms for individual research—five small, without windows, and three large, with double windows. Since the windows of the larger rooms may be screened, all these rooms may be used, as necessity dictates, as dark rooms. Every one of these rooms is amply supplied with electrical connections; with double wall plugs of 110-v. AC upon each wall and plugs for 28-v. DC and 400 cycle, 110-v. AC on the board of the interlaboratory wiring system. One of the rooms (405) has a sink supplied with running hot and cold water. These rooms are assigned to the graduate students in accordance with the needs of their researches.

The corridor on this floor is 6 ft. wide—more than sufficient for the traffic, which is light here. It was not made narrower, however, because the space saved by a lesser width, 5 ft. for example, was not needed. The advantage of increasing the depth of the rooms on this floor by 6 in. was regarded as being less than the advantage of having a broad hall through which apparatus is carried back and forth from the apparatus room and the shops to the individual research rooms.

Mezes Hall is a composite of all the psychological laboratories that have been built in the past. Its good features were borrowed freely from other laboratories. Its poor features, which will doubtless show themselves in time, must be charged not only to our lack of experience in designing a building for psychology but also to the lack of experience within the *Fach*. Although Psychology has had ample experience in remodeling attics and basements from which other Departments had escaped, it has had all too little experience in planning a building from the ground up. That experience, however, will eventually come—Psychology's 'stepmotherly' days are over. Psychology is getting out of the attics and basements and is receiving, by right of its own solid accomplishment, adequate facilities for its work. We may confidently expect, therefore, that new buildings, better and bigger, will follow in the wake of this one, and that eventually the psychological building will be as commonplace on our campuses as the buildings for physics, chemistry, and biology.

## A SEMI-AUTOMATIC BRIGHT-FIELD TACHISTOSCOPE

By F. D. KLOPPER, State College of Washington

Many investigations in perception and learning require brief exposures of serial material. Although a variety of apparatus has been developed for the presentation of this material, none has been entirely satisfactory. Most of the difficulties are well known—the problems of group versus individual presentation, controlled pre- and post-exposure fields, ready period, interval between exposures, steepness of exposure, and cut-off gradients—as they were described and discussed by Wundt,<sup>1</sup> Dodge,<sup>2</sup> and Whipple<sup>3</sup> before 1910. The recommendations of these authors, together with those of Twitmyer and Fernberger,<sup>4</sup> Newhall,<sup>5</sup> Vernon,<sup>6</sup> and Gullikson,<sup>7</sup> were included as characteristics of the apparatus described here.

The tachistoscope described here was designed for use in a study of perception, but it may be adapted to a wide variety of uses. Its essential characteristics are as follows:

(1) Exposure-time may be varied through a range of 5-1000 m.sec. Extension of this range above 1000 m.sec. is possible through a change of gears.

(2) Cut-off is complete in 1.9 m.sec., thus contributing essentially to the accuracy of the exposure-time.

(3) The accuracy of the exposure-time, photographically determined at 80, 400, and 1000 m.sec. is  $\pm 1.7$  m.sec.

(4) Time from the beginning of one exposure to the beginning of the next may be altered in multiples of 2.5 sec.

(5) The brightness of the field before, during, and after the exposure may be varied independently.

(6) The brightness of the field before and after exposure may be equated to that during exposure. Thus the stimulus-patterns appear and disappear on a field of constant illumination.

(7) It is possible to present stimulus-patterns in the pre- and post-exposure fields.

(8) A ready signal may be sounded at any fixed time before each exposure, but at any setting is constant for all exposures.

(9) Standard  $2 \times 2$  in. photographic slides may be shown with a projection-angle of  $4^\circ$ .

<sup>1</sup> Wilhelm Wundt, *Human and Animal Psychology*, Lecture 16, 1901, 239-244.

<sup>2</sup> Raymond Dodge, An experimental study of visual fixation, *Psychol. Monog.*, 8, 1907, 32-50.

<sup>3</sup> G. M. Whipple, *Manual of Mental and Physical Tests*, 1910, 263-264.

<sup>4</sup> E. B. Twitmyer and S. W. Fernberger, The Twitmyer demonstration tachistoscope, this JOURNAL, 38, 1927, 113-119.

<sup>5</sup> S. M. Newhall, Projection tachistoscopes, this JOURNAL, 48, 1936, 501-504.

<sup>6</sup> M. D. Vernon, *Visual Perception*, 1937, 217-227.

<sup>7</sup> H. Gulliksen, A new form of tachistoscope, *J. Gen. Psychol.*, 6, 1932, 223-226.



(10) The slide-holder will accommodate 12 slides—the maximal length of a series.

(11) Additional disks may be used to extend the length of an exposure-series. Disks can be changed in 15 sec.

(12) The order of the slides may be varied or kept constant through any number of trials.

(13) A new slide is automatically brought into position after every exposure.

(14) The noise-level of the apparatus is relatively constant, and the exposure is not accompanied by a noise.

(15) The mechanism automatically shuts off after each series.

The tachistoscope consists essentially of two fan-cooled projectors,

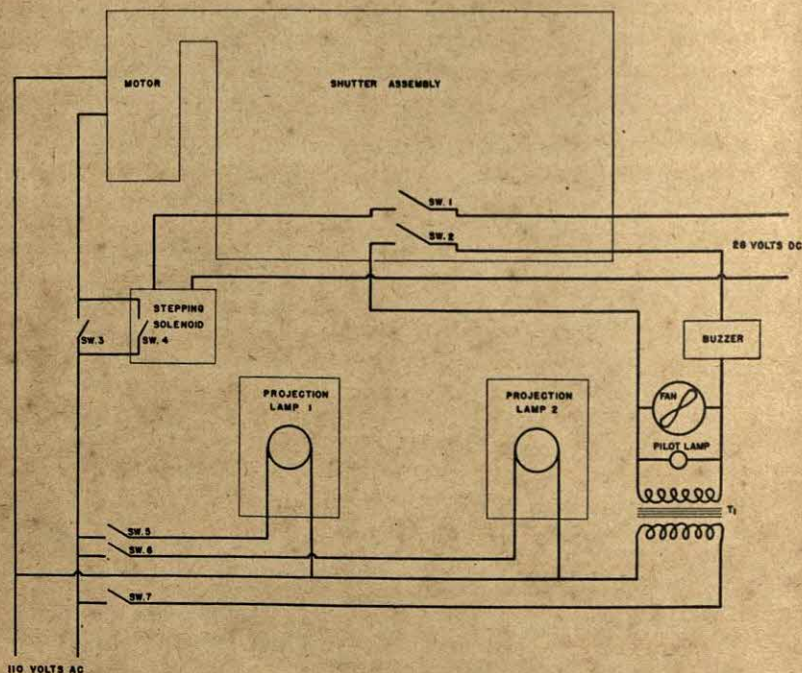


FIG. 1. WIRING DIAGRAM OF THE APPARATUS

a shutter mechanism, and a slide-changer. The interrelations of these parts are indicated in the wiring diagram (Fig. 1). Toggle switches (Sw 5, Sw 6, Sw 7) close the 110-v., AC circuits to the projection lamps and to the transformer supplying 6-v., AC to the fan, pilot lamp, and buzzer circuit. A push button (Sw 3) starts the motor which actuates the shutter assembly. A cam in this assembly momentarily closes microswitch (Sw 2) in the buzzer signal circuit. Following the exposure another cam momentarily

closes microswitch (Sw 1) in the 28-v., DC slide changer circuit, thus moving a new slide into position, which motion in turn closes another microswitch (Sw 4) in the motor circuit. The cam actuating this switch is mechanically linked to the stepping solenoid, which breaks the motor circuit after one complete revolution, or 12 steps, of the stepping mechanism.

The shutter assembly (Fig. 2) is a simple rotating disk and vane mechanism with mechanical linkage. Shutters 1 and 2, (Sh 1, Sh 2) are

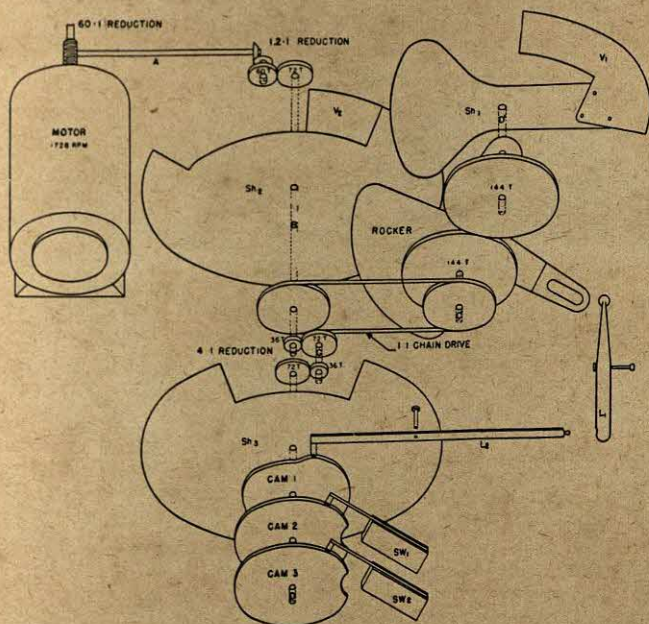


FIG. 2. SHUTTER ASSEMBLY OF THE APPARATUS

driven at 24 r.p.m. Shutter 1 intercepts light from Projector 2 during the interval in which the open sector of Shutter 2 permits light from Projector 1 to illuminate the screen.

The center of rotation of Shutter 1 is on a rocker that rotates on shaft C. When the rocker is moved in a clockwise direction the vane (V 1) on Shutter 1 intercepts the light from Projector 2. When the rocker is moved in a counterclockwise direction, the shutter continues to rotate but is no longer in the path of the projected beam. Projector 1 will illuminate the screen only when the projected beam and the open sectors of both Shutters 2 and 3 coincide. The time between these exposures is controlled by the reduction ratio between shafts B and D. The 4:1 reduc-



tion indicated in Fig. 2 permits an exposure every 10 sec. Cam 1 actuates the lever system (L 1, L 2) and rotates the rocker in a clockwise direction at the same time the open sector of Shutter 3 moves into position in front of Projector 1. Thus every 10 sec., and for a duration determined by the size of the vanes (V 1 and V 2) on Shutters 1 and 2, light from Lamp 1 alternates with light from Lamp 2 in illuminating the screen.

The brightness of the screen may be varied in four ways: (1) by the wattage of the lamps; (2) by the size of the diaphragm opening at the

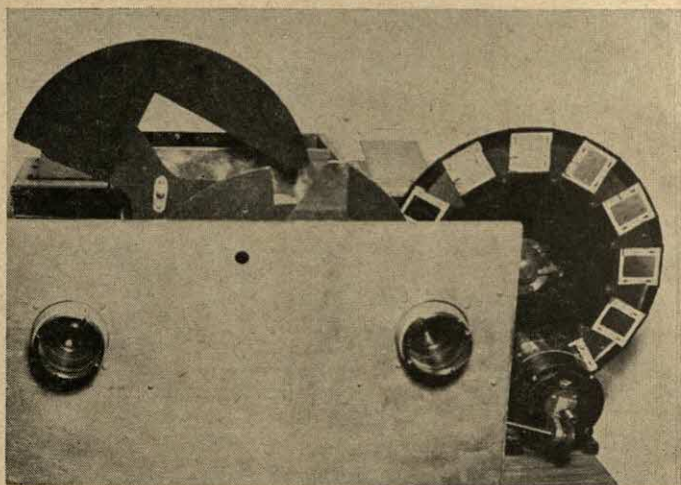


FIG. 3. FRONT VIEW OF THE TACHISTOSCOPE

focus of each projector;<sup>8</sup> (3) by displacing the filament-image at the opening of the diaphragm; and (4) by neutral density-filters in front of either or both projectors.

The slide-holder is a disk of 3/16-in. bakelite, 12 in. in diameter. Twelve slides, equally spaced around the perimeter, are held in sockets by spring clips. The disk is attached to the stepping mechanism by a simple rotating release assembly that locks or releases the disk with a 60° turn.

The lamp assembly, shutter assembly, and front panel with f. 1.9 projection-lenses are mounted on a 15 x 21 x 3/4 in. plywood base which is supported on 1 1/2 in. tubular aluminum legs. A shelf is provided for

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<sup>8</sup> This will alter, however, the slope of the cut-off gradient for every exposure. This cut-off, as reported in the list of characteristics, was determined with a diaphragm opening of 5/64 in. diameter.

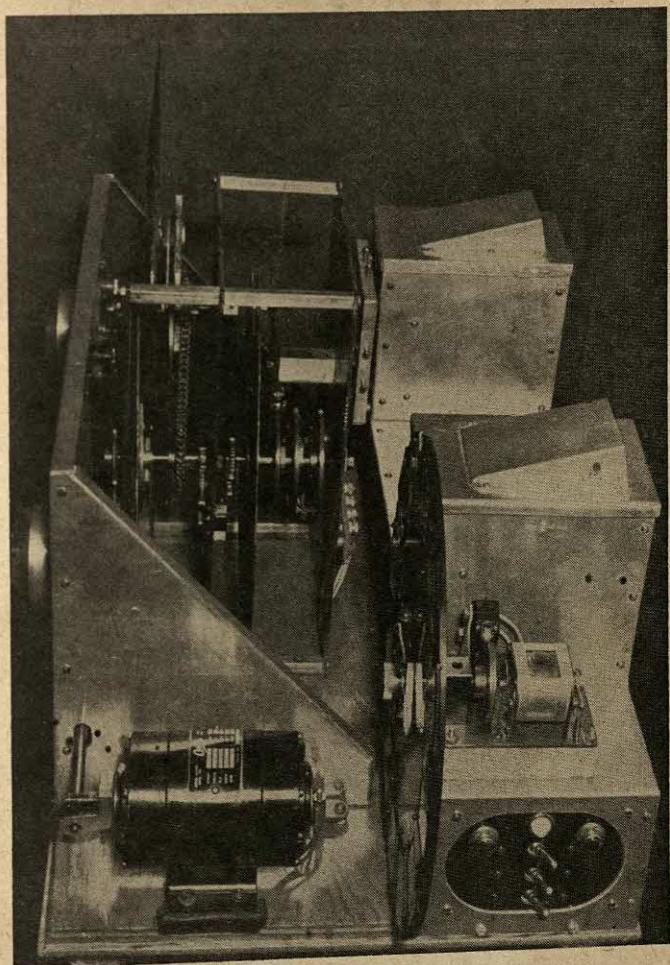


FIG. 4. SIDE VIEW OF THE TACHISTOSCOPE

auxiliary slide-holders, and a combined power supply and voltage regulator unit. Figs. 3 and 4 show front and side views of the tachistoscope.



## AN IMPROVED ELECTRONIC TACHISTOSCOPE

By JOHN G. MERRYMAN and HOWARD E. ALLEN,  
The Johns Hopkins University

The apparatus described here is a modification of the electronic tachistoscope reported by Kupperian and Golin.<sup>1</sup> The objective is to retain the

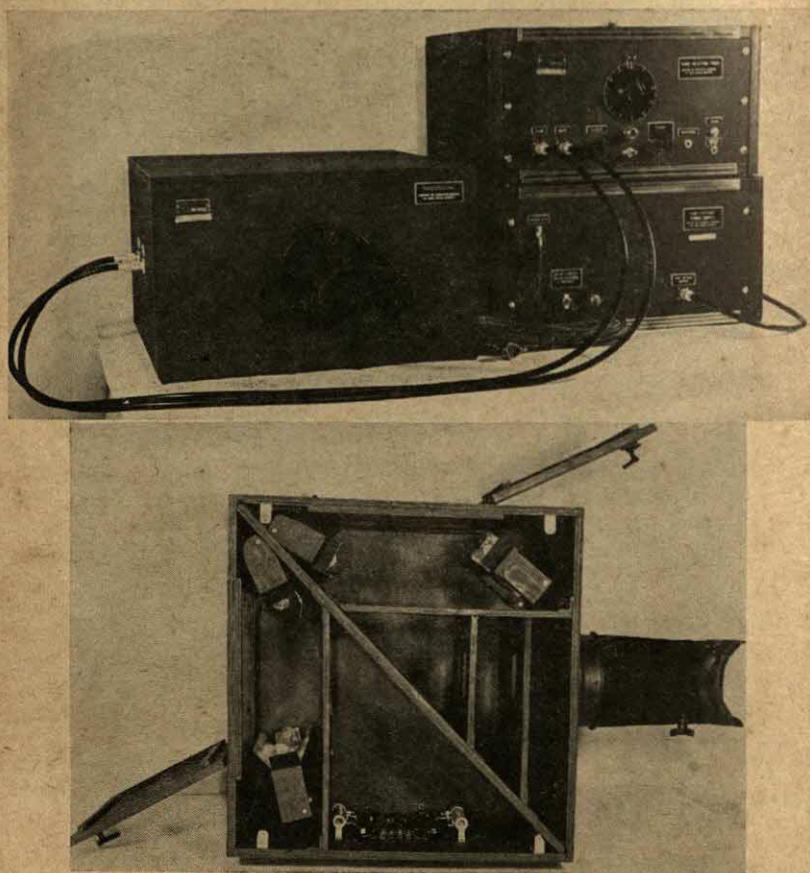


FIG. 1. THE ELECTRONIC TACHISTOSCOPE AND ACCESSORY EQUIPMENT

\* This research was carried out under Contract N5-ori-166, Task Order I, between the Systems Coordination Division, Naval Research Laboratory, Office of Naval Research, and the Engineering Laboratory, Institute for Coöperative Research, The

desirable features of their apparatus but, at the same time, to use components which are simpler and more readily available, incidentally resulting in an appreciable reduction in cost.

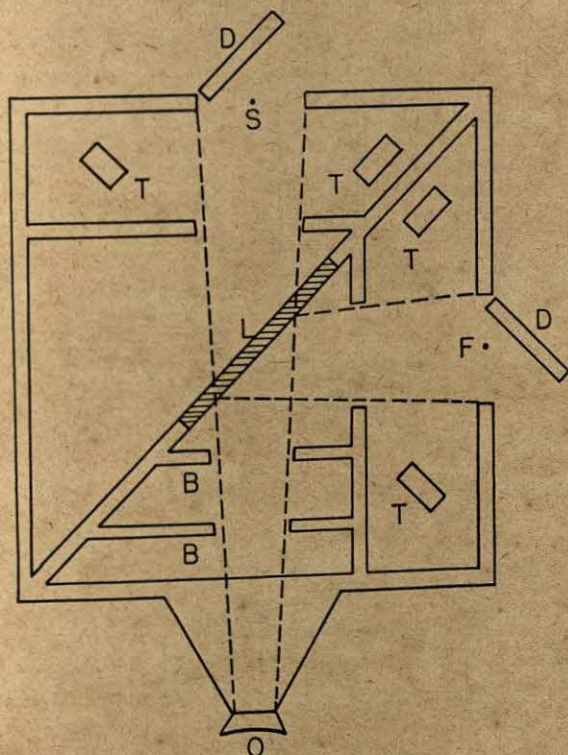


FIG. 2. PLAN OF THE TACHISTOSCOPE

T = Mercury argon tubes  
B = Baffles  
L = Lucite plate

O = Observer's face-piece  
F = Fixation-point  
S = stimulus-material

D = Hinged doors with card-holders

*Specifications.* The following requirements were considered as controlling factors in the design.

(1) A source of illumination should be provided which has the rectilinear pulse characteristics of helium-filled tubes but has superior spectral qualities for visual purposes.

Johns Hopkins University. This is Report No. 166-I-151, Project Designation No. NR 507-470, under that contract.

<sup>1</sup>J. E. Kupperian, Jr., and Edwin Golin, An electronic tachistoscope, this JOURNAL, 64, 1951, 274-276.



(2) This source should operate from a supply voltage lower than the 4000 v. required for helium-filled tubes.

(3) Exposure-times should be controllable to values as low as 10 m.sec.

*Tachistoscope.* A plan-view of the tachistoscope, a modification of Dodge's,<sup>2</sup> is shown, with the top removed, in Fig. 2. The interior dimensions are  $20 \times 20 \times 10$  in., and the material used is  $\frac{3}{8}$ -in. plywood with the inside painted a dull black to reduce the reflectance. A 10-in. height was chosen that the stimulus-holder would accommodate an  $8 \times 10$ -in. card. This holder is attached to the hinged door (D) to provide accessibility and convenience in changing cards. The viewing-aperture is

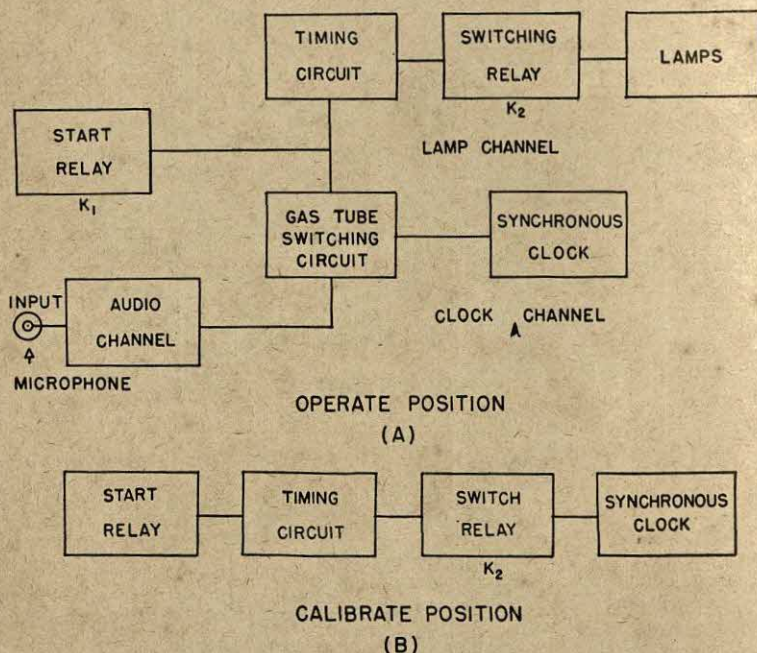


FIG. 3. BLACK DIAGRAMS OF TIMER AND SWITCHING CIRCUITS

fitted with a radar-screen light-shield (O) having a flexible face-piece such that outside light is excluded, even though the O may be wearing glasses.

Two compartments are formed by a diagonal partition and a system of baffles. These baffles reduce the possibility of interference by stray light and help to concentrate and guide O's vision. Two lamps (T) are placed in each compartment to produce an even illumination of the stimulus-material. The fixation-point is at (F) and the stimulus-object is placed at (S).

The reflecting-transmitting surface (L) is mounted in the diagonal partition—

<sup>2</sup>Raymond Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10-13.

$\frac{1}{8}$ -in. clear lucite being chosen for this function. This material is more desirable than a half-silvered mirror because a lower light intensity can be used to obtain adequate illumination of the stimulus-object. Since the stimulus-compartment is normally dark and the fixation-compartment illuminated, the lucite appears to be a mirror, reflecting the light to *O*. When the fixation-compartment is dark and the stimulus-compartment lighted, the lucite becomes so transparent that *O* views the stimulus-object for a predetermined time-interval. Because of the inequality of the coefficients of reflection and transmission, the illumination of the fixation-point is of lower intensity than that of the stimulus-field. This effect, however, has not been

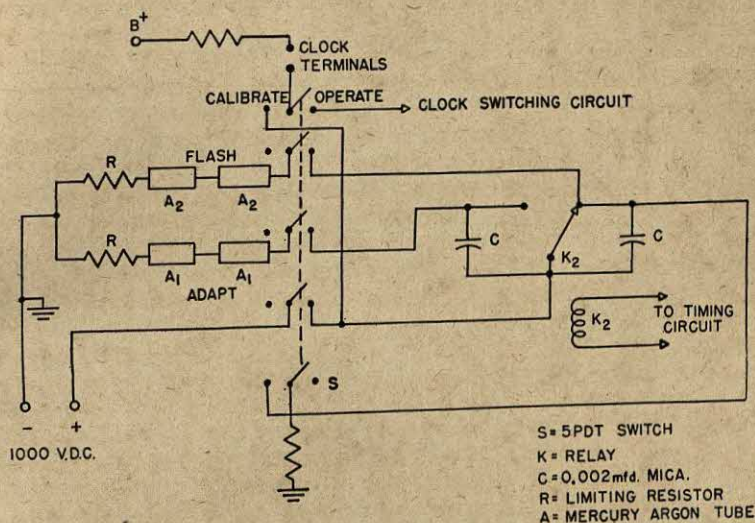


FIG. 4. WIRING DIAGRAM OF SWITCHING CIRCUITS

S = 5PDT switch  
K = Relay

C = 0.002mfd. Mica.  
R = Limiting resistor

A = Mercury-argon tube

found to be serious enough to justify the unbalance of the intensities of the light sources.

*Light source.* Clear tubes filled with a mixture of mercury and argon gas were found to meet our specifications in that (1) a 1000-v. low current supply is sufficient for two tubes connected in series, (2) the resulting light pulses are approximately rectilinear, and (3) the light produced is in the blue region of the spectrum, which for purposes of illumination is superior to that produced by helium-filled tubes. While a white light source would be preferable to a blue one, this can only be readily obtained from incandescent or fluorescent-coated lamps at the required intensity-levels. Neither of these sources can produce the required rectilinear light pulses, since the incandescent lamp has a warming and cooling time noticeable at 50 m.sec. or less, and the fluorescent tube has a persistent afterglow.



The mercury-argon lamps selected for this application are of the type used in advertising signs and are thus readily obtainable. They are 8 in. long with 2-in. electrodes and are 'C'-shaped. The glass tubing used for this purpose is impregnated with lead to reduce the danger from ultraviolet radiation. The series-connected lamps of each compartment impose a current drain of only 40 m.amp. upon the 1000-v. DC source. Since only one compartment is lighted at a time, this is the total current requirement, appreciably simplifying the design of the voltage source.

*Timing control and switching circuits.* A voice reaction-timer was modified to supply all timing and switching controls for both the externally connected synchronous clock and for the mercury-argon lamps.<sup>3</sup> A block diagram of the circuit is shown in Fig. 3 A. The timing circuit consists of a monostable multivibrator. Four switched capacitors in conjunction with a potentiometer provide flash-durations continuously variable from approximately 2 m.sec. to over 4 sec. When the 'Start' relay is actuated by the operator, voltages are simultaneously applied to the lamp channel and to the clock channel. Thus, the clock measures elapsed time from the instant that the stimulus-light is turned on and the adaptation-light goes off. The stimulus-compartment remains lighted for the period determined by the setting of the timer, at the end of which period the lamps recycle, the stimulus-light going off and the adaptation-light coming back on. The clock, however, continues to run until the *O* speaks into the microphone of the audio channel, the output of which is applied to the control tube of the clock, causing it to stop, thus measuring *O*'s reaction-time. The timer dial is so calibrated that the desired stimulus-exposure can be obtained; it is possible to check these calibrations directly by the clock by switching to the 'Calibrate' position (Fig. 3 B). The clock then permits a direct measurement of the timing pulse-duration to be made.

*Wiring diagram.* A wiring diagram of the 'Calibrate-Operate' wafer switch and of the lamp switching relay is shown in Fig. 4. The use of a 1000-v. DC supply makes it practicable to use a fast acting, single-pole, double-throw relay as the lamp-switching element, thus eliminating the electronic switch which is more suitable when 4000-v. lamps are used. Standard techniques are employed to reduce contact arcing to a negligible degree. An oscilloscope showed the light pulse to be essentially rectilinear and of constant amplitude over the full range of flash durations.

### SUMMARY

The tachistoscope described here employs an electronic method of controlling the duration of a light pulse to give a brief view of a stimulus-object. Mercury-argon tubular lamps are used as the light source. These lamps give essentially a rectilinear pulse of light, the duration of which is controlled by a modified voice reaction-timer. A 1000-v. DC source is used as the firing voltage of the lamps. This permits the use of a simple single-pole, double-throw relay as the switching element. Durations of the light pulse of 2 m.sec. have been obtained. A piece of  $\frac{1}{8}$ -in. lucite was used in preference to half-silvered mirrors as the reflecting-transmitting surface.

<sup>3</sup>R. G. Roush and Ferdinand Hamburger, Jr., An electronic chronograph for measurement of voice reaction-time, this JOURNAL, 60, 1947, 624-628.



## APPARATUS FOR AUDITORY MASKING

By EDWARD H. GREEN, Brooklyn College, N.Y.

Brooklyn College has been conducting for several years an integrated course in Science, with illustrative material derived not only from lecture demonstrations, but also from weekly two-hour laboratory exercises. Sensation is one of the subjects treated in the later stages of this course, and in this connection it was felt that a laboratory exercise in auditory masking would not only be a good illustration of the techniques employed in this field, but would have the important advantage that the explanation of the observed phenomena could be put in terms of theories of hearing. An obvious disadvantage, however, was that commercially available equipment for this purpose would be not only bulky and of intimidating complexity, but would also be prohibitively expensive, especially since it was intended that ten such laboratory setups would be in use simultaneously. In designing the apparatus described herein, the author has aimed, therefore, at low cost, small physical volume, and simplicity of controls.

By employing only such component parts as are commonly used in radio receivers, the cost was kept as low as \$25 per unit. Although the cabinet is small, the interior of the unit is not crowded; in fact, the size of the unit is dictated principally by the requirement of easy visibility of the calibration scales.

Referring to the wiring diagram, Fig. 1, it will be seen that one section ( $T_1$ ) of a 6SL7 twin triode tube and one section ( $T_2$ ) of a 6SN7 twin triode tube are employed as a variable-frequency oscillator of the Wien-bridge type. This is designed to cover the usable range of the headphones (about 350 to 4000 ~ per sec.). The remaining half ( $T_3$ ) of the 6SL7 is used as a phase shift oscillator, to generate a tone of a fixed frequency (about 800 ~ per sec.). The remaining half ( $T_4$ ) of the 6SN7 is used as an output amplifier. Besides its more obvious purpose of supplying adequate tone power, it serves to make the calibration of the attenuators,  $R_9$  and  $R_{10}$ , independent of the characteristics of the headphones, which, therefore, need not be permanently attached to the cabinet.

No particular power transformer or rectifier tube is specified, because  $R_{12}$ , which is part of the power supply filter, can be chosen by trial to provide the stated 175 v.

The following steps are employed in calibrating the apparatus.

(a) An oscilloscope is connected across the headphone jack J; switch S is in the 'open' position;  $R_5$  is so adjusted that a good sinusoidal waveform is obtained at all settings of the ganged frequency control,  $R_1$ .

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\* From the Department of Physics. Professor David Raab of the Department of Psychology of Brooklyn College, furnished valuable advice and assistance in the design of this apparatus.



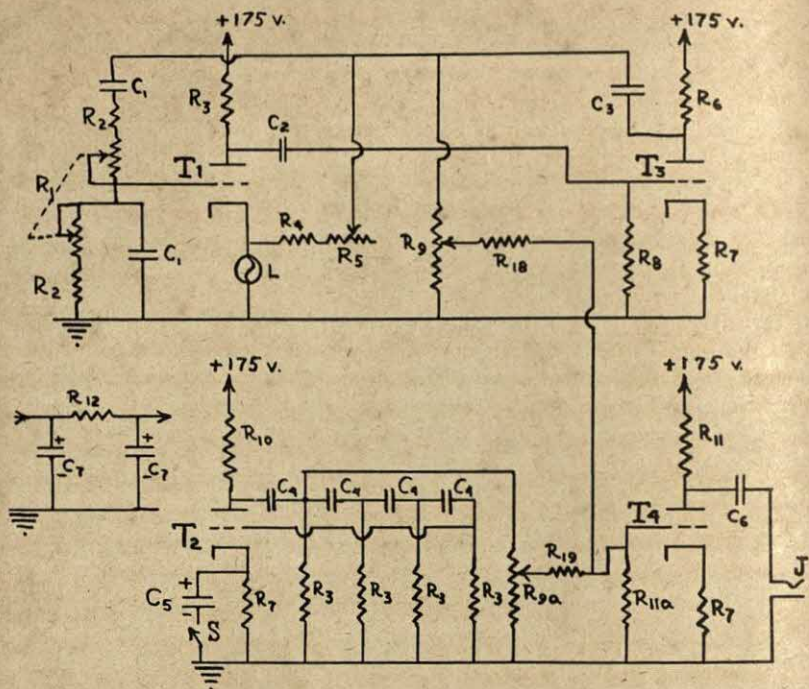


FIG. 1. WIRING DIAGRAM OF APPARATUS FOR AUDITORY MASKING

$T_1, T_2$	= 6SL7	$R_{12}$	= 25,000 ohm, approx.
$T_3, T_4$	= 6SN7	$R_{18}, R_{19}$	= 4 megohm
$R_1$	= Ganged 100,000 ohm potentiometers	$C_1$	= 5000 mmfd.
$R_2$	= 7000 ohm	$C_2$	= 0.01 mfd.
$R_3, R_{10}$	= 100,000 ohm	$C_3$	= 1 mfd.
$R_4$	= 2000 ohm	$C_4$	= 1500 mmfd.
$R_5$	= 1000 ohm potentiometer, inside cabinet	$C_5$	= 10 mfd., 25 v. electrolytic
$R_6$	= 20,000 ohm	$C_6$	= 0.1 mfd.
$R_7$	= 500 ohm	$C_7$	= 10 mfd., 450 v. electrolytic
$R_8$	= 1 megohm	H	= headphones, 2000 ohms or higher
$R_9, R_{9a}$	= 1 megohm potentiometer, audio taper	S	= switch, mounted on back of $R_{9a}$ .
$R_{11}, R_{11a}$	= 50,000 ohm	L	= 3 watt, 110 v. Mazda lamp
		J	= headphone jack.

All fixed resistors are half-watt carbon type. The cabinet is Budd No. C994, height 7 in., width 12 in., depth 7.5 in., with Budd chassis No. CB997.

(b) The frequency-dial is calibrated by comparison of the output with the output of a laboratory oscillator or other source of known frequency, using, for example, the method of Lissajous' figures. Our students are instructed to make their observations at frequencies of 350, 450, 600, 1000, 1500 and 3000 ~ per sec.

(c) Switch S is closed, thus setting the fixed-frequency source into oscillation, and the masking frequency is similarly determined. If it is necessary, increasing the value of  $R_{10}$  will give stronger and more stable oscillations.

(d) With S open, ascertain that the attenuator ( $R_a$ ) of the variable-frequency oscillator can be adjusted to cover a range from about 10 db. below threshold to about 40 db. above threshold in the region of 1000 ~ per sec. The scale position of the threshold may be shifted, if necessary, by alteration of  $R_{11a}$  (lower values of which will provide lower signal output). The headphones are then replaced by a voltmeter which carries a decibel scale. Any desired increment in decibels may be located by turning the attenuator knob and observing the readings of the voltmeter. A net range of 50 db. in steps of 5 db. is adequate. This procedure is then repeated for the other attenuator,  $R_{9a}$ . Since the output signal may be as small as 5 m.v., it may be necessary to use an auxiliary amplifier before the voltmeter, unless one has access to a sensitive meter such as the Ballantine Model No. 300. Such an auxiliary amplifier must be stable only for the few minutes required for a calibration.

It is to be expected that inexpensive circuit-elements such as are used here will alter with the passage of time. To simplify recalibration, the front of the cabinet carries a long card over which the control pointers sweep, and on which the calibration marks can be made easily.

The students, working in pairs, first determine threshold settings at the frequencies stated above, with the masking tone source silent. The masking tone threshold is determined next. Then the first determinations are repeated, using masking tones of 30 db. and then 40 db. above threshold. The results are plotted on semi-log paper, and clearly show typical masking contours.



## A STABLE APPARATUS FOR ANALYZING THE AREA OF POLYGRAPHIC CURVES

By C. E. HUMPHREY and JOHN E. THOMPSON,  
The Johns Hopkins University

The problem of analyzing polygraph-recordings has long troubled the experimenter interested in studies involving a recorded, fluctuating curve. Many attempts have been made to find a stable, accurate device which would analyze the polygraph-record in terms of curve-area. The planimeter and crude estimation have resulted in area-approximations which are both tedious and time consuming, while electronic efforts, in general, have not resulted in equipment of great stability. For example, Birmingham devised a rectifying type of integrator which, due to inherent difficulties in rectification circuits, is not as stable as desired over long periods of time.<sup>1</sup> Adjustment of the integrator is also awkward and must be repeated at frequent intervals. The present paper describes a device which has great stability, is relatively inexpensive, and simple in construction. It requires little attention during operation.

*Mode of operation.* The essential feature of the device is a highly stable feedback-amplifier circuit of the type used in analog-computers such as the Boeing-Electronic-Analog-Computer (BEAC).<sup>2</sup> The arrangement used by the writers permits one to record curve-area in either a positive direction, a negative direction, or over total area of the curve. This is done by using two separate integrators, one for positive deflections, the other for negative deflections. These are switched automatically into the circuit according to voltage-direction and sign, since the integrator will normally operate in either direction. As the input-voltage swings about a zero-point, it is so rectified that, as it tends to go negative from the reference-point, it activates a sensitive relay which closes the input-circuit of Integrator A of Fig. 1., while the other integrator remains idle. In the positive direction the reverse occurs, hence one integrator serves to record all positive deviations above the reference-line, while the other structurally identical integrator records negative deviations of voltage.

\* From the Applied Physics Laboratory, Silver Spring, Maryland. This report was prepared under Contract NOrd 7386 between the U.S. Navy, Bureau of Ordnance and The Johns Hopkins University.

<sup>1</sup>H. P. Birmingham, An electronic error integrator, *Naval Research Laboratory Report No. R.*, 3297, 1948.

<sup>2</sup>Maintenance manual, Boeing electronic analog computer, 1949, Boeing Aircraft Corp., Seattle, Washington.



The total curve-area then, both positive and negative, is the sum of the two separate outputs and this sum divided by time-of-integration yields average curve-area.

*Switching circuit.* Fig. 1 is a schematic-diagram of the apparatus used to switch integrators as the input-voltage changes sign. The 6SN7 balanced amplifier is used to amplify the incoming voltage and to isolate the

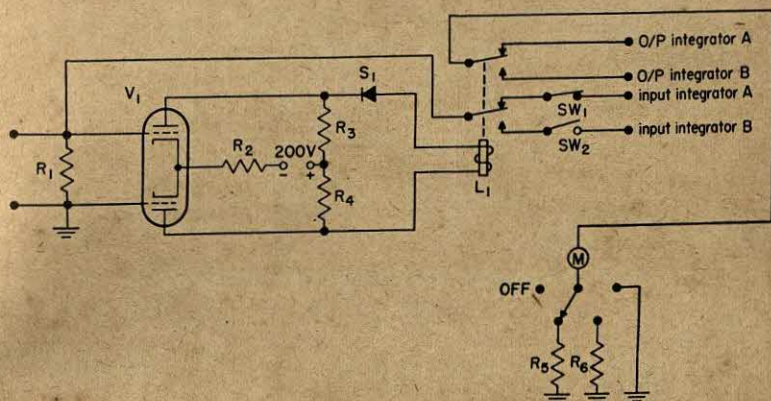


FIG. 1. SCHEMATIC WIRING DIAGRAM OF THE INTEGRATOR SWITCHING CIRCUIT

- $L_1 = 5$  m. amp. relay
- $M = 0-1.5$ -v. D.C. meter
- $R_1, R_2, R_3, R_4 = 50,000$  ohm, 1w. resistors
- $R_5 = 150,000$  ohm precision resistor
- $R_6 = 75,000$  ohm precision resistor
- $S_1 = 75$  m.amp. selenium rectifier
- $SW_1, SW_2 =$  S.P.S.T. switches
- $V_1 = 6SN7$  GT

rectifier-relay-circuit from the input-voltage source. As the signal-voltage goes negative in sign, the phase reversal occurring in the upper amplifier places a positive voltage on the selenium-rectifier. As the rectifier conducts, the relay is closed, placing the input-voltage on the grid of Integrator B. When the input-voltage changes sign, the relay is released, allowing the input to switch to Integrator A.

*Integrator-circuit.* The integrators are identical; consequently the schematic-diagram shown in Fig. 2 is applicable to both integrators. The design is a conventional analog-computer integrating unit, consisting of a three-stage balanced amplifier with a capacitive feed-back-loop. The only critically important component is Condenser  $C_1$ , which must be of a low-leakage type to avoid drift in the system. The writers used a Western



Electric D 161270 capacitor of one microfarad capacity. This condenser has an unusually small amount of leakage.

The input-circuit of Fig. 1 may require minor changes for different

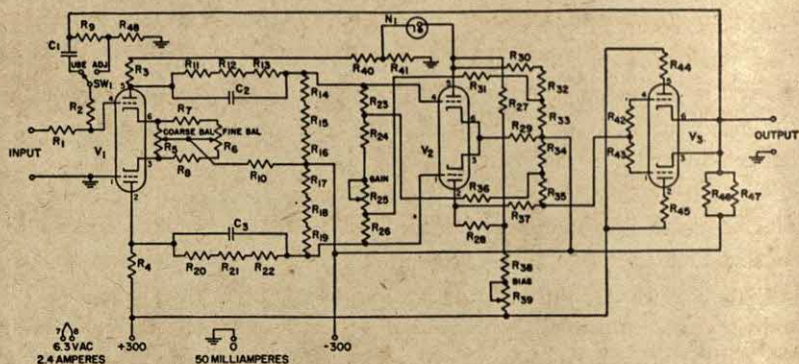


FIG. 2. SCHEMATIC WIRING DIAGRAM OF THE APPARATUS

$V_1 = 5692$	$R_{16} = 510\text{ K}$	$R_{37} = 200\text{ K}$
$V_2 = 6\text{SN7 GT}$	$R_{17} = 510\text{ K}$	$R_{38} = 62\text{ K}$
$V_3 = 6\text{SN7 GT}$	$R_{18} = 510\text{ K}$	$R_{39} = 50\text{ K w.w. PO-}$
$N_1 = \text{NE51 NEON}$	$R_{19} = 510\text{ K}$	$\text{TENTIOMETER}$
$\text{GLOW LAMP}$	$R_{20} = 510\text{ K}$	$R_{40} = 1\text{ MEG}$
* $R_1 = 1\text{ MEG}$	$R_{21} = 510\text{ K}$	$R_{41} = 39\text{ K}$
$R_2 = 5.1\text{ K}$	$R_{22} = 510\text{ K}$	$R_{42} = 5.1\text{ K}$
$R_3 = 200\text{ K}$	$R_{23} = 1\text{ MEG}$	$R_{43} = 5.1\text{ K}$
$R_4 = 200\text{ K}$	$R_{24} = 4.7\text{ MEG}$	$R_{44} = 510\text{ }\Omega$
$R_5 = 500\text{ }\Omega\text{ w.w. PO-}$	$R_{25} = 5\text{ MEG w.w. PO-}$	$R_{45} = 510\text{ }\Omega$
$\text{TENTIOMETER}$	$\text{TENTIOMETER}$	$R_{46} = 82\text{ K}$
$R_6 = 500\text{ }\Omega\text{ w.w. PO-}$	$R_{26} = 1\text{ MEG}$	$R_{47} = 82\text{ K}$
$\text{TENTIOMETER}$	$R_{27} = 200\text{ K}$	$R_{48} = 51\text{ K}$
$R_7 = 2\text{ K}$	$R_{28} = 200\text{ K}$	$C_1 = 1\text{ mfd}\dagger$
$R_8 = 2\text{ K}$	$R_{29} = 150\text{ K}$	$C_2 = 5\mu\text{mf } 600\text{ V}$
$R_9 = 5.1\text{ MEG}$	$R_{30} = 200\text{ K}$	$C_3 = 5\mu\text{mf } 600\text{ V}$
$R_{10} = 150\text{ K w.w. } 1\%$	$R_{31} = 3\text{ MEG}$	$\text{SW}_1 = \text{S.P.D.T. TOGGLE}$
$R_{11} = 510\text{ K}$	$R_{32} = 820\text{ K}$	$\text{SWITCH}$
$R_{12} = 510\text{ K}$	$R_{33} = 1.8\text{ MEG}$	$\dagger \text{ WESTERN ELEC-}$
$R_{13} = 510\text{ K}$	$R_{34} = 1.8\text{ MEG}$	$\text{TRIC D 161270}$
$R_{14} = 510\text{ K}$	$R_{35} = 820\text{ K}$	$1\text{ mfd } 200\text{ V}$
$R_{15} = 510\text{ K}$	$R_{36} = 3\text{ MEG}$	

\* Note: All resistors are 1% precision resistors except where otherwise noted

applications. For example, an input-circuit of insufficient voltage to drive a Brush recording type polygraph would require more amplification, or if a much higher input-voltage exists, a voltage-divider-network should be employed. The power-supply used must be stable and well filtered.

*Adjustment-procedure.* Place the 'Use-Adjust' switch in the 'Adjust' position after a minimal warm-up period of 20 min. Each integrator is adjusted separately with its input-circuit open and the condenser-shorting

switch in the shorting position. Adjust the coarse-balance-potentiometer for zero on the  $\times 100$  meter-scale. Switching to the  $\times 10$  position, adjust fine-balance and bias controls for zero voltage.

If the needle drifts upward, decrease the gain, and if a downward movement occurs, increase the gain-setting until the integrator is stable for several minutes. Turn the range-selector-switch to the  $\times 1$  position and repeat the balancing operation. A slight fluctuating needle-movement in this meter position is unimportant, since the amplifier gain is quite high in the 'Adjust' position. Return to the 'Use' position for operation.

The voltage summed on the meter will be the integrated voltage of that integrator which is switched into the circuit. Inasmuch as the meter indicates condenser-charge, it is unimportant that the meter drops to zero as switching occurs, since it will immediately indicate the charge existing in that particular circuit switched into operation. Meter readings are in terms of arbitrary area, but may be readily converted to actual area if desired, but this is unnecessary. Total curve-area is, of course, the sum of the voltage-readings for both integrators, one indicating positive area summation, the other negative summation of area.

The integrators should integrate identically along the same straight line up to 100 v. Each integrator after reaching any given voltage should, with the input-circuit open, hold the indicated voltage for several hours with no drift. When properly adjusted the integrator, after warm-up, will require no readjustment for days. In general, anyone familiar with the soldering-iron can construct the system readily with full expectation of successful operation.



## A SIMPLIFIED STIMULUS-GENERATOR

By CARROLL E. HUMPHREY, The Johns Hopkins University

Psychologists in many academic situations are troubled by insufficient funds with which to purchase experimental equipment. The present paper shows a device which is useful in classrooms or laboratory demonstrations of electro-stimulation with resultant responses of visual, auditory, and gustatory mechanisms. Somesthesia may also be demonstrated in terms of cutaneous pain-qualities such as itchy, pricking, and dull pain. Further, inner ear somesthesia may be demonstrated by stimulation of the semi-

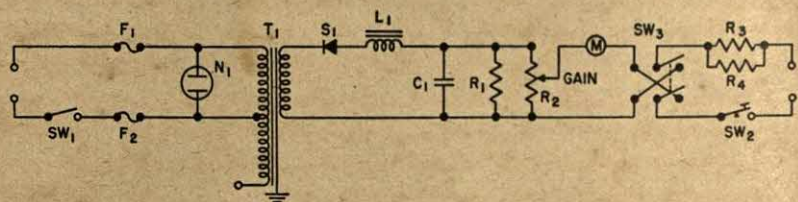


FIG. 1. SCHEMATIC WIRING DIAGRAM OF THE STIMULUS-GENERATOR

$C_1$  = 25  $\mu$  fd.—50 V. Capacitor

$F_1$  = 0.25 Amperes Fuse

$F_2$  = 0.25 Amperes Fuse

$L_1$  = 10 hy 50 Ma. Choke Transformer

$M_1$  = 0.5 Ma. D.C. Meter

$N_1$  = NE 51 Neon Lamp

$R_1$  = 2500  $\Omega$  5 W. Resistor

$R_2$  = 10 K  $\Omega$  3 W. Potentiometer

$R_3$  = 62 K  $\Omega$  1 W. Resistor

$R_4$  = 5100  $\Omega$  1 W. Resistor

$S_1$  = 125 or 75 Ma. Selenium Rectifier

$SW_1$  = Power Switch

$SW_2$  = Key

$SW_3$  = D.P.D.T. Switch

$T_1$  = 325-0-325 V. Power Transformer Reversed

circular canal with resultant vertigo. The apparatus is safe, readily constructed with simple parts, is inexpensive, and reliably stable in performance.

The schematic design is shown in Fig. 1. An ordinary replacement type radio-transformer is used with the mains-power introduced into one half of the secondary winding, and the output taken from the former primary winding. A half-wave selenium-rectifier is used to rectify the approximately 35-v. output which is then filtered and metered. A switch is employed to reverse the output polarity of the pulsating direct-current. A key permits simple pulsing of the output.

\* From the Applied Physics Laboratory, Silver Spring, Maryland.



Safety-precautions have been taken in that both sides of the power-source are fused and the output-voltage is separated from the chassis. The potentiometer is paralleled by a bleeder resistor in case of failure and a current-limiting resistor is always in series with the load. It is a shunt form for additional safety.

Output is controlled by  $R_2$  and is adjustable from 8 m.amp. with a zero resistance load to 1 m.amp. at 40 v. with a 50,000 ohm load. The device is arranged to produce a maximal output-current of 2 m.amp. at a potential of 30 v. when the load-resistance is of the order of 25,000 ohms. This is approximately the mean resistance of the body with one electrode applied to the palm and one applied to the supra-orbital region of the face.

Electrodes consist of a pair of ordinary meter test-leads. The removable needles were soldered into one lip of a 2-in. chassis hole-plug. Two plugs were then so forced together that the spring-lips interlocked thereby affording two smooth low-resistance surfaces for one electrode. Each electrode has such an arrangement.

To operate, the device is plugged into the mains-source and the potentiometer is rotated counter-clockwise. The electrodes are applied and the potentiometer is rotated clockwise until the desired current-flow is indicated on the meter. With one electrode in the palm and the other applied in the vicinity of the eye a phosphene occurs at less than 1 m.amp. of current. The sensation of taste occurs at approximately 2 m.amp., while vertigo occurs at 4 to 6 m.amp. To measure resistance, a voltmeter may be applied to the output terminals, then by using the indicated voltage and the current indicated on the meter, simple recourse to Ohm's Law will yield the desired information.

To operate, the power is turned on, electrodes are applied with the gain potentiometer fully counter-clockwise, then the gain control is advanced until the desired current is indicated. Maximal output is limited to between 8-9 m.amp. in a direct shorting of electrodes.



## AN APPARATUS FOR RECORDING CHANGES IN SKIN-TEMPERATURE

By LAWRENCE M. BAKER and WILLIAM M. TAYLOR, Purdue University

An apparatus for detecting and recording changes in skin-temperature, which we have successfully used, is described here.

The thermal pick-up attached to the skin is composed of several thermistors. These are connected in series and housed in a plastic material which is then embedded in an insulator such as celotex. Wires are soldered to the thermistors as indicated by the dotted lines in Fig. 1. The terminals are connected across a balanced bridge-circuit in which the bridge-arms have a resistance of the order of 3000-6000 ohms. The thermistors, when connected in series, have a constant potential of 22 v. applied to them. Any change in the flow of current will unbalance the

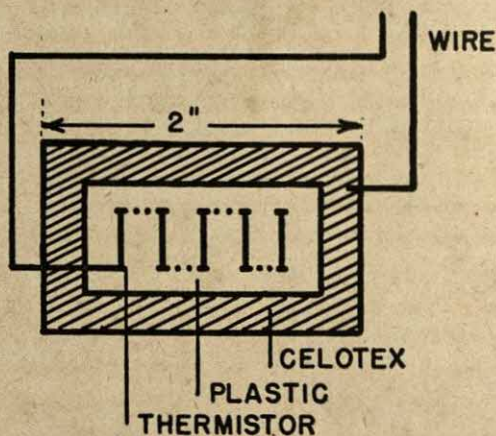


FIG. 1. APPARATUS FOR RECORDING CHANGES IN SKIN-TEMPERATURE

bridge-circuit, the potential of which is amplified to produce the proper energy for recording. A permanent ink-recording is produced on a polygraph which contains a 0.1 m.amp. recording movement.

According to the manufacturers of the thermistors, temperature changes give rise to changes in the resistance properties of the thermistors and hence cause variability in the flow of an electric current.<sup>1</sup> This variation in the potential is then used as an index of skin-temperature. Preliminary trials led us to use 10 thermistors in series—

<sup>1</sup>The thermistors used by the authors were manufactured by the Western Electric Company. They are semi-conductors composed of materials having great resistance variability with temperature changes.

this number was found to be sufficiently sensitive to obtain records showing changes in skin-temperature accompanying the presentation of emotionally loaded words.<sup>2</sup>

Each apparatus must be calibrated before confidence can be placed in its readings. Calibration was done as follows. A highly sensitive thermometer was placed in water with the thermistor pick-up. The temperature of the water was systematically varied and simultaneous readings were taken from the thermometer and the polygraph record. A change of  $0.2^{\circ}\text{C}$ . gave rise to an excursion of the recording stylus of about 1 cm. from the base-line. This relationship was linear over the range studied between  $29\text{--}35^{\circ}\text{C}$ . These results were further checked for comparative purposes with dry heat changes and also by attaching the thermometer and thermistors to the skin. In all cases the thermistors were faster in indicating any temperature change, thus revealing a time lag in the thermometer.

The apparatus is believed to be reliable, rugged, and highly sensitive to small and sudden temperature changes. Important to psychologists is its relative freedom from undesirable side effects in its use with human subjects when experimenting upon such problems as learning, emotion and work. It has, moreover, the obvious advantage of giving a continuous and permanent record.

While there are doubtlessly many uses to which this device may be put, we are interested primarily in using it to study temperature-changes under emotional stress and following the administration of drugs. The apparatus gives us, however, additional information regarding bodily changes. Even if it had no advantage over those already established and used, its use for that reason alone would still be justified. The neural and circulatory functions believed producing temperature changes are related to those giving rise to changes in blood pressure GSR, etc., hence there is promise that temperature changes, if accurately measured, will prove to be a valuable additional technique for exploring the relationship between such bodily functions as autonomic nervous activity, muscular responses, endocrine reactions, and various psychological phenomena.

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<sup>2</sup> Much of the work in assembling and modifying the apparatus described was done by the Lafayette Instrument Company.



## NOTES AND DISCUSSIONS

### PSYCHOLOGICAL-MATHEMATICAL PROBABILITY IN RELATIONSHIPS OF LOTTERY GAMBLES

Two recent articles in this JOURNAL have been concerned with the relationship between psychological and mathematical probabilities. Preston and Baratta reported on an experimental card game in which subjects could gamble for prizes (in points) at given odds, *i.e.* given mathematical probabilities.<sup>1</sup> Defining psychological probability as the ratio of the price paid for the opportunity to gamble for a given prize to the size of that prize, they determined what psychological probability was needed to correspond to a known mathematical probability. "For example, if a prize of \$50 is won by a price of \$4.50, when  $p = 0.01$ , we may say that such a person is behaving as if a  $p$  of 0.01 were a  $p$  of 0.09. The  $p$  of 0.09 we call a psychological probability."<sup>2</sup>

A graph of the relationship between the psychological and mathematical probabilities of their experiment showed that mathematical probabilities of less than 0.05 are subject to systematic overestimation and mathematical probabilities of more than 0.25 are subject to systematic underestimation, with an indifference point in the scale of probabilities in the range 0.05 to 0.25. This indifference point was discussed in relation to adaptation-levels, and the analogy was carried to the point of suggesting that the indifference point is in the neighborhood of the geometric mean of the mathematical probabilities, 0.24 in their experiment.

Following them, Griffith reported some empirical data from a non-laboratory test—the odds adjustment in horse-race betting.<sup>3</sup> The odds on the various horses in any race are set by the bettors and reciprocally express a psychological probability. The percentage of actual winners at any odds is taken as a measure of the true probability of winning. Making a necessary adjustment for the track 'take' (revenue for the track plus state and local taxes), Griffith's data corroborated Preston and Baratta's findings. There was a systematic undervaluation of the chances of short-odded horses and overvaluation of long-odded horses. The indifference point was at

<sup>1</sup> M. G. Preston and Philip Baratta, An experimental study of the auction-value of an uncertain outcome, this JOURNAL, 61, 1948, 183-193.

<sup>2</sup> *Ibid.*, 189.

<sup>3</sup> R. M. Griffith, Odds adjustments by American horse-race bettors, *ibid.*, 62, 1949, 290-294.



odds of about 6.1 which is reciprocally a probability of 0.16 and corresponds to the indifference range of 0.05 to 0.25. Griffith did not, however, concur with Preston and Baratta on their analogy to adaptation-levels but suggested that the indifference point may be fairly constant and relatively independent of the geometric mean.

More recently, Mosteller and Nogee conducted an experiment in which groups of students and National Guardsmen played a dice game for money.<sup>4</sup> The experiment was set up to measure the economic concept of utility, but with little extra effort, psychological probabilities were computed. For the students, no indifference point was discovered; their psychological probabilities systematically understated the mathematical probabilities. For the National Guardsmen, an indifference point occurred in the neighborhood of 0.50. The results seemed not to agree too well with either of the other two studies.

The present paper will present data from another non-laboratory source—the current French, Spanish, and Mexican lotteries. These lotteries are schemes for distributing prizes by lot or chance in which the chances are sold for money and the prizes offered are money. They differ in several respects from the Preston-Baratta laboratory gamble. (1) The data are generated by the economic system and not by a controlled laboratory experiment. (2) The prize units are not trivial sums of money. (3) There is a multiplicity of outcomes, *i.e.* thousands of prizes are offered. (4) There is a large number of participants. (5) Lotteries are unfair gambles, *i.e.* they are 'loaded' in favor of the management, and if an individual buys a ticket or a fraction of a ticket, his psychological probability must *over-value* the mathematical probability of winning.

There is no a priori reason to expect any agreement between lottery data and the results of the Preston-Baratta study because of these differences. If there is any agreement, the relationship which they report may be a more general phenomenon than a laboratory exercise. This, of course, is their expectation.

The characteristic of the lottery schemes which bears directly on the mathematical-psychological probability problem is the probability of winning a prize. The first problem is to decide what probability the individual evaluates when purchasing a ticket. The simplifying assumption will be made that he buys only one ticket because: (1) the probability of winning a prize with the purchase of one ticket is the ratio of the total number of

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<sup>4</sup> Frederick Mosteller and Philip Nogee, An experimental measurement of utility, *J. Polit. Econ.*, 59, 1951, 371-404.



prizes to the total number of tickets offered for sale and is a characteristic of the lottery gamble which a prospective participant faces at the time when he decides whether to buy a ticket; (2) the distribution of actual ticket purchases is not known; (3) the arithmetic of probabilities becomes tedious with more than one ticket.

There are two situations for which the individual's behavior is analyzed. First, he is considered to judge the distribution of prizes as a whole. The appropriate mathematical probability is the ratio of the total number of prizes offered in the lottery to the total number of tickets, *i.e.* the probability of winning a prize. Secondly, he is considered to look at one particular size of prize, the probability of winning which is the ratio of the number of prizes of that size to the total number of tickets.

An alternative statement of the Preston-Baratta hypothesis is that the scale of mathematical probabilities of gambles (call them  $P_m$ ) may be divided into three regions:

(a)  $P_m \leq 0.05$ : a systematic overvaluation of  $P_m$ .

(b)  $0.05 < P_m < 0.25$ : an indifference range.

(c)  $P_m \geq 0.25$ : a systematic undervaluation of  $P_m$ .

It should be pointed out that the values of  $P_m$  which define the boundaries of these regions (0.05 and 0.25) have not been accurately determined because no probabilities between 0.05 and 0.25 were offered to the subjects in the Preston-Baratta experiment. To be consistent with the hypothesis, the lottery probabilities *cannot fall in region (c)* because the individual ticket purchaser must overvalue  $P_m$  and (c) is a region of systematic undervaluation of  $P_m$ . If the lottery probabilities fall in region (a), they are clearly consistent with the hypothesis; if they fall in region (b) there is a doubtful agreement because of the inaccuracy in the determination of the bounds of this region.

*Situation 1: Total distribution of prizes.* The mathematical probability appropriate to Situation 1 is the ratio of the total number of prizes offered to the total number of tickets. The data for the French,<sup>5</sup> Spanish,<sup>6</sup> and

<sup>5</sup> The source of the French data is the *Journal Officiel de la Republique francaise*, Part I, *Lois et Decrets*, (Paris: Imprimerie Nationale) 1946-1949. The terms of the lottery are changed once each year and the same lottery is drawn weekly within one year. Note that the data are not complete for 1946 and 1949.

<sup>6</sup> The source of the Spanish data is the *Prospectos de Premios*, (Madrid: Impresor Gobiernista) 1947-1949. These handbills are distributed by the Director General de la Loteria Nacional. The author received a supply of them from a friend who was in Spain in 1949. The terms of the lottery change from one drawing to the next, although the same lottery may be drawn several times.



Mexican<sup>7</sup> lotteries are given in Table I. The outstanding characteristic of these data (for the hypothesis under test)<sup>8</sup> is that for every lottery in the three samples,  $P_m$  falls in region (b):  $0.05 < P_m < 0.25$ . There is no genuine disagreement with the hypothesis because the  $P_m$  did not fall in region (c) but, at the same time, there is no well-defined agreement because  $P_m$  did not fall in region (a).

*Situation 2: Individual size of prizes.* Each of the lotteries in the samples has several thousand prizes. There is one very large one, several smaller ones, and very many extremely small prizes.<sup>9</sup> The individual is here con-

TABLE I  
PROBABILITIES OF WINNING A PRIZE IN FRENCH, SPANISH, AND MEXICAN LOTTERIES

Year	France (1946-1949)		Mexico (January-June 1952)		Spain (1947-1949)	
	$P_m$	$N$	$P_m$	$N$	$P_m$	$N$
1946	0.174	8	0.117	*	0.135	5
1947	.229	52	.217	*	.136	2
1948	.243	52	.218	60	.137	7
1949	0.245	24	.219		.144	31
		—	.220		.145	43
	Total	36	0.230	1	.146	9
				—	0.164	2
			Total	61		—
					Total	99

\* Listed in the brochure of lottery schemes but not drawn.

sidered to look at one particular size prize. The appropriate  $P_m$  is the ratio of the number of prizes of that size offered to the total number of tickets. This situation is interesting and not unrealistic of the way in which lottery designers think that people behave in purchasing lottery gambles. Milton Friedman of the University of Chicago expressed this verbally to the author as a result of discussions of the French lottery which took place at a seminary which he offered at the École des Mines during the autumn of 1950. Members of the seminar told him that the maximal prize was the important one, and that the large number of small prizes are essentially

<sup>7</sup> The source of the Mexican data is a small brochure *Loteria Nacional, Calendario de Sorteos*. The author obtained a copy in the local lottery office in Tijuana, Mexico.

<sup>8</sup> It is also clearly evident that the chance of winning a prize is consistently greater in the French and Mexican lotteries than in the Spanish lotteries.

<sup>9</sup> The maximal prize in the French lottery is fifteen million francs; it is fifteen million pesetas in the Spanish lottery and often, but not always, ten million dollars in the Mexican lottery. The smallest French prize is 1000 francs. In Spain it is often, but not always, as small as 50 pesetas, and in Mexico, sometimes as small as \$10.



refunds which, when won, are an incentive to purchase a ticket in the next lottery.

In every French, Spanish, and Mexican lottery in the sample, the probability of winning any size prize except the very smallest was less than 0.05. The probabilities of winning the minimal prize in the French lottery ranged from 0.15 to 0.20; in the Spanish lottery it was consistently very close to 0.10; and in the Mexican lottery, it was 0.189 with one exception, which was 0.10. If individuals judge lottery gambles by looking at *one* of the larger prizes, the data are clearly consistent with the hypothesis. If they look at the smallest prize, there is again no clear-cut disagreement with the hypothesis, but at the same time, the agreement is doubtful.

#### SUMMARY AND CONCLUSIONS

There is no definite disagreement between the data for the probability of winning a prize in the current French, Spanish, and Mexican lotteries and the hypothesis that the scale of mathematical probabilities of uncertain outcomes may be broken into three regions: one of systematic overvaluation ( $P \leq 0.05$ ); one of systematic undervaluation ( $P \geq 0.25$ ); and one of indifference ( $0.05 < P < 0.25$ ), because lottery probabilities must be overvalued and none are greater than 0.25. If individual ticket buyers evaluate the gamble by looking at any but the smallest prize, the data are clearly consistent with the hypothesis because the mathematical probability of winning one of these prizes is, in every instance, less than 0.05. If the ticket buyer evaluates the gamble either by looking at the whole distribution of prizes and, therefore, the probability of winning *a* prize, or at the minimal prize and its associated probability, there is a doubtful agreement between the data and the hypothesis, because these probabilities are in the range from 0.10 to 0.20.

The general conclusion seems to be that the systematic over- and undervaluation of probabilities found by Preston and Baratta in their experiment is a phenomenon which does exist outside the experimental laboratory.

University of California

R. CLAY SPROWLS



## CONCERNING THE USE OF ANALYSIS OF VARIANCE ON LATENCY DATA

This paper is concerned with the treatment of data on reaction-latency. Its purpose is to present information about (a) the assumptions basic to the analysis of variance that are likely to be violated,<sup>1</sup> (b) the effectiveness of certain transformations for correcting data that do not meet the assumptions,<sup>2</sup> and (c) the kind and degree of error that may result from drawing inferences based upon analyses of uncorrected data.

The data to be discussed were obtained from an experiment in which 45 rats were given 100 acquisition- and 40 extinction-trials in a simple maze-running-bar-pressing situation.<sup>3</sup> Five Ss were assigned at random to each of the nine groups formed by combining three values of each of two independent variables—bar-weight and inter-trial interval—in a two-way factorial design. The data consist of the latencies of the bar-pressing responses.

Several analyses of variance were performed on various sets of these data. In every case the data failed to meet one or more of the assumptions. One of the most common methods for handling data violating these assumptions is the use of transformations, the usual purpose of which is to change the scale of measurement in such a way as to make the analysis more valid.<sup>4</sup> Logarithmic transformations appear to be particularly useful in the case of latency data because they tend to correct for the positive skewness which is characteristic of distributions of latency scores.

Four sets of data were selected for the present discussion. Table I identifies these sets and presents for the raw and transformed data the *F*-ratios and the values of *L* and *tau* together with the significance levels of these statistics. *L* is a measure of homogeneity of variance: when the variances are exactly equal, *L* equals 1.00; as they become more heterogeneous, *L* decreases in value.<sup>5</sup> Plots of the variances against the means for each set of raw data reveal a strong tendency toward proportionality; the value of *tau* is a measure of the intensity of this relationship. The rank order correlation coefficient *tau* is very useful for this purpose when the number of

<sup>1</sup> C. Eisenhart, The assumptions underlying the analysis of variance, *Biometrics*, 3, 1947, 1-21.

<sup>2</sup> M. S. Bartlett, The use of transformations, *ibid.*, 3, 1947, 39-52.

<sup>3</sup> K. C. Montgomery, An experimental investigation of reactive inhibition and conditioned inhibition, *J. Exper. Psychol.*, 41, 1951, 39-51.

<sup>4</sup> Bartlett, *op. cit.*, 39.

<sup>5</sup> F. E. Croxton and D. J. Cowden, *Applied General Statistics*, 1941, 359-362.



experimental groups is small, mainly because its distribution has been worked out more fully than that of *rho*.<sup>6</sup> A rough indication of the degree of approximation to normality was obtained by plotting frequency distributions of the original and transformed measures.<sup>7</sup>

The first analysis concerns the reaction-latencies during the entire 100 acquisition trials. The latency scores were averaged over all trials for each

TABLE I

A SUMMARY OF THE EFFECTS OF CERTAIN LOGARITHMIC TRANSFORMATIONS OF LATENCY DATA UPON *F*-RATIOS AND VARIOUS MEASURES OF THE CONFORMITY OF THE DATA TO THE BASIC ASSUMPTIONS FOR THE ANALYSIS OF VARIANCE

The transformations are described in the text. The significance level of each statistic is indicated in adjacent parentheses

	Row	Data	<i>F</i> for bar- weight	<i>F</i> for inter-trial interval	<i>F</i> for inter- action	<i>L</i>	<i>tau</i>
Acquisition trials	entire 100	A Original	15.33 (.001)	4.65 (.05)	1.40 (—)	.408 (.01)	.28 (.18)
		B Transformed	14.96 (.001)	7.93 (.01)	0.08 (—)	.673 (—)	.00 (.54)
	last 10	C Original	4.34 (.05)	6.28 (.01)	3.09 (.05)	.640 (—)	.50 (.04)
		D Transformed	4.17 (.05)	6.16 (.01)	0.49 (—)	.658 (—)	.39 (.09)
Extinction trials	first 10	E Original	3.25 (—)	0.43 (—)	0.92 (—)	.466 (.01)	.67 (.006)
		F Transformed	3.93 (.05)	0.72 (—)	0.84 (—)	.648 (—)	.50 (.04)
	first 20	G Original	5.08 (.05)	1.07 (—)	1.84 (—)	.321 (.01)	.83 (.0004)
		H Transformed	6.47 (.01)	2.42 (—)	1.23 (—)	.649 (—)	.50 (.04)

*S*, and an analysis of variance was performed on the resulting set of means.<sup>8</sup> The results appear in Row A, Columns 2, 3, and 4 of Table I. The *F* for bar-weight is significant at beyond the 0.1-% level; the *F* for inter-trial interval, at the 5-% level. Although the *F* for interaction is non-significant and the value of *tau* falls within the limits of sampling

<sup>6</sup> M. G. Kendall, *Rank Correlation Methods*, 1948, 37-54.

<sup>7</sup> To facilitate comparisons, the range of each set of data was divided into five equal class intervals represented in arbitrary units on the abscissa of each graph.

<sup>8</sup> In this and all succeeding analyses, the within-groups variance is used as the error variance.



error, the value of  $L$  is significant at beyond the 1-% level. Moreover, the frequency distribution is markedly skewed in the positive direction. To correct for the violation of these two assumptions (homogeneity of variance and normality of distribution), a logarithmic transformation was used.<sup>9</sup> For every  $S$  the mean latency for each block of 10 trials was computed, the log obtained for each value, and the mean of the log scores obtained. An analysis of variance, the results of which appear in Row B of Table I, was performed on the resulting set of means. The results show that the  $F$  for inter-trial interval conditions is now significant beyond the 1-% level, that no important changes occur in the other two  $F$ s, that the variances are homogeneous, and that the distribution of scores more nearly approximates normality. Thus the net effect of the transformation is to increase the sensitivity of the  $F$ -test for inter-trial interval conditions. Although no erroneous conclusions would have been drawn from the analysis of the uncorrected data, it is possible that cases might easily occur in which a more or less serious error would be made, *i.e.* an underestimation of the significance of treatment effects when the assumptions of homogeneity of variance and normality are violated.

The second analysis concerns the latency scores for the last 10 acquisition trials. To determine whether the treatment conditions significantly affected these scores, the mean latency was obtained for every  $S$  and an analysis of variance carried out. In Row C of Table I it will be seen that the  $F$ s for bar-weight and inter-trial interval conditions are significant at beyond the 5- and 1-% levels, respectively. Although the variances are homogeneous (Column 5), they tend, however, to be proportional to the means (Column 6). In addition, the frequency distribution is positively skewed and the interaction  $F$  is significant, indicating non-additivity.<sup>10</sup> To correct these data, the following transformation was used: the log of each average latency plus one was obtained.<sup>11</sup> The results of analyzing the transformed scores appear in Row D of Table I. The  $F$  for interaction is now far below significance, the proportionality between means and variances is reduced, and the distribution of scores is more nearly normal. Again no erroneous conclusion would have been drawn from the uncorrected data. Apparently, non-additivity must be present to a relatively marked degree before serious distortion occurs. This conclusion agrees

<sup>9</sup> In this and all succeeding cases, 'log' or 'logarithmic' refers to common logarithms.

<sup>10</sup> Eisenhart, *op. cit.*, 10.

<sup>11</sup> This particular transformation was employed because some of the original means are less than 1.00, and the procedure of transforming the scores and performing further computations is facilitated if there are no negative logarithms with which to work.



with the statement by Cochran: "Unless experimental errors are low or there is a very serious departure from additivity, this loss (of information) should be negligible when treatment and replication effects do not exceed 20 per cent, since within that range the additive relationship is likely to be a good approximation to most types that may arise."<sup>12</sup>

The third set of data consists of the latencies on the first 10 extinction trials. The results of an analysis of variance performed on the mean scores appear in Row E of Table I. None of the  $F$ s are significant at even the 5-% level. The values of  $L$  and  $\tau$  are highly significant, and the distribution is strongly skewed. Row F of Table I presents the results of an analysis of these scores transformed as in the preceding example. The  $F$  for bar-weight is now significant at beyond the 5-% level. Moreover, the variances are homogeneous, the proportionality between means and variances is considerably reduced, and the non-normality of the distribution is largely corrected. The net result of the transformation is to increase the sensitivity of the  $F$ -test for the bar-weight conditions. If the transformation had not been used, an erroneous inference would have been made, *i.e.* that reaction-latency is not related to bar-weight on this set of trials.

The last example concerns the first 20 extinction trials. An analysis of variance was performed on the mean latency scores. In Row G it can be seen that the  $F$  for bar-weight is significant, but that the variances are not homogeneous (Column 5) and are proportional to the means (Column 6). As in the previous examples, the distribution of scores exhibits marked positive skewness. The transformation described in the first example was used, and a second analysis of variance performed. In Row H it will be seen that the significance level of the  $F$  for bar-weight is raised from 5- to 1-% and that the  $F$  for inter-trial interval is more than doubled, although still not significant. Moreover, the variances are now homogeneous, their proportionality to the means is considerably reduced, and the distribution is more nearly normal. Again, correcting the data leads to an increase in sensitivity of the  $F$ -test.

From the preceding examples, and from others taken from the same experimental data but not cited here, several conclusions may be drawn.

(1) When measures of reaction-latency are taken under various experimental conditions it is probable that the data will exhibit one or more of the following characteristics: (a) positive skewness; (b) non-additivity;

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<sup>12</sup> W. G. Cochran, Some consequences when the assumptions for the analysis of variance are not satisfied, *Biometrics*, 3, 1947, 22-38.



(c) heterogeneity of error variance; and (d) proportionality of means and variances.

(2) Each of these violates a basic assumption of the analysis of variance; hence, some form of correction may be necessary if this technique is used.

(3) Logarithmic transformations appear to be particularly useful for correcting latency data.

(4) One cannot predict *a priori* which assumptions will be violated or what the effects will be of a given violation or combination of violations.

(5) When the analysis of variance is used on uncorrected latency data the main distortion, at least in the examples presented above, is a loss in sensitivity of the *F*-tests for treatment conditions. It is quite possible for cases to arise in which incorrect inferences will be drawn from the results of analyses of variance performed on such data, particularly when the *F*s lie near the borderline of significance. It is, therefore, strongly suggested that whenever the analysis of variance is used on latency data serious attention be given to the question of whether its basic assumptions are met.

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K. C. MONTGOMERY

### RELATION OF THE RAYLEIGH RATIO TO COLOR-TEMPERATURE

The Nagel anomaloscope affords such a useful test of color vision that any pertinent information concerning sources of significant error seems noteworthy. An effect of field-size on the value of the Rayleigh ratio has been demonstrated recently.<sup>1</sup> With normal observers at least, the greater the distance at which a fixed anomaloscopic field is viewed, the smaller the angular subtense and the more green is needed in the green-red mixture to match the yellow half of the field. With an artificial pupil before the eye, there seems to be less change in matching mixture with distance. Ordinarily, variation of field-size need not be a source of error because the eye-piece of the anomaloscope can keep the eye at nearly constant distance. In case of the Model I Nagel, by Franz Schmidt and Haensch, the diameter of the field is 2° of visual angle.

Unlike field-size, the influence of the color-temperature of the lamp illuminating the slits seems largely to have been ignored; and this factor

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<sup>1</sup>R. G. Horner and E. T. Purslow, Dependence of anomaloscope matching on viewing distance or field size, *Nature*, 160, 1947, 23-24; H. Hartridge, Dependence of anomaloscope matching on viewing distance or field size, *Nature*, 160, 1947, 831-832.



is not automatically controlled by the construction of the instrument. How closely the color-temperature needs to be controlled depends on how much it affects the observed Rayleigh ratios. The object of this note is to report some data on this point.

Each of five *O*s made five series of 10 matches each with a Model I

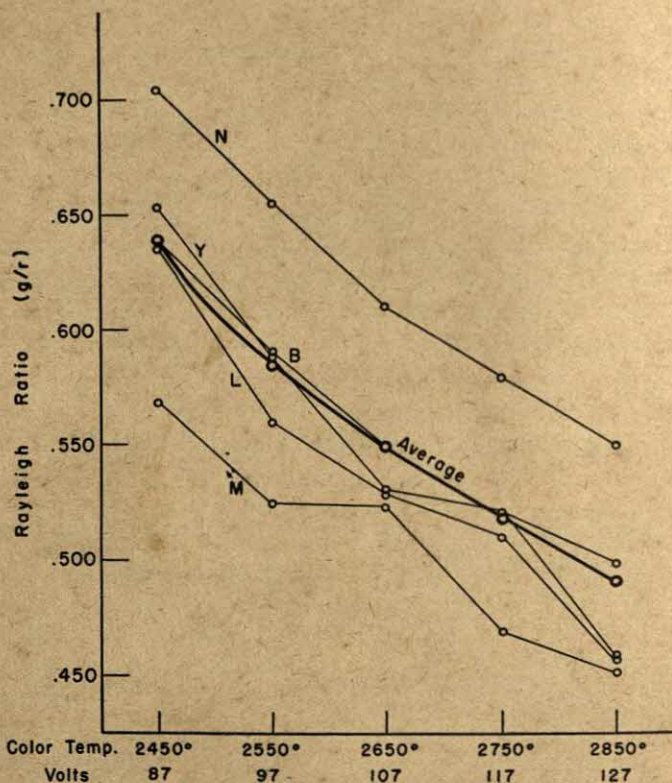


FIG. 1. THE EFFECT OF COLOR-TEMPERATURE ON THE RAYLEIGH RATIO  
Data for 5 *O*s and the average for the entire group are plotted.

Nagel anomaloscope, each series being at a different color temperature. A 60-w., inside frosted, 120-v. tungsten lamp was used. In Fig. 1, the Rayleigh ratios ( $g/r$ ) corresponding to the five mean matches of each *O* are plotted and connected by straight lines, while the averages for all the *O*s are connected by the heavy curve. The ratio is seen to vary inversely both with the color-temperature and the lamp-voltage. In the observational range of 2450° to 2850°, there was an average change of about 0.01 in

the Rayleigh ratio for every 30°-change in color-temperature, or every 0.26-v. change in lamp-voltage. This indicates the desirability of controlling the voltage rather closely. Since the relation between color-temperature and lamp-voltage is almost linear, a voltmeter with voltage-regulator provides a convenient means of control. Seasoned tungsten lamps of uniform type and rating usually differ but little in color-temperature when operated at the same voltage. A lamp operated below the rated voltage and used only occasionally for visual testing will not vary to any considerable extent for many months. Periodic checking against a standard is, however, desirable.

Eastman Kodak Company

SIDNEY M. NEWHALL

### THE SOLUTION OF ODDITY-PROBLEMS BY THE RAT

Some years ago, Lashley trained rats "in an attempt to establish the generalized reaction described by the clause, *that one of any three figures is correct which is different from the other two.*"<sup>1</sup> Although Klüver and Robinson had previously reported reactions to oddity in the monkey,<sup>2</sup> Lashley was unable to develop such reactions in the rat. Some of his animals, for example, were trained to choose a cross presented with two circles in a three-window jumping apparatus (white figures on black grounds) and then to choose a circle presented with two crosses. "Alternate training was continued in the hope that the animals might eventually come to choose whichever figure was presented singly. Instead, after the third to fifth reversal all animals became confused and either refused to jump or jumped persistently to one figure in spite of scores of bumps and falls."<sup>3</sup> The animals could master a variety of specific combinations, but failed consistently when a formerly negative card was presented with two formerly positive ones.

In subsequent experiments Lashley found it possible for rats to develop a 'conditional reaction' of a kind which could readily be interpreted in terms of stimulus-compounding—i.e. upright and inverted triangles were presented on black and striated grounds, with upright positive on one ground and inverted positive on the other.<sup>4</sup> Attempts to develop "general-

<sup>1</sup> K. S. Lashley, The mechanism of vision: XV. Preliminary studies of the rat's capacity for detail vision, *J. Gen. Psychol.*, 18, 1938, 176.

<sup>2</sup> Heinrich Klüver, *Behavior Mechanisms in Monkeys*, 1933, 1-387; E. W. Robinson, A preliminary experiment on abstraction in a monkey, *J. Compar. Psychol.*, 16, 1933, 231-236.

<sup>3</sup> Lashley, *op. cit.*, 177.

<sup>4</sup> Lashley, Conditional reactions in the rat, *J. Psychol.*, 6, 1938, 311-324.



zation of a second order" met, however, with no success; the animals could not learn to react on the basis that "any stimulus which is correct in situation A is incorrect in situation B."<sup>5</sup> In neither series of experiments, then, could Lashley's rats "derive the general principle from the series of specific incidents."<sup>6</sup>

There is, however, in the literature some evidence that the rat is capable of responding in an integrated fashion to a series of experiences which, in *concrete* terms, might be expected to engender only the sort of confusion and stereotypy described by Lashley. Krechevsky has reported that after a series of reversals in a light-dark discrimination it was possible for the rat to shift its preferences rapidly from light to dark and back.<sup>7</sup> As Harlow has noted in a recent paper on the behavior of the monkey under comparable conditions, such flexibility is not easily accounted for by stimulus-bound theories of the Pavlovian variety.<sup>8</sup> When in the course of recent experiments with the three-window jumping apparatus,<sup>9</sup> our observations led us to believe that the solution of oddity-problems was not beyond the capacity of the rat, Krechevsky's results encouraged us to reexplore the possibility.

The rats selected for the work had been given considerable training in jumping situations—several in a three-window apparatus and others in a single-window apparatus. Since all of the animals solved one or more oddity-problems in essentially similar fashion, the procedure and results for only one rat will be described in detail.

Rat No. 10 was trained first to a black card (positive) versus two white cards (negative). The method of correction was used—after three incorrect responses to any one arrangement of the cards, the animal was manually guided in the correct direction. Each of the three possible card-arrangements was presented equally often, with 18 trials per day usually being given. After the first errorless day, the problem was shifted to white card (positive) versus two black cards (negative), and the animal was trained to the same criterion. Then the first part-problem was introduced again, then the second, and so forth. After the fourth reversal the criterion of mastery for each part-problem was reduced to three successive error-

<sup>5</sup> *Ibid.*, 312.

<sup>6</sup> *Ibid.*, 322.

<sup>7</sup> I. Krechevsky, Antagonistic visual discrimination habits in the white rat, *J. Compar. Psychol.*, 14, 1932, 263-277.

<sup>8</sup> H. F. Harlow, Performance of catarrhine monkeys on a series of discrimination reversal problems, *J. Compar. Physiol. Psychol.*, 43, 1950, 231-239.

<sup>9</sup> Jerome Wodinsky and M. E. Bitterman, Compound and configuration in successive discrimination, this JOURNAL, 65, 1952, 563-572.



less trials. The animal mastered each successive part-problem with fewer errors than its predecessor, as in the experiments of Krechevsky and Harlow, until after about 30 reversals each shift was accomplished without error. At this stage the two part-problems were merged into a complete oddity-problem (all six card-arrangements being presented in random

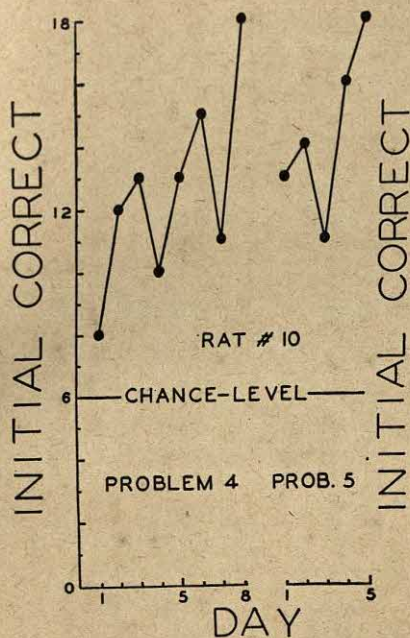


FIG. 1. PERFORMANCE OF RAT NO. 10 Black triangles and circles on white grounds were used in the fourth problem, and horizontally and vertically striated cards were used in the fifth.

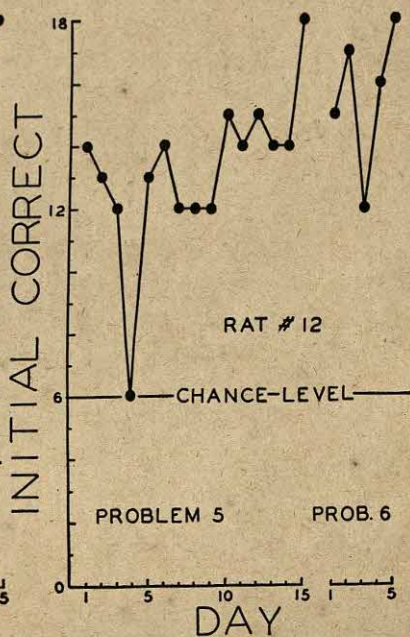


FIG. 2. PERFORMANCE OF RAT NO. 12 Black triangles and rectangles on white grounds were used in the fifth problem, and black circles and rectangles on white grounds were used in the sixth.

sequence). After a slight initial disturbance, the animal responded perfectly over a series of several days.

The second problem was related to the first, *but from the outset it and all subsequent problems were presented as wholes and not divided into parts*. The animal was trained to white triangle on black ground (positive) versus two black circles on white grounds (negative) and black circle on white ground (positive) versus two white triangles on black grounds (negative). In the third problem the figures were white on black grounds, and the animal was trained to respond to the odd form. In the fourth



problem the same figures were used, but the figure-ground brightness relation was the reverse of that in the third problem. In the fifth problem black and white striated cards, horizontal and vertical, were used—horizontal (positive) versus two verticals, and vertical (positive) versus two horizontals. Each problem was learned more readily than its predecessor, and, beginning with the third or fourth problem, there was evidence of better than chance performance on the first day of training. The performance of No. 10 on the fourth and fifth problems of the series is plotted in Fig. 1. In the three-window situation the chance level of performance is 6 correct responses in 18 trials, yet it will be noted that No. 10 made 8 correct responses on the first day of the fourth problem and 13 correct responses on the first day of the fifth problem. In Fig. 2, comparable performance for another animal (No. 12) on its fifth and sixth problems is shown.

Whatever the reasons may be for the discrepancy between our results and those of Lashley, it is now evident that the rat is capable of the solution of oddity-problems. This performance cannot be explained in terms of the differential reinforcement either of afferent components or of simple afferent compounds, and the data on transfer seem to rule out the possibility of solution based on configurational discrimination. Perhaps we must think in terms of an abstract level of functioning which most of us have hitherto been reluctant to grant to the rat.

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### A SET OF FLOWER DESIGNS FOR EXPERIMENTS IN CONCEPT-FORMATION

In studies of concept-formation the Wisconsin Card Sorting Test has been of great utility.<sup>1</sup> For many types of problems, however, the limitation of the series to three factors (color, form and number) is somewhat of a handicap. A new series has been developed which has four, rather than three, sets of characteristics. An additional feature is the use of naturalistic content, which reduces the artificiality involved with abstract designs and increases the interest of the Ss.

The new set consists of pictures of flowers prepared in a uniform

<sup>1</sup>E. A. Berg, A simple objective technique for measuring flexibility in thinking, *J. Gen. Psychol.*, 39, 1948, 15-22.



format.<sup>2</sup> Four different types of blossom are employed. There are also four types of leaves. Any particular plant can have one, two, three or four leaves. The blossoms are of four different colors. Thus a series of 256 different cards is available, made up of four different *dimensions* (type of blossom, color of blossom, type of leaf, number of leaves) each of which



FIG. 1. SAMPLE DESIGNS SHOWING VARIATIONS IN TYPE OF BLOSSOM, TYPE OF LEAF, AND NUMBER OF LEAVES

Actual cards have green stem and leaves, and red, yellow, blue or white blossoms. Entire set contains 256 different cards.

is represented at four *values* (e.g. red, yellow, blue or white color of blossom). Sample designs from the series are presented in Fig. 1. It is

<sup>2</sup> The author wishes to thank Dr. Walter Weiss for supervising the preparation of the cards, Mrs. Evelyn Kemm for drawing the designs and Dr. Donald G. Young for suggestions made during discussion of possible concept-formation games for children.



hoped to make sets available to other interested investigators in the near future.

The principal type of investigation employing the new materials to date has been the study of the relative effectiveness of 'positive instances' as compared with 'negative instances' in transmitting information concerning a concept. A situation has been devised in which identical information about a concept can be transmitted by four positive instances, four negative instances, or by a combination of two positive and two negative instances (Hovland).<sup>3</sup> The experimental problem is the relative rate at which the Ss assimilate the information about the concept when it is transmitted by these three different combinations. Research results on this problem will appear elsewhere.

Another type of use may be made of the cards in studying concept formation in children. For example, data on the effects of the sequence of positive and negative instances can be secured by using the cards in a game situation.<sup>4</sup> The children are told that a gardener plants only certain types of flowers (*e.g.* flowers with red blossoms and round leaves). They are to say whether the flower on a card is one he would plant or one he wouldn't. The cards are prepared with 'right' written on the back for positive instances and 'wrong' for negative. When the child correctly guesses whether it is or is not the type planted by the gardener he is allowed to keep the card. Otherwise the turn passes to his partner. The object of the game is to secure as many cards as possible. At first, choice is of course merely random, but the acquisition of the concept and the nature of errors can be studied through the subsequent choices as the game progresses. The partner may either be another child (with the *E* recording) or the *E* himself. In similar fashion, the effect of various amounts of information about the nature of the concept and the characteristics to be considered can be investigated under controlled but naturalistic conditions.

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CARL I. HOVLAND

### A NEW VISUAL ILLUSION

Though I have searched widely through the literature, I find no mention of the illusion reported here, hence I venture to describe it and to

<sup>3</sup> C. I. Hovland, A 'communication analysis' concept learning, *Psychol. Rev.* 59, 1952, 461-472.

<sup>4</sup> C. I. Hovland and W. Weiss, Transmission of information concerning concepts through positive and negative instances, *J. Exper. Psychol.*, (in press).



refer to it as 'new.' Anyone with one good eye and two fingers who is interested in the phenomenon can easily reproduce and observe it.

The method of producing it is simple. *O* closes his poorer eye. One index finger is placed about 2 or 3 cm. in front of the open eye and the other index finger is placed about 1 mm. behind and 1 cm. off to one side of the first finger. The fingers should never touch one another. When *O* slowly moves the finger closer to him towards the second finger he should see changes (1) in size, (2) in shape and (3) in rate of movement.

(1) *Changes in size.* As the fingers move a 'growing blister' seems to arise from the *rear* finger. The largest 'blister' I have observed is about 1 cm. high and 6 cm. long.

(2) *Changes in shape.* Changes in shape are best described in terms of curves for frequency distributions. When the fingers are approximately parallel the growing blister is broad and flat (platykurtotic); when the fingers are slightly angled the blister is smaller and higher (mesokurtotic); when the angle between the fingers is changed continuously the blister is skewed from one side to the other. If a single tip of flesh or fold of skin exists on the rear finger, a very high (leptokurtotic) blister arises. When two folds of skin are present the blister is bimodal and when more folds are present it is multimodal.

(3) *Motion.* The blister appears to grow and shrink as the forward finger is moved sideways. The slower the movement the slower the blister grows; the faster the movement the faster it grows. The minimal velocity of its growth is extraordinarily low and the maximal velocity is surprisingly high.

If the reader will perform this experiment, he should be able to observe these phenomena and he may possibly find others of equal interest as yet undescribed.

Louisiana State University

FRANK M. DU MAS

## SIXTIETH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The American Psychological Association held its Sixtieth Annual Meeting in Washington, D.C., on September 1 to 6, 1952. Both in attendance and in the number of scheduled events it was the largest annual meeting in the Association's history. A total of 4,050 members and guests officially registered. There were 397 scientific papers, 80 symposia, 23 addresses, and approximately 65 business meetings of various kinds.



J. Mc.V. Hunt delivered the Presidential Address on the evening of September 1. The address, entitled "Psychological Services in the Tactics of Psychological Science," was an argument for the necessity of an alliance between science and service if either psychological science or psychological service is to prosper.

Distinguished outside speakers included Vice Admiral Joel T. Boone, who spoke on "Psychology in the Veterans Administration Medical Program," Major General Lewis B. Hershey, who participated in a symposium on Problems of Specialized Manpower, Mr. Robert Ramspeck, Chairman of the U. S. Civil Service Commission, whose address was entitled "Psychology and Better Government," and Dr. Alan T. Waterman, Director of the National Science Foundation, who spoke on "The National Science Foundation and the Life Sciences."

The APA Board of Directors met for two and a half days and the Council of Representatives for two days in transacting the Association's increasingly intricate business. Perhaps the fact that best illustrates the current magnitude of APA operations is the 1953 budget of \$377,995.00, voted by the Council of Representatives. The meeting in 1953 is to be held at Michigan State College, East Lansing, Michigan.

Washington, D.C.

FILLMORE H. SANFORD

### ELEVENTH ANNUAL MEETING OF THE CANADIAN PSYCHOLOGICAL ASSOCIATION

The Canadian Psychological Association held its eleventh Annual Meeting at the Banff School of Fine Arts, Banff, Alberta, June 16-18, 1952. Sixty-five members were present, and 20 scientific papers were read. Two round table discussions were held on the topics, "A professional code of ethics and standards" and "Research programs in psychology at Canadian universities," and an address was given by R. B. MacLeod at a dinner on "Psychological research in underdeveloped areas." The presidential address, given by N. W. Morton, was on "Psychological research in Canada and in Canadian defence." The members enjoyed two special social events through the courtesy of the Universities of Alberta and British Columbia respectively.

The officers for the coming year are: Honorary President, William Line; President, D. O. Hebb; Past President, N. W. Morton; President-Elect, D. C. Williams, and Secretary-Treasurer, G. A. Ferguson.

University of Toronto

D. C. WILLIAMS



## ERRATA

Our attention has recently been called to an error in the formula for the correlation coefficient appearing on p. 34 of our article, "A statistical description of operant conditioning" (this JOURNAL, 64, 1951, 20-36). The last exponent in the equation was misplaced outside the parenthesis instead of being within it. The formula should read:

$$r = [ [(N - 2d) / N] - x^2 ] / (1 - x^2).$$

It is probably worth noting that the following alternative equations:

$$r = 1 - [p(A, B) / p(A) p(B)],$$

$$r = p_A(A) - p_B(A);$$

are usually more convenient to use than the one we gave.

Harvard University

FREDERICK C. FRICK

GEORGE A. MILLER

The ordinates of Fig. 1 (Parts A and B), which appears on page 498 of our article in the October 1952 issue of this JOURNAL (The rôle of selected stimulus-variables in the perception of the unvoiced stop consonants *op. cit.*, 65, 1952, 497-516), are in error. The correct values, which are obtained by multiplying the values given there by a constant of 1.65, are 1650~, 3300~, 4950~, and 6600~.

University of Connecticut

ALVIN M. LIBERMAN

University of Pennsylvania

PIERRE DELATTRE

Haskins Laboratories, New York

FRANKLIN S. COOPER

### John Dewey: 1859-1952

The death of John Dewey—America's philosopher of democracy and social change, a man of immeasurable influence, who has written and been written about so much that he has passed beyond being a mere person and is on the way to becoming a subject—must not pass unnoticed in these pages for he played an important rôle in the establishment of the new psychology in America and in giving American psychology its special functionalistic slant.

Dewey was born on October 20, 1859, and died on June 1, 1952. As a young man in his mid-twenties he was teaching philosophy at the University of Michigan and noting that it was the custom of the 1880s to approach philosophy by way of instruction in psychology. Accordingly in 1886 he wrote and published a textbook of psychology in which he under-



took to use the factual material of the new scientific psychology, the latest acquisition of the German philosophers, as a means for introducing basic philosophical problems. Thus his new text began with psychophysics and the sensory equipment; it considered knowledge, memory, imagination, thought and intuition; and then it passed on to the problems of the will and control of the person—physical, prudential, and moral control. The last chapter is on the source of ideals and their realization. This book was undoubtedly an improvement on the then popular Upham's *Mental Philosophy*, but Dewey's important views about the wholeness of the reacting organism and about the adaptive use of mind had not yet matured. Nor was this book, although it ran to three editions in five years, sufficiently informed to serve to introduce the new psychology to America. Ladd's *Physiological Psychology* of 1887 and James' *Principles* of 1890 did that in magnificent and elaborate style. Dewey appreciated the convictions and the genius of the *Principles*, and he wrote, in 1942 at the centenary of James' birth, of James' "welding of scientific method with eager interest in every aspect of human nature." Dewey added: "It is this union which, in my opinion, renders James' *Principles of Psychology* a classic for all time." Thus both James and Dewey were pioneers in the American functionalist movement, the outstanding explicit systematists to be contrasted with such implicit functionalists as Cattell.

Dewey was at the University of Chicago for ten years (1894-1904). His 1896 paper on the reflex arc concept is, in a way, a declaration of independence for American functional psychology, but it is also, along with James' *Principles*, one of those wise early American writings which vigorously rejected Wundtian elementism and thus could be exhibited later, when Gestalt psychology crossed the Atlantic to protest about elementism, as American anticipations, though hardly as the ancestors, of Wertheimer's complaints about the Wundtians.

At Chicago, Dewey with James Angell, assisted by the philosophers G. H. Mead and A. W. Moore, founded American functional psychology—as the influential and wide-spread Chicago school was called for a couple of decades. That is to say, they gave explicit formulation to the notion of mind in use, to the adaptive function of consciousness in the adjustment of man to his environment, and to the conviction that the organism must be taken as a coördinated whole and not as a congeries of separate interacting parts. It is a long cry from Dewey and the reflex arc to modern field-theory; yet the essential dynamic principle of the field was there in Dewey's argument that you can not understand a stimulus without its response, nor a response without its stimulus, and that the discovery of the nature of the true stimulus is just as much a basic problem of psycho-



physics as is the discovery of the reflex consequences of stimulation.

After Dewey went to Columbia in 1904 his mission to the new psychology was largely accomplished, like the mission of any parent when the child has grown up. He was to become a great philosopher of democracy. Yet his early conviction that philosophers should be psychologists and psychologists philosophers persisted, and psychologists continued to recognize his importance and to honor him. In 1910 he was the fourth psychologist to be elected—as a scientist, not as a philosopher—to the National Academy of Sciences. The rôle of philosophers in founding American psychology is shown by the fact that the psychologists who preceded Dewey in election to the National Academy were Cattell (1901), James (1903) and Royce (1906). After Dewey the elections ran: Stanley Hall, Thorndike, James Angell.

In 1930 Dewey was the first William James Lecturer in Philosophy and Psychology at Harvard University. He was in residence in Cambridge for a term and his ten lectures were published under the title *Art as Experience*. In 1942 he agreed to participate in the Centennial celebration of James' birth at the Boston meeting of the American Psychological Association, but the meeting was never held because the war restricted transportation. Dewey sent in his paper for publication, an appreciation of James' *Principles*, just a page long. In 1946 Dewey attended the Philadelphia meeting of the American Psychological Association and the celebration of the semi-centenary of the founding of the Psychological Clinic. The University of Pennsylvania at that time added to his long list of honorary degrees. In general Dewey kept up his professional affiliations. He was a charter member of the American Psychological Association in 1892, being named then as one of the twenty-six outstanding American Psychologists, and he resigned in 1927, at the age of sixty-eight and after thirty-six years of membership. The Association had then just added the new class of Associates and was beginning to become large. In 1927 it passed the 500-mark in membership.

And now John Dewey is dead, appreciated and honored, an influential philosopher who fitted in with the course of the *Zeitgeist* in America, being molded by it and molding it. Man never attains the ultimate in wisdom, but he may progress continuously, as Dewey believed, in the direction of perfection, becoming then both the agent and the symbol of progress. That is, indeed, a proper specification for Dewey: he both promoted and represented the spirit of a democratic progress. In these activities it is clear that he influenced American psychology at a crucial period in its history much more than American psychology ever influenced him.

Harvard University

EDWIN G. BORING



## BOOK REVIEWS

Edited by M. E. BITTERMAN, University of Texas

*Thinking; An Introduction to its Experimental Psychology.* By GEORGE HUMPHREY. London, Methuen and Co., New York, John Wiley and Sons Inc., 1951. Pp. xi, 331.

In this fine book the author gives us what is perhaps the first thorough-going review of the experimental psychology of thinking. To most psychologists, I fancy, the results of the so-called 'Würzburg School' under Oswald Külpe were largely abortive. In showing that the process of thinking was something more and other than an association of 'sensations,' 'images,' and 'feelings,' the definition of a new 'content of thought' was both difficult and of waning interest to a psychology that was ceasing to be introspective. Behaviorism, which was rising into prominence at the same time that the 'School' was flourishing, had no use for 'contents of consciousness,' while those who remained phenomenologists began to talk in terms of 'integration' and 'Gestalt' without recourse to conscious 'elements.' As one who participated during the first decade of this century in experiments on thought and thinking, both in Würzburg and in his own laboratory, I was in the midst of the controversy which Professor Humphrey now describes and evaluates. Quite recently, too, before I knew of Humphrey's book, I published a memorial on the subject, "Oswald Külpe and the Würzburg School," in this JOURNAL (64, 1951, 4-29).

It is the merit of Professor Humphrey's careful study that he has been able to review the history of the experimental investigation of thinking from the early work of the Würzburgers, Binet, Bovet, Woodworth, and others, through the reinterpretations of behaviorist and Gestalter, and to re-point the problem as it concerns psychology today. The titles to the ten chapters of the book give us the outline of his review and his conclusions. The first chapter deals with Association. In it the concept is reviewed historically and criticized as an all-embracing explanation of thinking. The inadequacy of the doctrine of Association is summarized in saying that it is sensationalist and particular while thinking is general, it is mechanical while thinking is directive, and it is atomistic while thinking is relational and continuous.

The next three chapters are devoted to the work of the Würzburg Group and a critique of its experimental studies, following which there



is a chapter on the work of Selz. In addition to the discovery of thought as an 'imageless content of consciousness,' the Würzburgers relied upon the *Aufgabe*, or 'task,' operating upon reproductive tendencies, to explain the direction and content of thinking. "It is perhaps in his treatment of the *Aufgabe*," writes Humphrey, "that Selz has made his greatest contribution. Selz's rejection of the associational scheme implies a corresponding rejection of the dual action of *Aufgabe* and reproductive tendencies, with the former exercising a directive influence over the latter" (p. 137). With Selz we come closer to an understanding of productive thinking although, according to Humphrey, his explanation "is of too great generality to provide any particular psychological understanding of the processes involved" (p. 145). "He is," however, "the first psychologist to incorporate an explicitly non-associational doctrine into an experimentally induced psychology of thinking" (p. 149).

Chapter VI discusses the 'Gestalt theory of thought' which makes use of 'tension' and 'disequilibrium' to explain both the directive and productive character of thinking. The Gestalt explanation is sympathetically presented, although Humphrey is inclined to believe that too little regard is paid to association and habit. "For Gestalt theory," he remarks, "the motive force in thinking is the dynamics of the perceived problem-situation. Is there then no 'motor' in sheer habit?" (p. 182).

Chapter VII deals with 'Thought and motor reaction.' In it most attention is given to the works of E. Jacobson and G. L. Freeman on muscular relaxation and tonus. The disappearance of thinking with relaxation and its possible dependence upon tonus are discussed, but after consideration of Lashley's experimental animals the conclusion is reached "that we have to do with a central pattern which, to repeat, is prior to the particular kinaesthetic pattern which it engenders in a particular situation in that different motor patterns may be the result of the same central neural pattern" (p. 202). Experimental evidence is also adduced to show that thinking cannot be explained as silent speech.

Chapter VIII is devoted to a further consideration of 'Language and thought,' and the idea is developed that there is no problem of meaning in language. "... we perceive the world directly, without the intermediary of some *tertium quid* such as 'sensations' or 'ideas,' the latter giving rise to the Lockian fallacy, according to which we can never perceive the world at all; and that similarly we *imagine* the world directly, without the intermediary of images. . . ." Psychologists, writes Humphrey, have "invented for themselves an entirely spurious problem" (p. 226). "The psychological activities of sensing (something), imagining (something), or talking



about (something) (*i.e.* reacting with certain effectors to a situation comprising another person and something else), depend on the biological correlates *organism* and *environment*. The same kind of confusion has been caused in biology by those who have tried, often unwittingly, to consider an organism apart from its environment" (p. 228).

Humphrey's conclusion, "There is no Problem of Meaning," is perhaps the most significant one which his study of the experimental psychology of thinking has brought forth. He mentions it first in the introduction to his book and refers to an article with the above title which has appeared in the *Brit. J. Psychol.* (42, 1951, 238-45). It would appear that Humphrey was persuaded to reach this conclusion in the course of writing his book. A similar conclusion was reached by the reviewer many years ago from his own experiments on thinking. An article called "The Phenomenon of 'Meaning'" —published in this JOURNAL (34, 1923, 223-30) and listed in Humphrey's bibliography—contains the following passage: "We said that the stimulus-word is perceived; to the older view this only meant that it was *sensed* or *felt*; the interpretation came afterwards, when, through association, it had revived some other content or contents than itself. According to the newer view, the word itself, as sensed, is at once the *nucleus* of the experience, the dispositional matrix being its neurological counterpart. Phenomenally, that is psychologically, the 'felt' word is already its meaning; for *there are no meaningless experiences*" (p. 227).

It seems strange that so simple a conclusion should be so slow in gaining acceptance. Long since, the radical behaviorists made us familiar with the denial of 'conscious' contents as scientific data. Although, as Humphrey has shown, we have the same problem of *relevancy* in an 'objective' psychology that the Würzburgers tried to solve introspectively, it might well clarify our experimental procedures if we would exercise a stricter usage of that ambiguous term 'experience.' Humphrey does not deal with the problem of 'consciousness,' as such, and he might not be ready to accept my statement that "there are no meaningless experiences," but his conclusion that "there is no problem of meaning" enables him to consider thinking as an immediate process characterized by direction and operating by virtue of some kind of 'motor,' even though we do not yet know what kind.

Chapter IX, on 'Generalization,' considers the problems of abstraction, beginning with Külpe's pioneering experiments and continuing with those of A. A. Grünbaum, S. C. Fisher, E. Heidbreder, and others. Humphrey concludes that "the organism must be such that it discerns, in a measure



gradually increasing as evolution advances, similarities existent under the superficial variations in its environment. Only by such discernment of similarities can the organism survive; without it organic response must be chaotic in a chaotic-seeming world. The ability to discern and act upon similarities hidden beneath divergence is the ability to generalize, discussed in this chapter. It is, at bottom, the ability to learn from experience" (p. 307).

In his 'Summary and conclusion' Humphrey points up the problems of motivation and consequence in thinking. They appear to involve something more than is accounted for by 'tasks' and 'determining tendencies,' or 'trial and error' procedures that lack a clear definition of 'error' and 'success.' In his modest refusal to whip his extensive material into a theoretical conclusion, he contents himself with a 'General statement of the present position' which falls under sixteen headings:

"(1) *Thinking as the term is understood in this book may be provisionally defined as what occurs in experience when an organism, human or animal, meets, recognizes, and solves a problem.*" A footnote (p. 311) dilates on the ambiguity of the term 'problem.' It is also remarked that "there is probably no hard-and-fast distinction between learning and thinking (p. 312).

"(2) *A problem is a situation which for some reason appreciably holds up an organism in its efforts to reach a goal.*

"(3) *The process of thinking involves an active combination of features which as part of the problem situation were originally discrete.*

"(4) *It involves the use of past experience. The fact is obvious, but the method by which it comes about is still not decided.*

"(5) *Not only the method but also the form of the impression of the past into the present is under dispute.* One school maintains that the relation between past and present is particular (Hull: the 'continuing theory'), an opposing school that it is general (Lashley).

"(6) *There is ubiquitous 'trial and error' during thought-activity, whether animal or human, overt or covert. . . . The mechanism by which the 'wrong' solutions are rejected in overt learning is obscure; it is doubly obscure in thinking.*

"(7) *For purposes of psychological analysis, motive (motor) may be distinguished as an aspect of thinking. . . . Motive implies a goal. It might thus be said that for the thinker the problem-field becomes polarized towards the goal.*" In a footnote the remark is made that "conceivably this is the process of 'seeing the problem.' The problem is perhaps a problem because of incomplete polarization" (p. 313). On the next page, however, we read that "it is entirely possible that further developments will render the dichotomy [of the 'poles?'] no longer useful." One wonders if this recourse to 'polarization' is more than adventitious; at least Humphrey makes nothing of it.

"(8) *In addition there must be postulated some principle to account for the 'direction' of thinking.*

"(9) *The Würzburg group, under the direction of Külpe, developed the doctrine that thought-as-experienced is free from sensory content of any kind. . . . Since they*



are stated in demodé terms the Würzburg results are unacceptable in their original form to many modern psychologists. An alternative statement is proposed in the text.

"(10) *The Würzburg psychologists were inclined to underestimate the importance of the image. The image is a form of organization which is part of the more inclusive process of response.*

"(11) *The Gestalt theorists . . . have stressed production as against (associative) reproduction in thinking. At the same time they have developed the notion of the organism under stress to account for the motor of thought.*

"(12) *Even when the thinker is overtly still, traces of the matrix of activity in which thought has grown up still remain in the changes of muscular tonus observed by many experimentalists. . . . Thus tonus, like muscular activity in general, may under the right conditions help and under the wrong conditions hinder solution. . . . 'Thinking out' may clearly prevent a disaster that would have been precipitated by 'acting out.' That is why the 'thinking' method has won its evolutionary place.*" This is perhaps as close as Humphrey will come to an identification of thinking with bodily behavior. There remains, however, the somewhat ambiguous 'experience' of thinking.

"(13) *A specialized form of activity is speech, which at least in its derivatives, such as writing and mathematics, is peculiar to human beings. Clinical, experimental, and factorial results agree that language cannot be equated with thinking.*

"(14) *Generalization may be defined as the activity whereby an organism comes to effect a constant modification towards an invariable feature or set of features occurring in a variable context. Since all learning involves a context which is to some extent variable, the process is common to both learning and thinking. Like all kinds of thinking, generalization does not necessarily involve language, though it is often improved by language. (Query: Is it ever impeded by language?)*

"(15) Thus a number of different grades and kinds of organization are involved in the total response to a problem-situation; of these (1) images of various modalities; (2) muscular action, including, in particular, (3) speech, have been mentioned as such; to this list there should perhaps be added (4) concepts. The total process is in general facilitated by these organizations, but, apparently, cases occur where it is hindered by at least (1), (2), and (3).

"(16) An artificial problem of 'meaning' has been created by treating the *image* and *speech-activity* apart from their total context. (Conceivably the same kind of confusion has been created by treating the 'concept' apart from its environmental context, thus invoking the 'problem of the Universal.')"

One may hope that Humphrey's book will stimulate experimental studies calculated to answer the questions he raises. In his article, "There is no Problem of Meaning," he concludes: ". . . as well as perceiving an object directly, we may also imagine and think it directly. Are these then three distinct processes? three mental operations, psychological 'things' we do to the surrounding world? Probably not. The relation between perception, imagination and thinking has long been debated; it may turn out that they form a continuum, along which something like the internal and external 'forces' of the Gestalt psychologists play a greater or lesser relative



part. This also must at present be considered a problem for the future. At the present time it looks to be not insoluble" (p. 244).

May it be suggested, however, that before a problem of this sort is solved it must be more narrowly defined both as to its theoretical and experimental treatment? "Mental operations" and "psychological 'things'"—even with *things* in quotes—are what Humphrey, himself, might call "dangerous terms." In the old days it was a common practice to declare oneself an 'interactionist,' a 'parallelist,' or an 'epiphenomenalist.' After such a commitment one might have some notion of the possible relations of "perception, imagination and thinking" to each other and to "the internal and external 'forces' of the Gestalt psychologists." Lacking such a declaration, the process of thinking is ill-defined. With Humphrey's conclusion that "there is no psychological problem of meaning *per se*" the reviewer has been for a quarter century in agreement; but the statement that "With any philosophical problem that the term meaning may engender, this book does not concern itself" (p. viii) is dismaying. For is not the philosophical meaning of 'meaning' somehow prior to any psychological postulates of "perception, imagination and thinking," to say nothing of those ubiquitous "internal and external 'forces' of the Gestalt psychologists"?

Cornell University

R. M. OGDEN

*Cerebral Mechanisms in Behavior.* Edited by L. A. JEFFRESS. New York, John Wiley & Sons, 1951. Pp. xiv, 311.

This book is the edited transcript of the Hixon symposium that took place at the California Institute of Technology, September 25-30, 1948. It brought together a distinguished group of scientists, most of whom have contributed notably to research on the cerebral mechanisms of behavior. Those who contributed papers were: John von Neumann, W. S. McCulloch, R. Lorente de Nó, K. S. Lashley, Heinrich Klüver, Wolfgang Köhler, W. C. Halstead, and H. W. Brosin. (Lorente's paper could not, unfortunately, be included in the book.) Also contributing to the discussion, which was lively, lengthy and included in the book, were: R. W. Gerard, H. S. Liddell, D. B. Lindsley, J. M. Nielsen, Paul Weiss, A. van Harreveld, Linus Pauling, John Stroud, C. A. G. Wiersma, and Lowell Woodbury.

The reader will find relatively little in the formal papers that is not to be found elsewhere in the books or research papers of the contributors. He will, however, find the principal contributions of each of the participants nicely summarized, so that within the covers of one book he may learn



about or review work that would otherwise be covered only with great labor. More important is the fact that the contributors were working together to bring their different facts and approaches to bear on the common problem of understanding cerebral mechanisms. In the discussion among them and the other participants, one sees the differences in point of view, the weaknesses and strengths of their theories and interpretations, and countless suggestions for experiments to settle unsolved problems.

The first three participants, von Neumann, McCulloch and Lorente de Nó, were natural scientists, not psychologists, attempting to find explanations of psychological phenomena from their respective fields of science. The mathematician von Neumann led off with 'The general and logical theory of automata,' and his approach was to liken the brain to an automaton (or computer) and then to see how the logic of automata might be applied to nervous functions. After explaining the difference between analogy and digital computers and recognizing that the nervous system has some characteristics of both, he musters arguments for considering the brain in digital terms. He concludes that the brain is more efficient in size and energy than a comparable digital computer but less efficient in terms of speed and number of operations. In general, he points out, the brain has sacrificed efficiency to minimize error and reduce 'noise' and 'corruption' in its operations. Taking off from the McCulloch-Pitts theory that anything that is completely specifiable in words may be represented in formal neural networks, he works around to the interesting hypothesis that, in the end, neural networks may make the best logical statement (or definition) for words and concepts. He ends by considering the problem of automata reproducing automata and, by appealing to Turing's theory, shows that there is nothing impossible about such reproductions.

McCulloch begins his paper, 'Why the mind is in the head,' by developing Wiener's idea that the nervous system may be considered as an information-processing machine. As such, it corrupts and wastes information to an extremely high degree, but this is the price it pays for obtaining a high level of certainty. He goes on to say that all the logical consequences one sees in the relations between stimuli and behavior may be depicted by formal neural networks, but to explain memory, purpose, and so forth, it is necessary to introduce circles or reverberating circuits into one's schemes. He then proceeds to use the notion of circles to deal with the operation of reflexes, sensory adaptation, and psychological 'values'—the latter are viewed as interlocking, competing reverberating circuits. He also describes his model for the equivalence of cortical areas in representing shapes and



other characteristics of sensory stimuli that he has developed from a matrix of (digital) relays.

Since Lorente's paper could not be included, one can only infer from the discussion that it dealt in part with recurrent nervous circuits. The discussants considered at some length the question of whether such circuits can adequately explain memory. They were inclined to believe that such circuits were inadequate and that a more likely explanation was to be had in terms of molecular or other enduring changes in neurons.

Lashley returned in his paper, 'The problem of serial order in behavior,' to a problem on which he has published before. This time he considers it in more detail and presents some new suggestions for solving it. After presenting a detailed account of serial behavior with special emphasis on the syntax of language, he discards as inadequate the older explanations in terms of associative chains and determining tendencies. He argues for a priming of expressive units of the chain through spatial and temporal activity in the nervous system. He arrays evidence for the existence of a system of spatial coordinates for all nervous activity and then for some basic temporal rhythms, e.g., breathing and heart beat, occurring independently of external stimuli. These two factors, taken together and with other factors, may explain the intricacy of serial order in behavior.

Klüver's paper, 'Functional differences between the occipital and temporal lobes,' covers more ground than its title implies. It reviews the four major areas of research for which Klüver is noted: equivalence reactions in visual discrimination, 'psychic blindness' following temporal lesions, the effects of drugs on behavior, and his most recent research on porphyria and the role of the porphyrins in nervous function. Although these are apparently rather different areas of research, he ties them together with the concept of 'equivalence'—that different stimuli, different neural loci, and different chemical conditions can produce identical behavioral results.

Köhler's paper, 'Relational determination in perception,' gives a good account of his research on figural after-effects, including the later work on three dimensional and kinesthetic effects. From this work he develops the hypothesis of electrotonic electrical fields in the cortex, then summarizes his records of direct-current changes in the visual cortex recorded from human subjects while viewing visual stimuli. He and the discussants were uncertain whether the changes could be interpreted as electrotonic effects representing visual contours, but the evidence looked promising.

Halstead's chapter on 'Brain and intelligence' is a very readable summary of his book of the same title. It describes the four factors he has



obtained through a factorial analysis of his battery of tests used with brain-injured patients, and it gives concrete illustrations of the test-performance that they represent. This chapter has an interesting postscript on the work he later published on the possibility that memory and mental functions depend on reorganizations of protein chains in neurons.

The final chapter by Brosin, 'The Symposium from the viewpoint of a clinician,' points up differences and agreements in the presentations of the participants and discussants. It also stresses the values of interdisciplinary research on cerebral mechanisms. It presents some new problems from the psychiatric point of view that might well be tackled in further research.

It is not possible to review concisely the comments and discussion of papers, yet these are a most valuable part of the book. Many new ideas were presented and weighed in the discussions, and some of them are first-class papers in themselves. The student of neurology or psychology will find in them experiments he probably was not aware of and many ideas for research. They are also somewhat unique in recording in detail the informal 'thinking-out-loud' of leaders in this field.

In conclusion, the reviewer should attempt to present some over-all evaluation of the volume. The reader who is already familiar with the field of physiological psychology may be disappointed that the papers do not contain more that is new, but he will find it extremely useful as a compact summary of work otherwise spread widely throughout the literature. He will also profit by interaction of ideas and the critiques of research to be found here and by the suggestions for research. As a source of information, it will prove most valuable to our students and to our professional colleagues who are not expert in physiological psychology; for they can find here the best digest of modern thinking on cerebral mechanisms. Certainly this was a symposium worth having in the first place and the transcript of it is a very worthwhile addition to our literature in this field.

Johns Hopkins University

C. T. MORGAN

*A Hundred Years of Psychology 1833-1933: With Additional Part on Developments 1933-1947.* By J. C. FLUGEL. Second edition, with extended title. London, Duckworth and Co., New York, Macmillan Co., 1951. Pp. 424.

This is the old 1933 book, reprinted with only the slightest changes in the plates (even the Preface is unchanged by a single word), with 38 new pages added to cover the fourteen years up to 1947. Assessing the immediate past is always difficult and, when the immediate past includes psychology in World War II, the undertaking requires sheer bravery.



The face of psychology altered radically between 1941 and 1947, as indeed it has also between 1947 and 1951, a fact which explains why Flugel sounds a little vague and confused about what is going on, for the psychologist-reader expects Flugel to be writing as of 1950 in 1951, whereas Flugel's point of view is actually of 1947. Things have both changed and settled down since 1947 when Dennis brought together his symposium on *Current Trends in Psychology*, a book which Flugel does not cite.

Psychology during World War II was a disorderly variety of many diverse efforts, and this brief chapter can not but mirror its subject. Of Flugel's sixty-six names of active contributors during the period, the more representative half, in the order of occurrence, seems to me to be: F. C. Bartlett, Gardner Murphy, Ruth Benedict, Margaret Mead, R. B. Cattell, Cyril Burt, Godfrey Thomson, L. L. Thurstone, Harry Murray, Mark May, Lewis Terman, Gordon Allport, W. H. Sheldon, Wilhelm Dilthey, Eduard Spranger, David Katz, Franz Alexander, Robert Sears, Clark Hull, Anna Freud, Arnold Gesell, William Healy, Erich Fromm, Kurt Koffka, Kurt Lewin, Wolfgang Köhler, R. S. Woodworth, Walter Miles, E. D. Adrian, and J. B. Rhine. I refrain from the invidious listing of unmentioned American contributors to psychology in the years 1933-1947, men of the eminence of Tolman and Skinner, and of those others whose work came to prominence, perhaps not yet international, after 1940.

The topics of Flugel's addendum run thus: war research—training—selection—social problems—delinquency—propaganda—polling—attitudes—prejudice—culture—intelligence—tests—projective techniques—marital happiness—therapy—constitutional types—Geisteswissenschaften—instincts—drives—psychology vs. psychiatry—psychosomatic medicine—psychoanalysis—hypnosis—shock therapy—electroencephalography—lobotomy—group therapy—child psychology—Gestalt psychology—memory—conditioning—experimental psychology—extrasensory perception. Here, as always, Flugel has stressed social and abnormal psychology and the psychology of personality at the expense of the experimental psychology of learning and perception. With American research piling up in these two fields and bursting finally in Stevens' *Handbook of Experimental Psychology* (1951), the British bias makes the American psychologist pause and wonder. (And who would have thought that Beck and Miles' theory of the olfactory stimulus was the most important discovery in sensory psychology of this period?) Doubtless Flugel was limited in space by the frugality of his publisher, who gave him only these spare 38 pp. which



are not even included in or added to the old page-index.

Since I reviewed the 1933 edition, it was natural that I should look to see whether Flugel accepted my fourteen suggested changes, which I thought of as 'corrections.' Six of them are included in 1951, items that did not change a line, like making "1885" into "1886" for Mach's *Analyse* and changing "A. W. Moore" into "T. V. Moore," as well as altering "Clark University Press" to "American Psychological Association," a change which threw out a whole paragraph of 36 lines. The other eight suggestions were ignored. Does Flugel think I am wrong? Does he have information that I do not possess? Was Brentano really Lotze's pupil? Are the upper limits of auditory frequency of men and dogs actually about what Flugel says they are (even though 1951's 'truth' is different from 1933's)? Or did the publisher refuse to change the plates when many lines would have had to be shifted?

Flugel's historical volume is still a useful book, and it is wholesome for Americans to be given at times the British slant on psychology; but I hope that young British psychologists are not to learn about recent American psychology from this source. At least let them also read Wayne Dennis' symposia, *Current Trends in Psychology* (1947), *Current Trends in Social Psychology* (1948), and *Current Trends in Industrial Psychology* (1949).

Harvard University

EDWIN G. BORING

*Comparative Psychology*. By Various Authors. Edited by CALVIN P. STONE. Third edition. New York, Prentice-Hall, 1951. Pp. 525.

This is the third edition of a well known book formerly edited by F. A. Moss. Like its predecessors, this volume is a collection of many excellent and stimulating chapters, written by specialists on various aspects of the subject. Like its predecessors it lacks genuine unity and coherence—all too often the individual contributions appear as discrete and unrelated parts of a subject matter that is never clearly defined.

Just what is meant by comparative psychology? Within the context of present thinking two views appear: One approach takes the term at face value—the application of the comparative approach to psychological problems, phylum by phylum, order by order. Studied in this manner, behavior would be seen both in its relation to the particular, specific biological propensities of the individual organism and against the backdrop of a broad evolutionary perspective. This point of view is well illustrated by Maier and Schneirla's *Principles of Animal Psychology*, a book which, although published in 1935, is still worth studying. (2) Another position,



steadily growing in popularity, is based upon the proposition that comparative psychology is, in reality, non-comparative. Adherents of this view are interested primarily in general theories of behavior and are likely to regard interspecies differences as of minor concern. The laws of behavior, it is hoped, will be found general enough to encompass at least all mammals, leaving the choice of the specific experimental animal as essentially a matter of convenience.

If either of these positions had been adopted consistently, the present work would have benefited substantially. As it is, a few of the chapters are decidedly comparative in outlook, particularly Harlow's section on 'Primate learning,' Nissen's on 'Social behavior in primates,' and Marquis' on 'The neurology of learning.' Spence's chapter on 'Theoretical interpretations of learning' is clearly at the other pole. Most of the others, however, fall uncertainly in between. This lack of unity is unfortunate because so many of the separate sections are, in themselves, excellent.

Almost all of the individual chapters have been extensively revised and many of them have been written by new authors. Stone's discussion of 'Maturation and instinctive functions' has been reworked to include material published during the last decade. P. T. Young's section on 'Motivation of animal behavior' thoroughly reviews the field, although one might wish that greater prominence had been accorded to the rapidly growing literature on secondary drives, particularly the work of Miller, Mowrer, and their associates. Heron's chapter on 'Learning: General introduction'—a condensation of his two chapters in the second edition—is an excellent introduction indeed, as is Marquis' 'The neurology of learning.' A new section on 'Abnormal behavior in animals' is contributed by Patton, comprising material on experimental neurosis, fixation and regression, and convulsive behavior in rats.

Two new and outstanding sections are devoted exclusively to primate psychology. Harlow's 'Primate learning' is an exhaustive account of the learning capacities of monkeys and apes, which poses serious objections to the continued over-exploitation of lower species and throws doubt upon the applicability of theories based primarily on them. Nissen's section on 'Social behavior in primates' is an interesting attempt to find the *Anlagen* of human social phenomena at the primate level. Both chapters, in pointing to the tremendous richness and complexity of primate behavior, open altogether new avenues to a psychology of learning that for too long has seen its subject matter through the eyes of a rat. An entirely different approach is represented by Spence's chapter on 'Theoretical interpretations of learning,' an extremely lucid outline of modern theories and controversies. The



compact presentation of S-R-reinforcement theory and of its position on many critical issues make this an especially important—but difficult—chapter.

Although Stone's *Comparative Psychology* contains many fine sections, its organization leaves something to be desired. The book does not seem too well suited for use as an introductory text in comparative psychology. Used as a handbook for more advanced students, or as a source of collateral reading, it should serve a valuable function.

Swarthmore College

HENRY GLEITMAN

*Skill and Age.* By A. T. WELFORD. London, Oxford University Press, 1951. Pp. ix, 161.

Here is a stimulating little book about the nature of skill and the changes which occur with advancing age. It will find a waiting and appreciative audience in the serious students of aging who have been groping for a theoretical interpretation of accumulating facts about age-changes in perception, reaction-time, and other functions. Perhaps a larger audience for the book will be provided by industrial psychologists, engineers, and personnel specialists who will find here factual material about the on-the-job performance of persons in their later years and suggestions for industrial studies.

The book has three main parts: (a) theoretical views on skill and the nature of aging, (b) descriptions of the methods and results of laboratory studies of mental and manual skills, and (c) descriptions of the methods and results of industrial studies. Each part shows such a freshness of viewpoint and such ingenuity in devising experiments that it would seem that our British colleagues are more than a little ahead of us in providing both facts and a conceptual basis for their interpretation.

All of the experiments were designed to fractionate a total process or skill into component events so that deficits and compensations might be described and localized. To this end detailed records were made of all phases of each individual's total performance; component parts of a task were not isolated and studied out of context. Comparisons among age-groups were made for such measures as the number of discrete movements per unit time, the extent of movements, and the number of over- and under-corrections. The relation of age to the various components of performance were studied both in machine-paced and self-paced tasks.

The deterioration of skill with advancing age is regarded primarily as an inefficiency in organization of incoming data or a failure of the "... central mechanisms of the receptor side." Such an interpretation is quite



different from that currently advanced in America where age-decrements in performance are commonly regarded as a failure of peripheral processes and the 'core' considered to be minimally changed with advancing age. Welford and his colleagues do not deny age-changes or apologize for them; changes are studied objectively and related to what man does in daily life. In industry decline in performance appeared much later than did the defects which could be seen in the laboratory. The human subject is apparently able to make adequate compensation for his deficits while adding to his experience, as evidenced by the fact that some of the oldest individuals studied were able to do their jobs as well as those in their twenties or thirties. Older workers were found to be more careful and more suitable for industrial operations where accuracy rather than speed was important. Another interesting finding is that light machine-paced operations are more difficult for older persons than heavy non-timed work in which strength is a factor.

In attempting to outline the limitations of the book, one might, after reflection, list two. The first stems from a problem which plagues all workers on aging—the limited availability of older subjects. The Cambridge investigators, like others, have had to depend upon wide age-groupings and limited numbers of subjects and for this reason they have not been able to specify the rate of decline in performance with age or the form of the functional relation between age and other variables. The second limitation is to be found in the lack of specificity in defining the receptor and effector mechanisms. Ultimately we want to localize temporally and spatially in the nervous system any event in a total performance that we choose to discuss. Perhaps the use of schematic diagrams, however tentative, would help to make the theory more explicit and facilitate discussion.

The reviewer believes that many readers will wish, as he does, that Cambridge were not so far away. A visit to the laboratory would provide more insight into these new ideas than can be given in this short volume.

National Institute of Mental Health

JAMES E. BIRREN

*Propaganda in War and Crisis.* DANIEL LERNER, Editor. New York, George W. Stewart Publisher Inc., 1951. Pp. xvi, 500.

Contrary to its title, this fourth addition to the Library of Policy Sciences is an anthology of writings "selected and analyzed with the aim of clarifying some key problems that confront American policy." Propaganda enters the picture since it "is the use of symbols to promote policies" and it "is first and always an instrument of policy." When the reader forgets



the title and, instead, adjusts himself to the purpose of the book, the selections become more meaningful, their patterning becomes evident, and the book as a whole is seen as a worthwhile contribution to the understanding of problems of national policy. Perhaps, for the psychologist, the most significant contribution of the book is its panoramic presentation of national policy in which the psychologist can discover those points at which he can make unique and valuable contributions.

Each of the four parts of the book is introduced by a cursory two-page description of the selections included in that part and of their relation to the broader problem. Part I provides an orientation to the contemporary role of propaganda in, as Lasswell says, "peacefare and warfare." Part II describes the relation of policy decisions and 'enemy' intelligence to propaganda. Part III considers the relation of policy to the functioning of the propaganda agencies, the relation of policy to the personnel of such agencies, and the reciprocal effects of the functioning of propaganda statements and agencies on policy. Part IV presents some examples of propaganda evaluation, some appraisals of the effectiveness of propaganda, and a prediction as to the future.

Most of the 27 articles included in the anthology were published in the five-year period ending in 1949. Of the four previously unpublished articles in the book, comprising 16% of its pages, two are reprints of reports prepared by Psychological Warfare Division, SHAEF, and two were prepared especially for this volume by Lerner and by Harold D. Lasswell. The point of view of the book is most clearly seen by examining the professional backgrounds of the authors. Sociologists contributed 31% of the pages; political scientists and lawyers, 22%; psychologists and psychiatrists, 18%; government employees and officials (including officers of the armed forces), 15%; representatives of communication industries, 7%; members of the 'working press,' 3%; and economists, 1%.

The reason for the discrepancies between citations in the Table of Contents and chapter-headings, and for the omission of orienting statements at the beginning of each chapter, is not clear. The headings of chap. 7, 10, 11, 15, 17, 23, 24, 25, and 27 do not agree with the listings in the Table of Contents. More serious, however, is the omission of introductory statements to the individual selections. The selections were made to illustrate particular problems of policy, but the relation of selection to problem is indicated only briefly in the introductions to each of the four parts. In many cases the lack of a proper setting seriously handicaps the reader. For example, chap. 25, "The political situation in Aachen" (prepared at some time by PWD-SHAEF), begins: "We spent most of January in



Aachen . . ." What January? The selection has one meaning if it is 1945, but quite a different meaning if it is 1947 or 1950. We can infer that it is 1945, but this inference is based on a statement in the next paragraph that in "the last 3 months a new elite has emerged in Aachen," and on a statement 9 pp. later that the mayor of Aachen, the leader of this elite, took office on 30 October 1944.

Frequently, the chapters represent only opinion, and some orientation to the author and his background would aid in evaluating what is said. For example, chap. 26, "Appraisal," published in 1946 by James P. Warburg, states that propaganda had "negligible" effects on Japan's surrender, and that "the last touches to the collapse of the Japanese spirit" were added by the atomic bomb and the Soviet declaration of war. Yet chap. 19, "Decisive Broadcast," published in 1946 by Ellis M. Zacharias, describes Japan as being ready to surrender "*thirteen days* before the first atomic bomb was dropped on Hiroshima, and more than two weeks before the Soviet's entry into the war" and points out that "there was unanimity among Japanese newspapermen that our propaganda not only shortened the war but made the bloodless occupation of Japan possible." Could not the editor have given some information to aid the reader in evaluating such discrepancies?

Psychologists will be particularly interested in two chapters—7, "German Personality Traits and National Socialist Ideology: A war-time study of German Prisoners of War" (*Human Relations*, 3, 1950), by Henry V. Dicks, senior intelligence officer of the British Directorate of Army Psychiatry; and 22, "Social and Psychological Factors Affecting Morale" (*The effects of strategic bombing on German morale I*, United States Strategic Bombing Survey, Government Printing Office, 1947), by the USSBS Morale Division. The book as a whole should be read by all those who are interested in the problems of social relations and in the rôle of psychology in the work of interdisciplinary teams.

Knox College

ROBERT S. HARPER

*Science: Its Method and its Philosophy.* By G. BURNISTON BROWN. New York, W. W. Norton & Co., 1951. Pp. 189.

This volume was intended, as the author tells us in his Preface, to provide an answer to three questions: What is the Scientific Method? How has it arisen? What is the scientific outlook on the Universe in general today?

The first question is considered in two chapters on 'Animals' and 'Words.' Defining science as a special method for extending our knowledge of the world—as a special way of learning—the author provides a genetic



account of learning as it appears in the evolutionary scale. However good this account may be (and Brown seems quite at home in the psychological literature) its purpose is not apparent unless it is to show that animals below man are incapable of the kind of learning which science implies—the crucial difference lying in their inability to invent the symbols essential for abstract thought. It is in the *symbol-situation* that the origin of words and the origin of meaning are to be found. The author takes great pains to show that terms such as 'fact' and 'theory' must be carefully defined if the nature of the scientific method is to be understood. Brown himself provides definitions which should be acceptable to every scientist.

The second question is answered in terms of a summary of the views of major contributors to scientific thought. Aristotle was first to conceive the idea of organized research and one of the founders of the inductive method. The characteristic procedure of his geometrized science was to begin with axioms (generalizations based on particular instances) and from them to derive by syllogistic argument other essential attributes of nature. These deductions were never tested by experiment. Bacon, in the *Novum Organum*, contributed an emphasis upon experimentation as the only reliable method of science, but failed to appreciate the importance of hypothesis in observation and experiment. Newton, continuing in the Baconian tradition, developed a mathematical approach to the problems of physics and astronomy. Unlike Bacon, Newton, at least in his early years, realized the importance of hypothesis, but later changed his mind: *Hypotheses non fingo*. It was Whewell, in his *Novum Organon Renovatum*, who first clearly appreciated the limitations of a purely inductive science: "Hypotheses may be useful, though involving much that is superfluous, and even erroneous; for they may supply the true bond of connexion of facts; and the superfluity and error may afterwards be pared away." The fruitfulness of Whewell's orientation is illustrated by the research of William Charles Wells as described in his famous *Essay on Dew*. A chapter is devoted also to the philosophies of science of Eddington and Milne in which the postulational-deductive approach finds some of its most sophisticated expression. Their theories, however, deal chiefly with metrical science. Both make use of *identification* in order to establish a connection between elements of their deductive systems and the physical quantities known to the ordinary physicist.

The answer to the third question is provided in a discussion by Sagredo, Salviati, and Simplicius—the same gentleman to whom we have been introduced in Galileo's *Dialogues Concerning the Two Principal Systems of the World*—of the implications of the modern scientific outlook. They



have come a long way and learned a great deal. Science is 'the reliable way of arriving at true statements about the Universe.' Facts are propositions which are true, *i.e.* which can be verified. The possibility of verification is based on the assumption of the uniformity of Nature, without which scientific method would not be possible. There is much insistence upon clear definition of terms, and on the distinction between facts, hypotheses, and theories. Hypotheses and theories are inventions, suggested by experience, to explain facts. The difference between hypotheses and theories is that the latter are supported by a wider range of facts. A good theory is one which not only explains facts, but also suggests new experiments from which more facts can be derived.

Brown's book displays an unusual insight into the actual procedures of scientists and an exceptional understanding of their systematic problems. The author has a fine feeling for the essential; simplicity of style and economy of expression have made it possible for him to condense in this small volume a great deal of valuable material which cannot help but broaden and deepen the student's conception of modern science.

Ripon College

E. L. SALDANHA

*Gestalt therapy: Excitement and growth in the human personality.* By FREDERICK PERLS, RALPH F. HEFFERLINE, and PAUL GOODMAN. New York, Julian Press, 1951. Pp. xiii, 466.

The authors attempted to "develop a theory and method that would extend the limits and applicability of psychotherapy" and to "lay the foundation for the full application of Gestaltism in psychotherapy as the only theory that adequately and consistently covers both normal and abnormal psychology." The authors do not, however, develop methods which stem directly or solely from Gestalt theory. They utilize concepts and methods from the work of Freud, Adler, the neo-Freudians, Wilhelm Reich, Sullivan, Goldstein, and Korzybski. They justify calling their approach Gestalt therapy, in spite of this "eclecticism," by stating that their therapy aims to create "a wholeness and to do away with dualism in the person and to create a new whole." To this end they shift the concern of psychiatry from "the adoration of the 'unconscious,' to the problems and phenomenology of awareness." The concept of awareness is faintly related to Gestalt psychology. Awareness is said to be characterized by contact, sensing, excitement, and Gestalt-formation.

The book is divided into two volumes. The first deals essentially with an attempt to get the reader to engage in certain exercises involving situational- and self-analysis. The general aim is to develop or, rather, to re-



create in the reader the "Gestaltist mentality," which is characterized as a unitary outlook, the original, undistorted, natural approach to life. There are specific techniques which are aimed at helping the reader to contact the environment, others are aimed at developing and directing awareness, and still others are concerned with giving the reader experience in manipulating the self. These same techniques are involved in the proposed therapeutic procedure. One who is well set in his ways of psychotherapy may dismiss this approach as just so much hocus-pocus, but the experimentally oriented therapist may get some hints for the development of new methods for individual and group therapy.

Volume II of the book attempts a formal statement of theory, but is not very systematic—after all, the book is but an *attempt* at laying the basis for the development of a theory. The presentation includes a critical and novel evaluation of psychoanalytical theory. The nature of man, the nature of society, and the nature of the relationship between the two is not that portrayed by such Gestalt psychologists as Wertheimer.

In brief, this book makes use of some concepts of Gestalt psychology but is not basically a Gestalt approach. It utilizes in a descriptive rather than in an explanatory sense such concepts as figure-ground, Prägnanz, tendency to closure, and tensions created by interrupted tasks. The book's strength lies in its methods and in its appeal for a different approach in psychotherapy. It may conceivably serve as a stimulus for the development of new methods and new concepts in psychotherapy.

McGill University

ABRAHAM S. LUCHINS

*Mental Hygiene in Teaching.* By FRITZ REDL and WILLIAM W. WATTENBERG. New York, Harcourt, Brace and Company, 1951. Pp. xiii, 454.

In one way or another practices aimed at fostering mental health have to be woven into the fabric of our common life for they cannot be left to the expert. A message of mental health carried through the schools has a maximum chance of reaching the home. The aim of mental hygiene is a kind of toughness with tolerance which makes people feel at home in the world, *not* passive conformity and acceptance of group-patterns without question. This is the underlying theme of a book in which Redl and Wattenberg, two Wayne University professors, attempt to convey some basic principles of mental hygiene as it relates to the teacher's work with young people.

Fundamental concepts are developed in chapters upon behavioral mechanisms and developmental tasks (chap. iii and iv). The theory is stated simply and runs something like this: Impulses or desires push in



one direction; concepts of what would be wise or acceptable press in other directions. Coping with the resultant internal clashes is a critical part of all human existence and, in some fashion, control must be established over impulses. One type of control arises from insight into consequences; another from conscience, the deeply ingrained sense of right and wrong; and still another from the ideal self.

Behavioral mechanisms are somewhat stereotyped ways of coping with the outside world and the world within oneself. Mechanisms of denial—repression, rationalization, projection—enable one to ignore or overlook unpleasant facts. Mechanisms of escape—withdrawal, intellectualization, compulsive behavior, regression, and psychosomatic reactions—permit evasion of potentially unpleasant situations. Mechanisms of shift and substitution—displacement, compensation, sublimation, and identification—alter the form of the conflict. All can be found in normals, but any one or combination of these mechanisms may be employed to the point of dominating a person's life and removing him from effective relationships with people about him. The mechanisms can be observed in the behavior of an entire group as well as in the actions of individuals.

Development appears to be a matter of stages and phases, each with a specific task to fulfill. An appreciation of the developmental tasks of children and adolescents permits the teacher to understand the purpose of an otherwise puzzling sequence of behavior and to estimate how long it probably will last. Two strands, those regarding sex-role and concepts of the self, are traced through from early childhood through adulthood to illustrate the continuity of developmental processes. Emphasis is placed upon the recognition by the individual of his own identity as a separate being with a will of his own and, at the same time, of his involvement in what happens to other people and to the groups to which they belong. The psychological outcome is a self-image, the individual's perception of himself as he thinks he is, which may or may not fit with the ego-ideal.

The reader is prepared for the theoretical portion by eleven brief case-profiles and a discussion of mental hygiene in Part A. The remaining chapters in Parts B, C, and D lean more or less heavily upon the concepts of behavioral mechanisms and developmental tasks. Notions of adjustment, maturity, and normality, all taken in a relative sense, are developed after consideration of influences which shape lives and bring about distortions of personality. In the part devoted to applications, the chapters on group living and upon the psychological roles of teachers reflect the research and professional experiences of the two authors. The last part on specific problems, like most of the preceding portions of the book, is



illustrated by case-materials and anecdotes of typical situations.

*Mental Hygiene in Teaching* takes less account of the learning process and is less specific about techniques of inquiry and ways of working with children than *Fostering Mental Health in Our Schools*, the 1950 Yearbook of A.S.C.D. The reactions of a small number of teachers who have read the two books during the past summer would seem to indicate that both communicate and make sense to them. *Mental Hygiene in Teaching*, however, is a text. It thus has certain advantages over the yearbook for the psychologist assigned to teach a course in mental health at the senior level for teachers-to-be and others returning for a master's degree. Suggestions at the ends of the chapters refer the reader to a few additional books and to audio-visual aids. The only references to research are to be found in occasional passages and footnotes. One appendix is devoted to sources of help and information, the other provides a glossary of frequently used psychiatric terms.

University of Texas

CARSON MCGUIRE

*Grundriss der Religionspsychologie*. By WILLY HELLPACH. Stuttgart, Ferdinand Enke Verlag, 1951. Pp. 212.

*Sozialpsychologie*. By WILLY HELLPACH. Third revised edition. Stuttgart, Ferdinand Enke Verlag, 1951. Pp. viii, 215.

Both volumes are presented as textbooks. It is unlikely, however, that either will arouse much interest in this country; and it is certain that, even if competently translated, neither would find a place on an American undergraduate reference shelf. This implies no condemnation of the author; it indicates merely that the times have changed and that America is not Germany. For more than fifty years Professor Hellpach has been a successful teacher and a prolific writer. He has published on social psychology, folk psychology, clinical psychology and the psychology of religion. These volumes, he says, are his last contributions. They reflect the thought and the concerns of an older generation. They are full of German neologisms, with almost exclusively German references, and they show little appreciation of the recent developments in psychology, in Germany or in any other country. They are worth reading, however, if only because they remind us that there are psychological problems about which this generation has forgotten.

Cornell University

ROBERT B. MACLEOD



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## THE RÔLE OF THEORY IN EXPERIMENTAL PSYCHOLOGY

By EDWIN G. BORING, Harvard University

The dedication of a great new psychological laboratory turns the mind back upon the history of experimental psychology, and how the need for separate laboratories arose, and how the business of laboratory founding prospered and continued in the 1880s and 1890s, first in Germany, begun by Wundt's new Psychologisches Institut at Leipzig in 1879, and then in America—only four years later, mind you—by G. Stanley Hall's new Psychological Laboratory at Hopkins in 1883, exactly seventy years ago.

The early psychological experiments were for the most part sensory. For the first two-thirds of the nineteenth century experimental psychology, such as it was, went on in physiological laboratories. The course of development of interest was: physiology, the nervous system, sensory nerves, sensation. The alternative would have been: nervous system, motor nerves, movement, behavior, conduct, ending up with discriminatory response instead of sensation, but history chose the former, not the latter course by which to reach the present. This early special concern with sensation was promoted by philosophy which in the eighteenth century accepted empiricism, with sensation as the avenue of communication between the mind and the outer world. So it came about that from 1800 to 1850, say, systematic psychology belonged to the philosophers, the empiricists and associationists among them, while experimental psychology was practiced by certain physiologists. After that, ostensibly under Wundt's influence but perhaps more as an inevitable sequence in the *Zeitgeist*, the German philosophers took over the experiments, founded the laboratories, and developed experi-

\* An address delivered at the dedication of Mezes Hall at The University of Texas on April 1, 1953. Professor Boring represented The Society of Experimental Psychologists which held its 1953 meetings, as part of the dedication, at The University of Texas.



mental psychology, properly named *physiological psychology* in accordance with its lineage, under the aegis of philosophy.

It was William James who made America conscious of the new German movement, and very soon America was following close behind Germany in its practice of psychological experimentation, although always at first with its eye on German leadership. By the end of 1892 there were fourteen laboratories of experimental psychology in America, and by 1898 there were twenty-four, the latest being the laboratory at the University of Texas.<sup>1</sup>

The Germans continued the liaison of experimental psychology with philosophy, and in 1905, when Emerson Hall was dedicated at Harvard, Wundt wrote Münsterberg congratulating him that the new laboratory was still to be affiliated with philosophy and not to "migrate to the naturalists."<sup>2</sup> Most of the leaders of the new psychology in America, however—nearly all but James, Münsterberg and Ladd—sought for psychology's emancipation. Cattell and Titchener, poles apart on most matters, were agreed on this, and gradually the departments of philosophy and psychology got separated, though Harvard's psychological laboratory moved out of the philosophers' building only in 1945 and Texas has now built a new laboratory and taken its philosophers in.

While the adolescent new psychology was seeking emancipation from its parent, it was also exhibiting its inheritance. The talk in the new laboratories turned on theories and systems. All the new textbooks followed the pattern of Wundt's systematic *Physiologische Psychologie*. The word *system* then meant two different things: coverage and consistency. The texts tried to treat of every chapter known to be proper to psychology, but they also tried to create a definition of psychology and to remain epistemologically true to it. The later systems of this sort were Titchener's in 1910, Watson's in 1919, and McDougall's in 1923. Withal there was an increasing effort to avoid epistemological discussion and elaborate theorizing and to get down to concrete fact, with the uncontroversial standard textbook of physics as a model. In 1935 two colleagues and I, thinking that the victory of fact over theory was at last ready to be consummated, blithely published a textbook, written by a congeries of experts, and called it *Psychology: A Factual Textbook*, meaning to leave out all controversy and speculative theory and to give the students only the depersonalized facts of psychology as they had been accumulated in the new science. We man-

<sup>1</sup> C. A. Ruckmich, The history and status of psychology in the United States, this JOURNAL, 23, 1912, 517-531, esp. 520.

<sup>2</sup> H. Münsterberg, Emerson Hall, *Harvard Psychol. Studies*, II, 1906, 3-39, esp. 31.



aged pretty well as to coverage and depersonalization, but the *Zeitgeist* was against us, and I think now, a mere twenty years later, that I was insufficiently aware of the true nature of science, as I shall describe it in the exegesis that follows.

In the 1930s the systems changed. They became set less on coverage and more on consistency. It was Tolman's *Purposive Behavior in Animals and Men* in 1932 which initiated the new trend. Interest shifted to learning and then expanded to include action. Tolman (not unlike Jennings in 1904) was able to see in behavior such new phenomena as purpose, phenomena that thitherto had been regarded as conscious. In 1936 he introduced into systematic thinking the "intervening variables," new concepts to replace the now obsolete mental processes. Hull, with a belief in the power of formal logic, also became interested in learning and behavior. His latest, and, alas, his last contribution is his posthumous *A Behavior System* of 1952. Tolman and Hull did not always agree and they both inspired disciples, some of them now distinguished in their own right. Others came in. Soon the social psychologists were trying to create systems for their special subject matters. At the present time you can find laboratories where concern with theory and systems is so great that the divorce from philosophical heritage seems little greater than it was fifty years ago.<sup>3</sup> Is that wrong? Or, in view of what has happened to theoretical physics since relativity theory took hold of it, may it be good?

There are indeed other laboratories where theorizing with hypothetical concepts and the building of systems is regarded with strong disapproval.

<sup>3</sup> An especially wise, mature, and effective discussion of recent contributions to psychological theory is given by Gustav Bergmann, *Theoretical psychology*, *Ann. Rev. Psychol.*, 4, 1953, 435-458. Of Bergmann's 40 references, the following are most important: Bergmann, The logic of psychological concepts, *Philos. Sci.*, 18, 1951, 93-110; J. S. Bruner and Leo Postman, Perception, cognition, and behavior, *J. Personal.*, 18, 1949, 14-31; Egon Brunswik, *The Conceptual Framework of Psychology*, 1952 (International Encyclopedia of Unified Science, I, no. 10); David Krech, Notes toward a psychological theory, *J. Personal.*, 18, 1949, 66-87; M. H. Marx, Intervening variable or hypothetical construct?, *Psychol. Rev.*, 58, 1951, 235-247; P. E. Meehl and Kenneth MacCorquodale, On a distinction between hypothetical constructs and intervening variables, *ibid.*, 55, 1948, 95-107; B. F. Skinner, Are post-theories of learning necessary?, *ibid.*, 57, 1950, 193-216; K. W. Spence, The postulates and methods of 'behaviorism,' *ibid.*, 55, 1948, 67-78; E. C. Tolman, Operational behaviorism and current trends in psychology, *Proc. 25th Anniv. Celebr. Inaug. Grad. Stud.* (Univ. So. Calif.), 1936, 89-103. Then there is the compilation of 47 reprinted articles by M. H. Marx, *Psychological Theory*, 1951, a collection which includes the papers by Marx, Meehl and MacCorquodale, and Tolman, cited above. There is also the excellent discussion of learning theories by K. W. Spence, *Theoretical interpretations of learning*, *Handbook of Experimental Psychology*, 1951, 690-729, which has a bibliography of 144 titles. Then there is W. Dennis, R. Leeper, H. F. Harlow, J. J. Gibson, D. Krech, D. M. Rioch, W. S. McCullough, and H. Feigl, *Current Trends in Psychological Theory*, 1951, esp. Feigl, 179-213.



The new fury of theories, say these single-minded experimentalists, is a regression, taking us back into the slough from which we have just struggled. Are these critics right? What are we to think?

Because there is this uncertainty, I ask you now to inventory psychological and scientific theories with me. Let us see what kinds there are and what rôles they play in experimental science and especially in psychology; yet first let me lay down a basic dictum which my exposition should eventually justify. It is this. *The hypothetical constructs pervade science. They are the stuff of which it is made. The difference between being theoretical and empirical is mostly a question of how far the process of reification of the constructs has progressed.* Now to our list.

(1) *Theories with no evidence.* Let us begin at one extreme by observing that there are theories that have no empirical basis whatsoever. Any mystic can dream up such a theory, but the pressure of western culture is so great nowadays that he is almost bound to find some rationalizing evidence for it. It then becomes difficult to say whether the evidence produced the theory or the theory the evidence. Thus I may perhaps be forgiven for once again ushering onto the stage those hard-worked stooges of philosophic interpretation, the rustic empiricist and the rustic rationalist, who for the first time saw a real elephant. The empiricist said (as surely everybody now knows): "Gee, I always thought elephants were yellow; what's the use of thinking anyhow?" And the rationalist said: "Hell, there ain't no such animal!" There you have two theories formed without evidence, the theory of yellow elephants, and a general taxonomic theory that precludes the existence of an elephant.

(2) *Theories with rationalized support.* Very close to these unfounded theories are theories which are strongly held but in which the evidence is scanty and seems, at least in part, to have been dug up as rationalization. Voliva supported his theory of the flat earth by a photograph of twelve miles of the shore line of Lake Winnebago; you could see, he argued, that the shore is horizontal and not bowed. There have been many theories that the earth is hollow, and Teed's view that not only is it hollow but that we live on the inside he supported with evidence as well as invective against the bigoted scientists who would not accept his theory. Animal magnetism grew out of astrology with little empirical basis, but think how much evidence for it was being adduced when it got into Mesmer's hands.<sup>4</sup> Anyone who has examined Velikovsky's *Worlds in Collision* knows how packed with evidence and argument that book is. These men qualify as cranks since they show a paranoid strain when their enthusiasm is rejected or ignored by orthodoxy.<sup>5</sup> Does the great Goethe also belong here with his brilliant and preposterous arguing through two decades and two volumes that Newton was wrong on the basic theory of color mixture and that his own inadequate theory should be substituted?<sup>6</sup>

<sup>4</sup> On Mesmer and animal magnetism, see E. G. Boring, *History of Experimental Psychology*, 2 ed., 1950, 116-123, 130 f.

<sup>5</sup> A great wealth of data on scientific cranks, unorthodox theories and unpopular enthusiasms, ranging from dianetics to ESP, is given by Martin Gardner, *In the Name of Science*, 1952.

<sup>6</sup> On Goethe's color theory and his bitterness about Newton's, see Boring, *Sensation and Perception in the History of Experimental Psychology*, 1942, 112-116, 123 f.



(3) *Theories with insufficient evidence.* There are also the theories that are based on insufficient evidence and are entertained with but a low level of confidence. This group contains the plausible suggestions in addition to theories once strongly held but now on the way out. Take the theory of recapitulation, the conception that man in his development recapitulates his phylogenetic history, morphologically in the embryo and also psychologically in the neonate and the child. Haeckel and Spencer originated this theory, but it was Stanley Hall who held most specifically that the child's life echoes his phylogenesis, showing both aquatic and arboreal reverberations. This theory Hall entertained gladly but without too great certainty. There was evidence for it but the evidence was not controlled.<sup>7</sup> Hering's theory of color vision, vigorously enough defended when it was new in the 1870s, was still held at the beginning of the present century, held in spite of such contradictions as the failure of primary green and primary red to appear to be complementary, held weakly by those who found no better theory to displace it. Here we meet up with J. B. Conant's principle of the horror vacui for scientific theories: an established theory is not displaced by contradictory facts but continues in use until there is a better theory to supplant it.<sup>8</sup>

(4) *Hypotheses that can not be tested.* In the modern design of experiments it is customary to form hypotheses which can be tested by experiment. The hypothesis is derived by intuition or induction from observations or experiments. The tests consist of experiments which verify or fail to verify a particular difference that has been deduced from the hypothesis and is thus predicted by the hypothesis. Such hypotheses are, in the broad sense of our present usage, theories—theories to be confirmed or discarded or kept around for future test.

Now among these hypotheses are those that can be formulated but not tested. Mostly the reason for the inscrutability of such hypotheses lies in the fact that they contain terms for which there are, or seem to be, no available observational operations, terms which therefore lack practicable operational definitions. An example is McDougall's drainage theory of attention which assumes a limited amount of cortical energy ('neurokyme') that can be displaced in the cerebral cortex as attention moves from one focal object to another.<sup>9</sup> This theory was attempting a physiological explanation of the limited range of attention, of the fact that attention to one thing always requires the drainage of attention from others; but there was, when this theory had its vogue, no operational definition of neurokyme. Bridgman might therefore have called the concept meaningless;<sup>10</sup> nevertheless cortical energy could have been defined as concentration of electrical potential, and the operations for observing the degree and locus of such a concentration could have been specified even though they could not then be applied to the brain. (Similarly it has been argued that the other side of the moon does not exist because it can never be seen.) In the same way, it could have been said that Köhler's psychocerebral theory of isomorphism was meaningless because there was no way of observing electrical concentration of

<sup>7</sup> On G. Stanley Hall and phylogenetic reverberation in the child, see Boring, the influence of evolutionary theory upon American psychological thought, in Stow Persons, *Evolutionary Thought in America*, 1950, 268-298, esp. 279-282.

<sup>8</sup> J. B. Conant, *On Understanding Science*, 1947, 84 et passim.

<sup>9</sup> William McDougall, The state of the brain during hypnosis, *Brain*, 31, 1908, 242-258; reprinted in part in McDougall, *Outline of Abnormal Psychology*, 1926, 102-115.

<sup>10</sup> P. W. Bridgman, *Logic of Modern Physics*, 1927, 3-31.



charge on the brain; but now Köhler has discovered a method.<sup>11</sup> (And similarly some day a human being might see the other side of the moon from a space ship.) It would, in my opinion, be a mistake to exclude from science concepts with operational definitions in which the operations appear to be impossible. Sometimes the operations become possible with new discovery of techniques. Sometimes they can be approximated.

As psychology turns more and more to the use of statistical concepts in its theories, it finds itself relying more and more upon operations which are not literally possible, because the probability concepts of statistics have an infinite dimension which can not be realized in practicable observation. If an event in a series has a probability of  $p$ , the theory asserts that the frequency of the occurrence of the event will converge upon  $p$  as the number of occasions increases; yet no observed frequency can ever disprove this theory because convergence is asserted only for an infinite number of cases.<sup>12</sup> All use of probability theory as scientific model suffers from this disability.

Is the case then different with Bohr's principle of complementarity (which includes Heisenberg's principle of uncertainty), the principle that certain values in quantum mechanics, like the momentum and position of an electron can never be observed for exactly the same moment? Bohr thinks that we must agree that two complementaries that cannot be observed simultaneously also can not exist simultaneously, and Bridgman should agree with him. Einstein, on the other hand, thinks the two exist simultaneously although they can not be observed together.<sup>13</sup> Modern physics moves toward Bohr's view, but the psychologist, accustomed to meeting up with so many useful unobservable concepts, may find himself inclined to linger with Einstein.<sup>14</sup>

(5) *Hypotheses that can be tested.* When an hypothesis gets confirmed, however, it becomes a fact. These constitute our next class of theories: hypotheses that are confirmed, or are partly confirmed, or can be tested for confirmation if the necessary time and trouble are taken. Seventy-five years ago there was the pretty good theory that the functioning of the occipital lobes of the brain is essential for visual perception in man; now that theory is confirmed and a fact. At present there is another theory that patterns of retinal excitation correspond topologically to patterns of occipital excitation, but that theory still needs more confirmation as to detail. It is not quite yet a fact. W. H. Sheldon has a theory that relates tripolar patterns of temperament to tripolar patterns of body type.<sup>15</sup> There seem to have been a number of gross approximations obtained for this theory, although with a good deal of variance and with the publication of much of the observation still lying in the future. Certainly this hypothesis is still just a theory on its way to becoming some kind of

<sup>11</sup> Wolfgang Köhler, R. Held, and D. N. O'Connell, An investigation of cortical currents, *Proc. Amer. Philos. Soc.*, 96, 1952, 290-330.

<sup>12</sup> Boring, Statistical frequencies as dynamic equilibria, *Psychol. Rev.*, 48, 1941, 279-301.

<sup>13</sup> E. H. Hutten, On existence and complementarity in physics, *Amer. J. Physics*, 11, 1943, 328-334.

<sup>14</sup> Thus Einstein thinks of an ultimate truth as lying behind the limitations of man's techniques of observation. He is quoted as having said on probabilistic concepts' being due to man's restricted observation: "Der Herr Gott würfelt nicht!" ("The Lord God does not play games of chance.")

<sup>15</sup> W. H. Sheldon, *Varieties of Human Physique*, 1940; *Varieties of Temperament*, 1942.



a fact. It is these still unconfirmed theories that attract attention and interest, but we shall do well to remember that every established fact is a relation and was a theory if it once existed unconfirmed in a tentative state.

(6) *Generalization as theory.* If a fact is a confirmed theory, then a scientific generalization is a theory. Let us take this matter back to its bare fundamentals. The question is sometimes raised as to when in history science began. Sarton thinks science emerged with primitive man long, long before history was recorded, but in that he is, I think, much too conservative.<sup>16</sup> Surely before man animals had scientific theories, generalizations about variable nature. Here I am suggesting, with the contributions of Gestalt psychology in mind, that an object is a theory about an invariance in ever changing and chaotic experience. Since size-constancy has been found for chickens and apes, it is plain that they know how to make the basic classifications of objects as to size, that they act in accordance with a theory of the invariance of objective size.

It follows, of course, that any Aristotelean classification is a theory and often a fact because it brings the classified item into relation with other items, thus transcending the particulars of observation as all theories must do. The Galileans, of course, are right about the next step in scientific sophistication, but you have to have generalization first. At this level the much snubbed faculty psychologists were right. It used to be said that to account for a memory by referring it to a faculty of memory is a meaningless tautology, but actually what you are doing here is to describe an event as a memory, thus classing it with other events and abstracting it from still others. The modern faculty psychologists who practice factor analysis engage in the same kind of description and claim rightly that a factor analysis is a theory, one which could become a fact.<sup>17</sup>

In brief, a generalized description is a theory. This meaning for the word *theory* is admitted by those who discuss the philosophy of science, although for the most part they prefer to limit the term to the more complex cases, the theories that exhibit the interrelations among abstract concepts. I am insisting on the broader meaning because I am arguing from continuity. I am saying that concepts are created by inductive generalization, that science is made up of confirmed relations among concepts, not among data, that theory is so pervasive that it penetrates even the observational instant, when the observer decides whether to classify his black-white perception as 10.7 or 10.8 on the ammeter scale.

(7) *Systematic classification as theory.* Thus we can pass at once to the theories that use systematic constructs as classificatory concepts for the data of immediate observation. In Titchener's laboratory at Cornell you used to learn to identify sensations in order to note their occurrences in your introspective reports. Later there was doubt whether you were really observing sensations or only their attributes. You would think the observer could tell the nature of what he observes, but, of course, he could not. He had to be trained in classification, which is what introspective training was.<sup>18</sup> It was the same in the Harvard Psychological Clinic twenty years later when they had invented for classification and communication a special set of

<sup>16</sup> George Sarton, *History of Science: Ancient Science through the Golden Age of Greece* [I], 1952, 3-18.

<sup>17</sup> C. C. Pratt, Faculty psychology, *Psychol. Rev.*, 36, 1929, 142-171.

<sup>18</sup> Boring, *Sensation and Perception* (op. cit., 1942), 22-25, 48 f.



terms for needs and other motivational concepts.<sup>19</sup> When you knew how to use the list, you could spot particular needs in protocols with relative ease, just as the psychoanalyst keeps seeing complexes to which the layman is blind. I remember how a professor of genetics many years ago showed me published drawings of cell nuclei dated both before and after the discovery and description of chromosomes. Chromosomes kept showing up in the later drawings, not in the earlier. In other words, microscopes do not reveal concepts until the concepts have been invented. The shape of a chromosome is as much a theory to the histologist as is the size of a grain of wheat to a chicken.

(8) *Descriptive theories.* Next come the theories that are more than generalizations and the classification of data as concepts and that stop short, nevertheless, of the introduction of new hypothetical concepts. These are descriptive theories, which describe not merely data and simple phenomena, but principally the functional relationships among data, relationships abstracted from phenomenal experience by the experimental method. Here is where Skinner, by his own vehement choice, comes in. His theories of behavior are the generalized functional relations, the curves plotted between time and cumulated responses, when reinforcement and extinction are introduced in accordance with particular experimental designs. Skinner asks: "Are learning theories necessary?"; and he means: Is it necessary to go further to intervening variables and hypothetical constructs, after the manners of Tolman and Hull, in order to have a scientific knowledge of the facts of learning, or may one stand firmly on this positivistic base with the facts clear before him and almost no hypothetical superstructure at all?<sup>20</sup> To these questions no categorical 'yes' or 'no' is possible. Skinner exhibits a great many important relationships in behavior and, standing closer to his data, he enjoys maximal security; yet this is not to say that learning theory could not conceivably advance further from the data. It might gain power by moving in Hull's direction, or, as Skinner himself suggests, rational equations might be discovered for some of these functions. Inevitably the terms of rational equations would require the creation of new theoretical parameters representing them. Otherwise the equations would not be rational. Skinner's position is sound and safe for the time being, yet I can not imagine psychologists being content not to progress further if they can find stable concepts which eventually gain enough versatility to make them seem real—as electricity and atoms do now.

The preceding three cases, (6)–(8), and some instances of the still earlier ones delineate theories that are, in a sense, self-contained, that do not, in Skinner's phrase, appeal to a second dimensional system for explanation. These are the descriptive theories, the ones that are sometimes not called theories, the theories that Skinner is espousing when he asks "Are theories necessary?" There are also, however, the theories which are often called *explanatory*, in contradistinction to *descriptive*, the ones where the generalized data in one system are explained, as the phrase is, by terms in another system. The next five classes of theories, (9)–(13), are of this kind. It is with them that the current discussion of theoretical psychology is ordinarily concerned.

<sup>19</sup> H. A. Murray, *Explorations in Personality*, 1938; see glossary, 743-750, for a list of 79 special concepts.

<sup>20</sup> B. F. Skinner, Are theories of learning necessary?, *Psychol. Rev.*, 57, 1950, 193-216.



(9) *Analytical theories.* Of these correlational or parallelistic modes of explanation the best known is undoubtedly the explanation of the molar by reference to the molecular, or elementism, as it has been more often called. Water is  $H_2O$ . The whole is simply the sum of its parts. Chemistry got far with such theories, and first the associationists and then Wundt tried to do as well for psychology with a mental chemistry. Not so many now can recall the assured faith of the introspective elementists only fifty years ago. "The Elements of the Mental Life." "Sensation and Feeling as the Basic Forms of Psychic Elements." "The Structure of Sensory Ideas." "Intensive Ideational Connections." "Psychic Connections." So run the more atomistic of Wundt's chapter headings in his *Physiologische Psychologie* of 1902-1903. Yet the reflexologists were as elementistic as the introspectionists. John Dewey inveighed against this atomism of reflexes in his early pre-Gestalt paper of 1896, and the Gestalt psychologists rejected Watson's behaviorism, along with Wundt's and Titchener's introspectionism, as not recognizing that the whole is apt to be more than the sum of its parts; that is to say, to depend not only on the parts but also on the relations amongst them.

Analytical theories are still useful. The analysis of facial vision into component sensory clues most closely resembles old-fashioned mental chemistry,<sup>21</sup> but you have also in modern phrase the analysis of capacities and skills into primary mental abilities by factor analysis,<sup>22</sup> the analysis of discriminatory behavior into abstracted parts of the situation by the observation of transfers of training,<sup>23</sup> and the analysis of personality into traits, needs, abilities and factors in ways too numerous to detail. All that has happened to discountenance these analytical theories is the success of Gestalt psychology in impressing on psychologists the importance of field theory and the frequency with which simple disjunctive analysis fails adequately to explain the phenomenon under investigation.

(10) *Physiological theories.* Because psychophysical parallelism dominated nineteenth-century psychology, the more common kind of psychological theory has been the explanation of conscious events in terms of the nervous system. So Titchener in 1910 wrote: "The psychologist answers the question 'what' by analysing mental experience into its elements. He answers the question 'how' by formulating the laws of connection of these elements. And he answers the question 'why' by explaining mental processes in terms of their parallel processes in the nervous system."<sup>24</sup> That triad of activities was set up as the goal of the new scientific psychology, which had been on this account called *physiological psychology*: (1) analysis, (2) synthesis, and (3) explanation in terms of the nervous system. Actually Titchener's system would have collapsed if he had had only conscious events to analyze and synthesize

<sup>21</sup> Michael Supra, Milton Cotzin, and K. M. Dallenbach, "Facial vision:" The perception of obstacles by the blind, this JOURNAL, 57, 1944, 133-183; Philip Worchel and Dallenbach, "Facial vision:" Perception of obstacles by the deaf-blind, *ibid.*, 60, 1947, 502-553; Cotzin and Dallenbach, "Facial vision:" The rôle of pitch and loudness in the perception of obstacles by the blind, *ibid.*, 63, 1950, 485-515.

<sup>22</sup> R. B. Cattell, *Description and Measurement of Personality*, 1946; L. L. Thurstone, *Multiple-Factor Analysis*, 1947.

<sup>23</sup> R. S. Woodworth, Transfer of training, *Experimental Psychology*, 1938, 176-207, bears on this kind of analysis in the sense of Woodworth's statement that "the theory about which most of the experiments on transfer revolve . . . is called 'the theory of identical elements'" (p. 177).

<sup>24</sup> E. B. Titchener, *Text-Book of Psychology*, 1910, 41.



without the nervous system and the world of stimulus for underpinning. So we had in those days the Hering theory of vision and the Helmholtz theory of hearing to explain the observed sensory relationships of those two sense departments, and, if the physiology of these explanations seems now so speculative as to place the theories under our next classification, still we have today better physiology from Hecht and Békésy and others to fill this classical need. The establishment of sensory and motor centers in the brain was good psychophysiological theory, though it must be admitted that Lashley's theories of equipotentiality and mass action as cerebral theories for learning have stood up less precisely than he originally hoped. Nevertheless there exists today a great deal of good physiological fact whose correlation with psychological events is well established, and C. T. Morgan, later assisted by Stellar, has found enough of it to fill the greater part of a book.<sup>25</sup> So, in spite of unrealized expectations, retracted assertions and great gaps that never were filled, there is actually today a large body of reliable fact that constitutes the physiology of psychology, the body of the physiological explanations of behavioral and conscious events.

(11) *Conceptual theories.* Skinner has remarked, undoubtedly with a gleam in his eye, that perhaps the term CNS, when it is used by psychologists, means Conceptual Nervous System.<sup>26</sup> That is one of Skinner's contributions, his purging the organism of the pseudophysiology which psychophysical parallelism had forced upon psychology in the nineteenth century. Hering did not know that there were three color substances in the retinal cones, each capable of undergoing a reversible reaction to provide a photochemical basis for each of six primary colors. His was merely an *als-ob* theory: if the eye worked in this fashion, then these perceptual laws would hold.<sup>27</sup> The substances still needed to be validated by being discovered in some second way other than by observing the laws of color. G. E. Müller's cortical gray to explain the seventh primary color by molecular motion in the cells of the occipital cortex,<sup>28</sup> and McDougall's cortical energy to explain the phenomena of the limited range of attention<sup>29</sup> are similar physiological fictions. Perhaps Avenarius's System C is the best example of the hypothesization of physiological entities, for the System C was merely that part of the nervous system which is necessary for consciousness.<sup>30</sup> Avenarius did not know what part was necessary, yet he was sure that some part was necessary; so he named it, thus making a greater contribution to semantics than to neural anatomy. There have been many physiological theories of psychological events in which the physiological 'facts' turned into concepts as a theory lost acceptability and became a description instead of a correlation.

While Skinner was rejecting the Conceptual Nervous System and sticking to honest description, Tolman was rejecting the Nervous System and keeping the Concepts.<sup>31</sup> It was his idea that you can deal with psychology in terms of stimulus

<sup>25</sup> C. T. Morgan and E. Stellar, *Physiological Psychology*, 2 ed., 1950.

<sup>26</sup> Skinner, *The Behavior of Organisms*, 1938, 421 f.

<sup>27</sup> On Hering's color theory, see Boring, *Sensation and Perception* (op. cit., 1942), 206-209, 218.

<sup>28</sup> On Müller's cortical gray, see *ibid.*, 213.

<sup>29</sup> McDougall, The state of the brain during hypnosis, *Brain*, 31, 1908, 242-258.

<sup>30</sup> On Avenarius's System C, see Boring, *History of Experimental Psychology*, 2 ed., 1950, 396, 433.

<sup>31</sup> Tolman, Operational behaviorism and current trends in psychology, *Proc. 25th Anniv. Celebr. Inaug. Grad. Stud.* (Univ. So. Calif.), 1936, 89-103; reprinted in M. H. Marx, *Psychological Theory*, 1951, 87-102; also in Tolman, *Collected Papers in Psychology*, 1951, 115-129.



and response but that you need to simplify the description of relationships by the use of hypothetical constructs, conceptual entities which are conceived as variables intervening between stimulus and response. Tolman's idea was that between stimulus and response there intervene the effects of physiological drive (P), heredity (H), previous training (T) and age or maturity (A), and that these effects, these intervening variables, I, are functions of P, H, T, and A. Behavior is thus a function of the stimulating conditions and all these variables, and the intervening variables, when they first come into existence, are just as redundant as Avenarius's System C. Gravitation was at first a similar redundant hypothetical construct. The justification for such concepts in theory building will lie in their fertility. If by simplifying thinking, they suggest hypotheses that are at first unexpected and then confirmed by experimental test, they will have been fertile. If the same concepts begin to enter into more and more hypotheses about different empirical relationships and the hypotheses stand up, then the concepts will begin to acquire thinghood or reality, as gravitation has.

We can not here go further with Tolman nor enter into the complicated system of behavior that Hull has offered psychology, a system that makes wide use of intervening variables and hypothetical concepts.<sup>32</sup> It is enough to say that we are now considering the most active field of psychological theorizing. Although it may seem at times as if Tolman and Hull got but little distance beyond the tautologies of description, as if they had not yet fully escaped from the meshes of philosophy which hindered psychology at the first, this matter is something that only posterity can judge. The systems of both these men have been useful in creating bands of loyal disciples, hard-working scientific in-groups who may, indeed, ultimately contribute more to psychology than do the systems which first captivated their imaginations.

(12) *Physical models.* After the physiological and conceptual explanatory systems come the physical and mathematical models. It is hard to keep the models distinct from the theories, and London believes that any theory which is explanatory and not merely descriptive is a model.<sup>33</sup> You see, if you set up a physical analogy for some set of psychological events, a model whose physical interrelations appear to constitute an explanation of the events, then there are three positive things that may happen, independently or in combination. You can get interested in the mathematical expression of the physical relationships that are the essence of the model, and then you have either a physico-mathematical model, or, if your interest shifts to the formulae, a wholly mathematical model. Or you can wonder whether your physical model may not, after all, really exist in the nervous system, and presently you find yourself with a physiological theory on your hands, one which could become real if its terms were later to be validated. Or you may change your model over into a conceptual system and hope eventually that its terms will get validated, will acquire thinghood and be counted as real. The main difference between a model and a theory is, in my opinion, that you do not think of a model as actually existing nor do you usually hope that it may eventually acquire thinghood. Thus the model is recognized as an analogy as the theory is not, and you accept small deviations from the model as reasonable, whereas small deviations from a theory should be

<sup>32</sup> C. L. Hull, *A Behavior System*, 1952.

<sup>33</sup> I. D. London, The rôle of the model in explanation, *J. Genet. Psychol.*, 74, 1949, 165-176.



fatal to it unless they may be counted as errors of observation or sampling. Hering's theory of vision would have fared better had it been thought of as a model rather than as physiological fact. A model tolerates exceptions.

We may consider the physical models first. There was, for instance, McDougall's hydrostatic analogy for learning and inhibition, a model which tended to move over toward physiological theory as a system of cortical neural plumbing.<sup>34</sup> Before the modern electronic robot came on the scene, there were various simple little mechanical analogies constructed, like Hull's mechanical model of the conditioned reflex.<sup>35</sup> Perhaps the most influential physical model in the history of experimental psychology was Köhler's model of the distribution of electrostatic charges, which he used to explain the organization of perception in his *Physische Gestalten* of 1920, the book addressed primarily to physicists and biologists, the one that is said to have got him his chair at Berlin.<sup>36</sup> This model, however, came to move over toward physiology under pressure from Gestalt theory's doctrine of psychocerebral isomorphism, and now the model is actually acquiring thinghood as Köhler measures the movement of electrical potentials in the occipital cortex when the stimulus-object moves in the visual field.<sup>37</sup> This is the kind of future that every modelist would like to see his model eventually achieve.

Of course, the physical models that everyone is discussing nowadays are the calculating machines, the servo-mechanisms, and the other robots of cybernetics.<sup>38</sup> Only seventy-five years ago psychologists were rejecting Helmholtz's notion of unconscious inference as a paradox, as a contradiction of terms. Nowadays, if you want quick reliable complicated inference, you get a machine and not a man. Whether the machine is, therefore, conscious does not bother the twentieth century positivist as it did the nineteenth century idealist.

Probability theory and statistics still sometimes appeal to urns that contain black and white balls to be drawn out and their frequencies noted, but these urns are conceptual nowadays. No modern probability theorist would dare use real urns with less than an infinity of balls, and infinity simply is not a physical dimension.

(13) *Mathematical models.* As mathematics is the powerful chief tool of physics, so the mathematical models are more suitable for adaptation and inference than the concrete physical models. They are not so dramatic, nor so clear to those who do not think readily in mathematical terms, nor is the promise of attaining status as reality so plainly written on them. On the other hand, they are easily manipulated by those trained to think mathematically, and the assertions derived from the initial assumptions can often be tested by comparison with known fact.

Some time ago Rashevsky announced that he thought that the time had come for "a purely theoretical biophysical psychology," and he proceeded to show what should be done. He assumed nervous conduction, connections between neurones, the con-

<sup>34</sup> McDougall, The nature of inhibitory processes within the nervous system, *Brain*, 26, 1903, 153-191, esp. 167.

<sup>35</sup> H. D. Baernstein and C. L. Hull, A mechanical model of the conditioned reflex, *J. Gen. Psychol.*, 5, 1931, 99-106.

<sup>36</sup> Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, 1920.

<sup>37</sup> Köhler, Held, and O'Connell, *op. cit.*, *supra*, note 11.

<sup>38</sup> N. Wiener, *Cybernetics or Control and Communication in the Animal and the Machine*, 1948, esp. 137-155.



vergence and divergence of connections, the principles of excitation and inhibition, the all-or-none principle of conduction, and two kinds of transmission between neurones. All these principles he put over into mathematical terms and proceeded to work out relationships that would constitute the lawful principles to be found in the nervous system by virtue of these first assumptions.<sup>39</sup> Earlier he had done the same sort of thing for the brain.<sup>40</sup> There are also some mathematically minded psychologists who have worked on mathematical models of learning, like Pitts<sup>41</sup> and Bush and Mosteller.<sup>42</sup> Nevertheless, for all the work that has been done in this field, there does not seem as yet to be much new fact in the form of confirmed deductions. Examine the *Handbook of Experimental Psychology* or the volumes of the *Annual Reviews of Psychology* for new facts derived from the use of mathematical modelling to see how little there is.

Perhaps a comment is in order here about mathematiphilia and mathematiphobia. There has always been a great deal of research in experimental psychology that could be successfully attacked without the use of much mathematics. Psychologists were not, therefore, forced to use mathematics in order to succeed professionally. Thus there have been some who eschewed mathematics and some who delighted in its use. The 'phobes have believed that the 'philes are trapped by the sheer pleasure of mathematical manipulations, and certainly there have been instances where precise and elaborate deductions have been drawn from unreliable and scanty data. On the other hand, there can be little doubt that the 'phobes include many persons who suffer, when they face mathematical argument, from mental dazzle, as David Katz has named the way in which efficiency is diminished by the irrelevant appearance of difficulty or complications.<sup>43</sup> If you find it easier to add 22 and 43 cents than 22 and 43 *Clostridii pastoriani*, the difference would be due to the dazzle that the formidable name of these bacteria induces.

(14) *Reification of concepts.* We come finally to the question of how concepts become real, how they gain thinghood, as Bergmann puts it. In the first place we must note that none of the material that enters into science and stays there is a phenomenal particular. Science considers only generalities or constructs. Many of these constructs have attained the status of real relations or objects by the versatility and consistency with which they enter into scientific theories and facts. In the equation for the falling body,  $g$ , the acceleration due to gravity, is a construct and quite real. So is gravitation real, and an atom, and a star, and the red color of that book. They are all constructs which fit into so many factual formulae that they have lost their dubiety. It is Bergmann who reminds us that a star and a microscopic object are just as much constructs as an atom.<sup>44</sup> So is the atom just as much an object as the

<sup>39</sup> N. Rashevsky, Mathematical biophysics and psychology, *Psychometrika*, 1, 1936, 1-26.

<sup>40</sup> Rashevsky, Outline of a physico-mathematical theory of the brain, *J. Gen. Psychol.*, 13, 1935, 82-112.

<sup>41</sup> W. Pitts, A general theory of learning and conditioning, *Psychometrika*, 8, 1943, 1-18, 131-140.

<sup>42</sup> R. R. Bush and F. Mosteller, A mathematical model for simple learning, *Psychol. Rev.*, 58, 1951, 313-323.

<sup>43</sup> David Katz, *Gestalt Psychology*, 2 ed., trans. 1950, 115 f., 125-127.

<sup>44</sup> Bergmann, Outline of an empiricist philosophy of physics, *Amer. J. Physics*, 11, 1943, 258 and esp. 335-342.



star. In this way we discover how to measure the success of the theorizing that is going on in psychology. Do the intervening variables and the other hypothetical constructs stand up, get involved in more and more relations, find themselves used by many persons, appear to be gaining the status of something real? They will not have succeeded until they get in some degree that sort of thinghood.

Another way of saying this same thing is to note that, as long as a new construct has only the single operational definition that it received at birth, it is just a construct. When it gets two alternative operational definitions, it is beginning to be validated. When the defining operations, because of proven correlations, are many, then it has become reified. For the most part the factors that factor analysis yields stand tautologically at the first level. They merely describe the data that are included in a correlational matrix. Naming them, by examining their relations to the tests that generated them, adds no whit of reality to their existence. On the other hand, an empirical correlation does. When Halstead sought to relate the factors that came out of the analysis of a test battery to his observation of brain lesions, he was actually making one of the rare attempts at this kind of validation. Unfortunately his correlations are not sharp and the best he could do was to relate to gross lesions in the brain an impairment index, based on all four of the factors of biological intelligence.<sup>45</sup> I once hoped that Spearman's general factor would turn out to be speed of reaction; that would have been a validation, but of course my hypothesis was wrong.<sup>46</sup> Still the basic fact remains. A thing or a relation is real in as far as it has many alternative defining operations.<sup>47</sup> That is why an object is really a theory about experience.

There lies psychology's array of theories, and the question of the rôle of theory in experimental psychology is answered. Psychology *is* theory, just as science is theory—descriptive theory sometimes, and explanatory theory at other times, yet theory, because it is concerned with constructs that are things and their relations. Psychology and science are theory empirically based. You check constantly against phenomenal particulars but you are after the generalities that the particulars yield. So I might stop here, but there is still the question of the relation of psychology to philosophy. Did experimental psychology once break away from philosophy and is the modern interest in theory and system bringing it back? And is this trend, if it is a trend, progressive or regressive? Do we encourage it or discourage it?

In general, science has always been philosophy—natural philosophy,

<sup>45</sup> W. C. Halstead, *Brain and Intelligence*, 1947, esp. 147-149.

<sup>46</sup> Helen Peak and Boring, The factor of speed in intelligence, *J. Exper. Psychol.*, 9, 1926, 71-94.

<sup>47</sup> P. W. Bridgman, *Logic of Modern Physics*, 1927, 3-25, opposes this view, holding to the pluralism that different defining operations always yield different constructs. I think Bridgman is wrong. See both Bridgman and me in Symposium on operationism, *Psychol. Rev.*, 52, 1945, 241-294, but especially the references to Question 2 on pp. 241, 243 (Boring), 247 (Bridgman), 279 (Boring). Did Bridgman partially yield the point?



mental philosophy; but historically there have been two schisms, one between the rational and the empirical, and the other between idealism and freedom, on the one hand, and materialism and determinism, on the other. The Greeks and the Scholastics, from Plato to St. Thomas, felt a certain scorn for the empirical as work for artisans, and deep respect for rationally given ideas. It was in these days that the circle, a perfect form, was accepted a priori for the orbits of the planets which, because of their divine origin, could not conceivably have been provided with inferior orbits. When the observed data did not agree with this superior knowledge, the theory of epicycles (a circular motion of a circular motion) was found to lessen the discrepancy. In the same manner the modern fanatic holds to his theory while exerting his ingenuity to find rationalization to support it.

The Middle Ages gave place to the new learning with the weakening of aristocracy and the beginnings of democracy. The acceptance of the Copernican theory was, in a sense, a sign that scientific empiricism comes in as the divine right of kings goes out. By the time you get to Descartes, Newton, and Leibnitz, there is no discernible line between natural philosophy and the other kinds. It was then that empiricism, with Locke, Berkeley, and Hume, took over philosophy, establishing the ground for what later became empirical or scientific psychology. Empiricism runs to positivism, and it was Hume's positivistic skepticism that created a crisis for the empiricistic trend. First the Scottish philosophers rose against Hume, and then Kant, being wakened from his dogmatic slumbers, accomplished the restoration of the a priori ideas which Hume had condemned. Kant did not, however, destroy empiricism; he simply led the German idealists away from determinism to freedom and moral responsibility, while the scientists, the physicists and physiologists, stayed behind. Thus the nineteenth century became the period of the greatest schism between philosophy and psychology, and the reaction of many of the new scientific psychologists—not all, not Wundt or Stumpf or Münsterberg—against philosophy was but part of this general movement.<sup>48</sup>

Now in the twentieth century relativity theory has again obliterated the line between theoretical physics and philosophy, and it may be that psychology should profit by physics' example. Certainly, if the line between philosophy and science was never more than a Kantian artifact, we need not worry when it wavers and disappears. The main matter for the theoretical psychologists to keep in mind is, as I said before: Are the new concepts, the

<sup>48</sup> A not inconsiderable portion of the thought of the last two paragraphs I owe to the enlightening article by Philipp Frank, *The origin of the separation between science and philosophy*, *Proc. Amer. Acad. Arts. and Sci.*, 80, 1952, 115-139.



new hypothetical constructs, the new intervening variables, getting thinghood fast enough? A healthy concept grows up, is weaned, and eventually goes out from the home where it was conceived to make new friends and to play a lasting rôle in the real world, that is to say, the world of constructs. Experimental psychology in the United States is healthy and growing rapidly. Mezes Hall with its splendid facilities is a symptom of national vigor in psychological research. All this great and expanding body of investigation ends, moreover, in theory, because theory is whither it is directed and the only goal in which it can end. The difference about which we are inquiring is actually not qualitative but quantitative. Should theory be kept simple and near description, we are asking, or should we generate hypothetical constructs and intervening variables freely for use in conceptual structures, in the hope that they may in due course harden into the stable realities of science and thus promote more progress than timorous description alone would yield? It would not be enough to examine the handbooks to see what the new conceptualism has contributed to firm fact, for that would be like attempting to divide college Freshmen into able and dull by looking them up in *Who's Who*. The new concepts are not yet old enough to have produced or to have become reified. We must perforce wait and posterity will make the judgment.

Meanwhile it is proper to advocate a modified tolerance. Let us try to get rid of lazy persons and of those who use rationalization to support pet theories that are contradicted by many facts, but among the tough-minded hard-workers let us bless both the factualists, congratulating them on their sanity, and the conceptualists, saluting them for their courage. It will take many different motives and personalities working synchronously, though perhaps not together, to manoeuvre psychology ahead to that understanding of human nature of which the world now stands in such great need.<sup>49</sup>

<sup>49</sup> Since this paper was written, K. M. Dallenbach has published his address of 1951 on the same topic, The place of theory in science, *Psychol. Rev.*, 60, 1953, 33-39. Here Dallenbach writes in favor of accepting Titchener's advice to "carry your theories lightly," although he does not underestimate the importance of theory in the choice and formulation of experimental problems. The chief difference between us—and it is a big difference—is that he distinguishes fact from theory, whereas I, appealing to continuity, do not; and I distinguish fact from the phenomenal particulars of data, whereas I suspect from reading this paper that Dallenbach does not.

The current activity in making and criticizing theories is illustrated by the fact that this same number of the *Psychological Review* (January, 1953) contains five other important articles on theory: F. H. George, Logical constructs and psychological theory, pp. 1-6; R. C. Davis, Physical psychology, pp. 7-14; L. O. Kattsoff, Facts, phenomena, and frames of reference in psychology, pp. 40-44; D. Bakan, Learning and scientific enterprise, pp. 45-49; and K. MacCorquodale and P. E. Meehl, Preliminary suggestions as to a formalization of expectancy theory, pp. 55-63.



# TEMPORAL DISCRIMINATION BY THE HUMAN EYE

By ALEX L. SWEET, University of Kansas

The purpose of this investigation was to measure the ability of the human eye to discriminate between two lights which flash at different times. This is a measure of the temporal resolving power of the eye and, as such, is related to the critical flicker frequency. The novelty of the method used in this experiment is that the discrimination occurs between two spatially discrete stimuli and not between successive stimuli exciting the same retinal position. Though the kind of stimulus-situation used in this investigation occurs also in studies of apparent movement, the latter have been concerned primarily with the optimal conditions for the perception of movement and not with temporal resolution as such.

In this experiment, the discriminations were made between adjacent lights and between widely separated lights, under conditions of dark and light adaptation.<sup>1</sup>

## APPARATUS AND PROCEDURE

*Apparatus.* The stimuli were produced by two 'glow-modulator' flash tubes of the kind used in television. They are especially suited to this type of work because of the extremely rapid rise and fall of their light output, being about 20  $\mu$  sec. in each case.

In this experiment, the pair of lights flashed once every 2 sec. and the duration of each flash was 15 m.sec. Each light was white in color and had a luminance of 10 millilamberts. The shape of each stimulus was circular and the diameter, at the test-distance, subtended a visual angle of 14'.

The electronic equipment which activated the lights was specially designed. Among the controls was one for varying the time-interval by means of which the time of onset of either light was delayed with reference to that of the other. A description of this equipment has been published and the reader is referred to it for further details.<sup>2</sup> Frequent calibrations showed the apparatus to be stable and precise. Time-measurements are accurate to within 1 m.sec.

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<sup>1</sup> Information about the temporal sensitivity of the visual system can be obtained from several experimental fields. Among these are studies on (1) reaction-time, including those on *Empfindungszeit*; (2) critical flicker frequency; (3) discrimination of two successive stimuli from one light source; (4) electrical potentials of the retina and the optic nerve; and (5) perception of movement.

<sup>2</sup> R. G. Roush and F. Hamburger, Jr., Light-flash generator, *Electronics*, 21, 1948, 100-102.



A wooden perimeter (cf. Fig. 1), whose radius was 4.2 ft. and at whose center *O* sat with his head on a chin-rest, was used to orient the flash tubes at different positions. Each flash tube was mounted on a small wooden platform which could be placed anywhere around the perimeter. Each tube, moreover, was so mounted that when the two platforms were placed side-by-side the two lights appeared tangentially adjacent.

In front of each flash tube was a sheet of flashed opal glass. The front surface of the glass was covered with an opaque material except for a circular area, 0.2 in. in diameter, through which the light was transmitted and diffused. In this way, the light appeared to originate from the glass surface as a small circle.

*Scotopic conditions.* To obtain data on the dark-adapted eye, readings were taken in a completely dark room. *E* was located at a distance from *O* and manipulated the

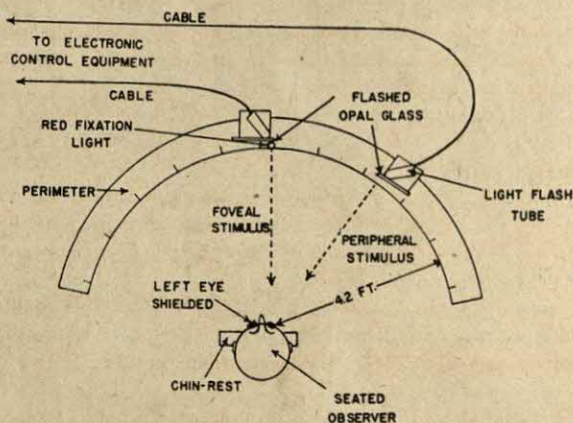


FIG. 1. DIAGRAM OF THE APPARATUS  
(View from above)

controls behind a black curtain. For foveal fixation in the dark, a small red light was used.

*Photopic conditions.* The conditions for discrimination with the light-adapted eye were identical with those for the dark-adapted eye except that the two lights appeared against a homogeneous gray background of constant brightness. The background consisted of neutral gray cloth, 6 ft. high and 13 ft. in arc length, draped in a vertical plane along the front edge of the perimeter. Five foot-candles of diffuse illumination were cast upon this field by two fluorescent 'day-light' lamps. The luminance was 2.5 millilamberts.

The lights were exposed by cutting out windows, 2 in. square, from the cloth at the desired positions along the horizontal meridian. The opal glass, covered with the same cloth as that of the background except for the circular aperture, was placed against the back surface of the window. *O* thus saw two lights flashing from a totally gray field.

*Procedure.* In general, two lights flashed at different times of onset and *O* was required to discriminate between them with respect to any difference in time. The



discriminations were measured as a function of the following variables.

(1) *Retinal position.* All observations were monocular with the right eye. The left eye was shielded by an eye-patch. The nasal retina of the right eye was stimulated (the lights coming from the temporal field of view) at each of the following positions: fovea,  $5^\circ$ ,  $10^\circ$ ,  $20^\circ$ , and  $40^\circ$ . The order in which these retinal positions were measured was randomized.

(2) *Spatial separation.* Two different series were run. One series consisted of one light stimulating only the fovea, with the second light located at either  $5^\circ$ ,  $10^\circ$ ,  $20^\circ$ , or  $40^\circ$  in the temporal field. The second series of discriminations was conducted with both lights tangentially adjacent to each other and together located at either the fovea,  $5^\circ$ ,  $10^\circ$ ,  $20^\circ$ , or  $40^\circ$ .

(3) *Adaptation of the eye.* Data were obtained on both the dark-adapted and the light-adapted eye. All data on the dark adapted eye were collected first. *O*, before discriminating in the dark, was dark adapted for 30 min. by wearing special polaroid glasses which kept all light from the right eye. In the photopic condition, *O* looked for 5 min. at the illuminated field before making judgments.

*Observers.* Six young men, undergraduates, whose vision was free of any manifest defect, served as *O*s for the scotopic discriminations. Of these, two were used in the subsequent photopic series.

*Criteria for judgment.* For all conditions but one, the judgment was with reference to the relative times of onset of the two lights. As each pair of stimuli flashed, *O* reported 'same' or 'different' to indicate whether the two lights appeared to flash at the same time or at different times.

During the *photopic* series, it was discovered that this criterion could not be used by *O* when the two adjacent lights stimulated the periphery because he could not clearly see two separate lights under these conditions. Instead, the two flashes fused into one and appeared as a streak of light.

Despite this spatial fusion, there were two criteria by which a temporal discrimination could still be made. (a) The light appeared either as moving or stationary. The fused light could be seen to move, as a whole, with delays of about 5 m.sec. When the temporal difference approached 0 m.sec., however, the streak appeared stationary. (b) Seeing either one or two flashes. The two flashes seen as a single fused light at low delay values could be seen to flicker when delays of the order of 35 m.sec. or more were used. Two successive flashes were seen, with both flashes appearing to come from the same location. This criterion was applicable only to the  $10^\circ$ ,  $20^\circ$ , and  $40^\circ$  positions where the visual acuity was definitely inadequate.

*Psychophysical method.* The psychophysical method of limits was used in presenting the stimuli. The difference between the times of onset of the two flashes was varied in steps and in orderly sequence by *E*. *O* reported at each setting. Time differences were termed positive when the foveal light lagged behind the peripheral light; negative when the foveal light led. A series of judgments ranged from a positive delay, where a difference was clearly perceived, to a negative delay where a difference was again perceptible. Or it could proceed from negative to positive.

For each *O* under each condition, 10 such series were run, 5 in ascending order and 5 in descending order. The ascending and descending series were randomly distributed in the order of the 10 series. Two practice series were given before data were recorded.



For most conditions, a step interval of 5 m.sec. was used in going from one delay setting to another. For those conditions which yielded poor discrimination, resulting in relatively high thresholds, step intervals of 10 or 20 m.sec. were used to shorten the length of the series. The number of steps, however, in any one series was at least 10. Thus, a statistically adequate total number of judgments was obtained from which measures of central tendency and variability could be computed.

*Treatment of data.* A frequency distribution of the total number of equality (or non-discrimination) judgments was prepared for each *O* at each condition. Thus, if no discrimination occurred at a given delay, the number of equality judgments would be 10 since there was a total of 10 series presented. On the other hand, if another delay permitted discrimination all the time, the number of equality judgments for that delay would be zero. The means and probable errors of these distributions are the basic measures in the analysis of the data.

*Interpretation of the mean.* The mean of a distribution of equality judgments is a constant error: it shows the difference in latency between the two retinal positions being stimulated. For example, if the mean of the equality judgments between one light located at the fovea and another light located at 40° was + 15 m.sec., it would

TABLE I  
MEANS AND PROBABLE ERRORS (IN M. SEC.) OF THE JUDGMENTS OF EQUALITY  
Discriminations between two separated lights; one at the fovea,  
the other at the retinal position indicated.

Adaptation	Os	0°		5°		10°		20°		40°	
		M	PE	M	PE	M	PE	M	PE	M	PE
dark	J.R.	-2.1	5.42	4.9	9.53	6.8	12.51	18.2	15.02	15.0	17.06
	K.W.	0.1	4.84	28.6	11.41	28.3	9.98	21.2	14.94	8.4	9.00
	J.A.	1.9	6.52	9.8	11.51	7.3	12.44	2.1	7.57	7.6	12.32
	S.D.	-0.9	5.11	-8.2	12.41	3.8	18.06	-14.4	10.57	-10.1	11.43
	J.S.	-1.0	6.25	6.8	7.33	7.8	9.13	5.6	5.71	11.6	8.30
	R.S.	0.1	4.52	22.6	7.75	27.6	8.08	36.9	8.96	30.2	7.45
	Means	-0.3	5.44	10.8	9.99	13.6	11.70	11.6	10.46	10.5	10.93
light	J.R.	0.5	5.49	13.3	8.34	17.0	13.89	45.3	16.14	23.5	14.03
	K.W.	1.8	5.61	-0.5	3.97	3.9	10.55	9.5	10.10	16.7	9.85
	Means	1.2	5.55	6.4	6.16	10.5	12.22	27.4	13.12	20.1	11.94

indicate that the peripheral position had a 15 m.sec. greater latency than the fovea.

*Interpretation of the probable error.* The probable error is a measure of the variability of the distribution and indicates the sensitivity or precision of the discrimination. It also represents the difference limen: stimulus-values equal to the mean plus or minus one PE will be perceived as different half the time.

## RESULTS

*Lights spatially separated.* The means and probable errors of the equality judgments for each *O* when the two lights were spatially separated are presented in Table I for both the dark- and light-adapted eye. The averages of the individual means and the averages of the individual probable errors are graphically shown in Figs. 2 and 3.

For the dark-adapted eye, as can be seen in Fig. 2, a similar pattern is shown by both the mean and probable error with changes in retinal



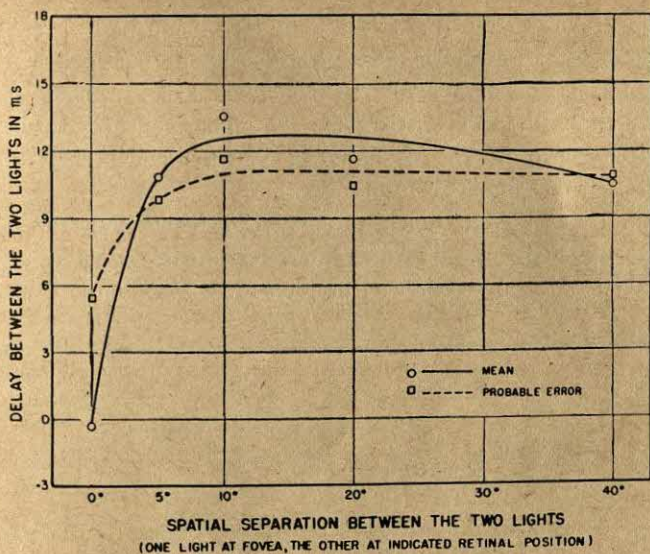


FIG. 2. MEANS AND PROBABLE ERRORS OF THE JUDGMENTS OF EQUALITY  
Averages for 6 Os; lights separated; scotopic conditions.

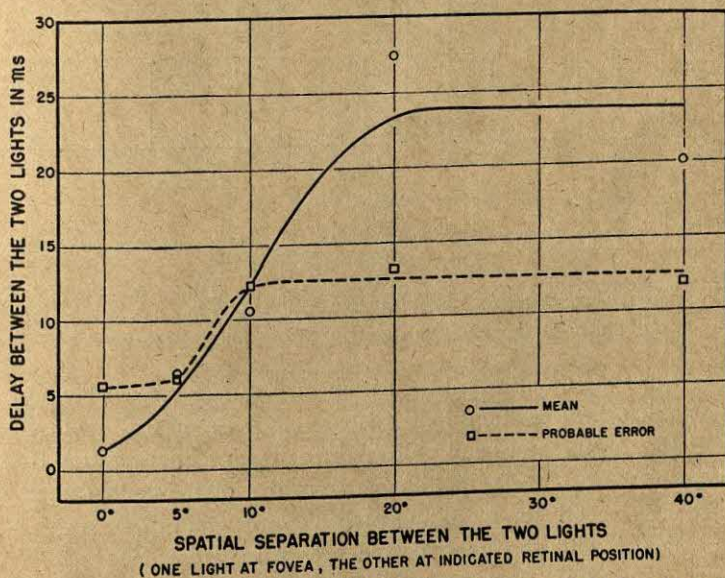


FIG. 3. MEANS AND PROBABLE ERRORS OF THE JUDGMENTS OF EQUALITY  
Averages for 2 Os; lights separated; photopic conditions.



position. Minimal values for each measure are obtained when both lights are adjacent at the fovea. The mean at this condition is approximately zero. For varying amounts of separation with one light always at the fovea, there is a rapid rise in the value of the mean and probable error from the fovea to  $5^\circ$ ; from  $5^\circ$  to  $40^\circ$  the magnitude of the mean and probable error is approximately the same at about 11 m.sec.

Statistical tests indicate that the means and probable errors at the various peripheral positions are all reliably greater than the foveal values. The differences between the peripheral positions, however, are not significant.

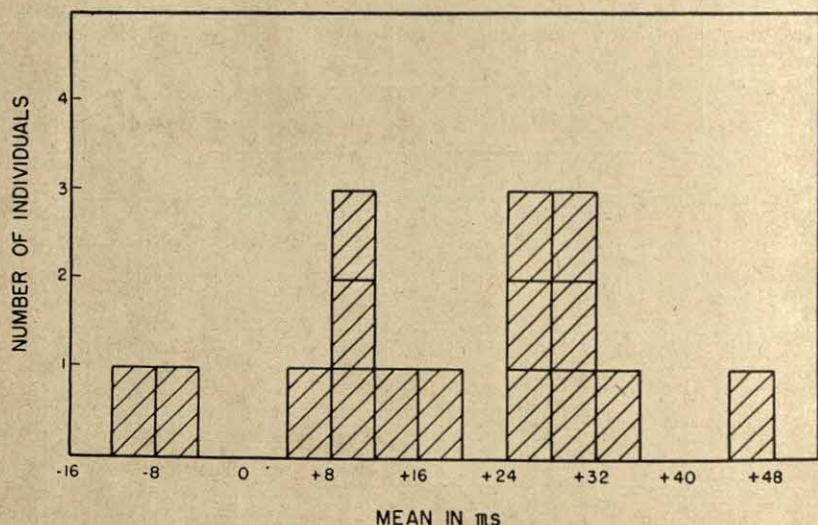


FIG. 4. FREQUENCY DISTRIBUTION OF THE MEANS OF JUDGMENTS OF EQUALITY OF 16 INDIVIDUALS

Each rectangle represents one individual. Conditions: one light at fovea, second light at  $40^\circ$ , eye dark-adapted. A positive mean indicates that the latency at  $40^\circ$  was greater than at the fovea; a negative mean indicates the contrary.

The fact that the means of all the peripheral locations are reliably positive indicates that these positions have a latency greater than that of the fovea.

Variation of the means and probable errors with retinal position for the light-adapted eye (Fig. 3) is different from that of the dark-adapted eye. The change in both the mean and probable error from the fovea to  $5^\circ$  is more gradual with the light-adapted eye. Also, from  $20^\circ$  to  $40^\circ$ , the means and probable errors are of different magnitudes, the means being about 24 m.sec., the probable errors about 13 m.sec. Statistical tests show that neither the means nor the probable errors at each of the peripheral



positions differ reliably from the foveal values. Nor do the means and probable errors differ reliably among the various peripheral positions, with the exception of those at  $5^\circ$  which are significantly lower than some of the other peripheral values.

No statistically reliable differences were obtained between the values for dark- and light-adaptation at any retinal position with respect either to the mean or to the probable error.

An examination of Table I shows widespread individual differences. This point is illustrated by the results of S.D. who has a *negative* constant error at several peripheral positions indicating faster reaction in the periphery than at the fovea. Since he was the only O to show negative values, additional tests were made under dark adaptation upon 16 individuals with one light at the fovea and the second at

TABLE II  
MEANS AND PROBABLE ERRORS (IN M. SEC.) OF THE JUDGMENTS OF EQUALITY  
Two adjacent lights at various retinal positions.

Adaptation	O	$0^\circ$		$5^\circ$		$10^\circ$		$20^\circ$		$40^\circ$	
		M	PE	M	PE	M	PE	M	PE	M	PE
dark (criterion: same or different)	J.R.	-2.1	5.42	-5.9	7.70	3.5	17.71	3.1	20.55	13.1	26.16
	K.W.	0.1	4.84	2.1	5.53	4.3	18.30	-2.0	25.93	-1.8	31.50
	J.A.	1.9	6.52	5.5	8.11	1.4	16.42	-0.1	22.80	2.9	18.18
	S.D.	-0.9	5.11	2.3	6.00	-2.2	8.59	10.3	29.58	-12.3	25.74
	J.S.	-1.0	6.25	-1.9	4.32	3.1	11.52	0.1	26.08	2.8	28.50
	R.S.	0.1	4.52	-0.4	4.98	-1.6	18.31	-3.4	22.93	-5.2	20.15
	Means	-0.3	5.44	0.3	6.11	1.4	15.16	1.3	24.66	-0.1	25.07
light (criterion: movement or simultaneity)	J.R.	0.5	5.49	-1.0	3.56	-0.8	2.78	0.9	4.34	0.4	3.79
	K.W.	1.8	5.61	-0.4	4.14	0.9	3.14	0.7	4.40	-0.3	4.21
	Means	1.2	5.55	-0.7	3.85	0.1	2.96	0.8	4.37	0.1	4.00
light (criterion: one or two flashes)	J.R.	—	—	—	—	-5.0	21.00	3.8	26.14	-8.3	25.15
	K.W.	—	—	—	—	-0.8	18.88	3.0	51.26	-0.8	43.07
	Means	—	—	—	—	-2.9	20.44	3.4	38.70	-4.6	34.11

$40^\circ$ . The distribution of their constant errors is shown in Fig. 4. The mean of this distribution is +19.0 m.sec. (reliably greater than zero) and the standard deviation is 14.29 m.sec. Two of these Os, however, yielded negative values.

*Lights adjacent.* The means and probable errors of the equality judgments for every O when the two lights were adjacent are shown in Table II. The values for the dark-adapted eye were obtained according to O's report of 'same' or 'different' times of onset of the two lights. There are two sets of values for the light-adapted eye: one according to the criterion of 'movement' or 'simultaneity,' and the other according to the criterion of seeing 'one' or 'two' discrete flashes. No figures for this latter criterion are given for the fovea and  $5^\circ$  since the visual acuity at these positions was adequate for O to see two spatially separated lights.

An inspection of Table II shows that the means are small in magnitude and that they are distributed essentially in a random fashion about zero.



This would be expected. Since the retinal positions stimulated were adjacent and since the lights were physically equal, no one position should have, in general, any greater latency than its neighbor.

Fig. 5 shows for the three conditions of discrimination the means of the individual probable errors at different retinal positions listed in Table II. Though the three curves in Fig. 5 were obtained according to different criteria, they are plotted on the same graph to indicate the different degrees of temporal sensitivity. This figure shows that the greatest sensitivity is

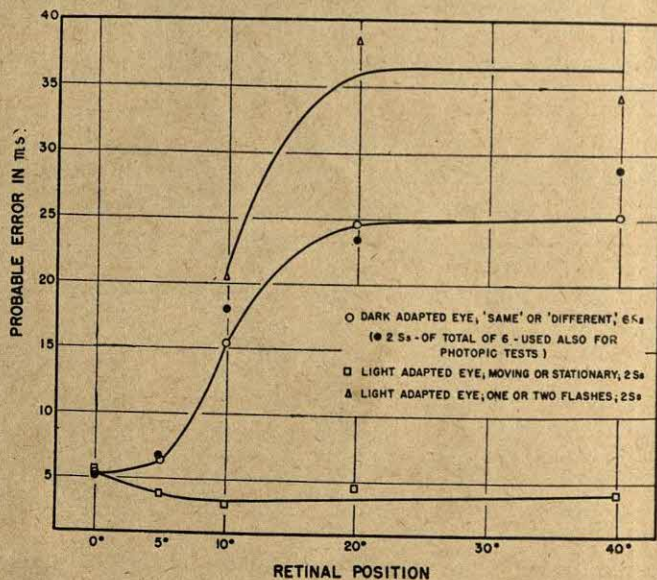


FIG. 5. MEANS OF THE PROBABLE ERRORS OF JUDGMENTS OF EQUALITY BETWEEN TWO ADJACENT FLASHES AT DIFFERENT RETINAL POSITIONS

The scotopic values of the two Os who were also used for the photopic tests are shown separately for comparison between conditions with the same Os.

obtained with the light-adapted eye and with movement as the criterion for discrimination. This particular discrimination is more precise in the periphery than at the fovea. The probabilities of the differences between the fovea and any of the four peripheral positions being due to chance are less than 5%. When the individual probabilities are combined, the periphery, as a whole, is reliably more precise than the fovea at less than a 1-% level of confidence.

The discriminations by the dark-adapted eye are less precise than the discrimination of movement with the light-adapted eye. Though the mean



probable error for the dark-adapted eye is almost identical with that of the light-adapted eye at the fovea, for all other positions the thresholds for the dark-adapted eye are reliably higher. The scotopic curve also shows a rapid rise in threshold from  $5^\circ$  to  $20^\circ$  which is statistically reliable. The differences between the fovea and  $5^\circ$ , and between  $20^\circ$  and  $40^\circ$  are not statistically significant.

The thresholds for perceiving a two-flash flicker with the light-adapted eye are reliably higher than those of the scotopic discriminations and those of the photopic discriminations of movement. The difference is statistically reliable when the differences at the three retinal positions ( $10^\circ$ ,  $20^\circ$ , and  $40^\circ$ ) are combined, though the differences at single positions are not significant. Within the flicker curve itself, there are no significant differences in threshold value between the retinal locations.

*Separated vs. adjacent lights.* For comparing the judgments of the

TABLE III

STANDARD DEVIATIONS (IN M.SEC.) OF THE MEANS OF THE TEMPORAL JUDGMENTS OF EQUALITY WHEN THE LIGHTS ARE SEPARATED AND WHEN THEY ARE ADJACENT  
(Dark-adapted eye; 6 Os.)

Condition	$5^\circ$	$10^\circ$	$20^\circ$	$40^\circ$
Lights separated*	12.03	10.23	16.22	11.88
Lights adjacent	3.61	2.51	4.48	7.85

\* One light at fovea, the other at retinal position indicated.

separated lights with those of the adjacent lights, the scotopic data are more valid than the photopic results. With the dark-adapted eye, the criterion was the same for both conditions, and a greater number of Os was tested.

In this comparison for the dark-adapted eye, two differences are clear. The first is that, when the lights are adjacent, the mean or constant error of the judgments does not differ reliably from zero, whereas with one light at the fovea and the second at various peripheral positions, the mean is significantly greater than zero.

The second is that the individual differences in the values of the mean are greater when the lights are separated than when they are adjacent. This is shown by Table III listing the standard deviations of the six means (one from each of six Os) when the lights were separated and when they were adjacent. The array of standard deviations for the separated lights is of the order of 12 m.sec., that for the adjacent lights is of the order of 5 m.sec. The difference is reliable at less than a 1-% level of confidence.



## DISCUSSION

*Discrimination of spatially discrete flashes.* When the fovea was paired with different peripheral positions, two variables were changed at the same time: (1) the retinal positions comprising the pair, and (2) the amount of intervening space. It is not possible, therefore, to isolate the effects of each from the data. For example, the different results between the fovea-5° judgments and the fovea-10° judgments could be due to the fact that the 10° position was twice as far from the fovea as 5° and not due to the local characteristics of the 5° and 10° positions.

Varying retinal space could affect the judgments by summation. Whether space could be operative through other mechanisms is more obscure. It is known that when two or more light stimuli are brought close together the total visual effect is greater than with only one of the stimuli.<sup>3</sup> With increasing spatial separation between the lights, however, the enhancement of the total response decreases with negative acceleration until the total response is not different in magnitude from the response to a single stimulus.<sup>4</sup> From studies on the human retina, it can be judged that an interaction effect at moderate intensities of brightness might occur with as much as 5° separation. An effect extending over 10° or more of the retina is likely to be slight. This would mean that the responses in this experiment were certainly subject to summation when the two lights were adjacent and possible when they were 5° apart. These are independent estimates, of course, and do not follow from the present experiment.

Judging the point of temporal equality between the two lights when they were separated was highly subject to individual differences. This is one of the outstanding features of the data and indicates that this particular discrimination is quite unlike several other visual functions, *e.g.* brightness sensitivity or dark adaptation, in which most *Os* show highly similar patterns. The inter-individual variability, however, greatly decreased when the two lights were adjacent. This is understandable since the juxtaposition of the adjacent flashes permitted a direct comparison; a temporal discontinuity

<sup>3</sup> E. D. Adrian and R. Matthews, The action of light on the eye: III. Interaction of retinal neurones, *J. Physiol.*, 65, 1928, 273-298; R. Granit, Comparative studies on the peripheral and central retina: I. On interaction between distant areas in the human eye, *Amer. J. Physiol.*, 94, 1930, 41-50; R. Granit and P. Harper, Comparative studies on the peripheral and central retina: II. Synaptic reactions in the eye, *Amer. J. Physiol.*, 95, 1930, 211-228; F. A. Geldard, Brightness contrast and Heymans' law, *J. Gen. Psychol.*, 5, 1931, 191-206; R. J. Beitel, Spatial summation of subliminal stimuli in the retina of the human eye, *J. Gen. Psychol.*, 10, 1934, 311-327.

<sup>4</sup> See especially Granit and Harper, *op. cit.*



between the lights was in the nature of an immediate sensation. But judging whether two lights separated by large distances were coming on at the same or different times required more subjective estimation. This could serve to increase the inter-individual variability.

The data for the dark-adapted eye indicate that the peripheral positions have longer latencies than the fovea. This agrees with the reaction-time studies.<sup>5</sup> Unlike the reaction-time studies, however, the comparative latency of the periphery does not increase in any consistent fashion with increase of eccentricity.

*Comparison of scotopic and photopic conditions.* Between the scotopic and photopic conditions, there were two differences: state of adaptation and stimulus-contrast. For the scotopic condition, the eye was dark-adapted and the stimulus appeared against dark surrounds. In the photopic condition, the eye was light-adapted and the stimulus appeared in a field with a luminance of 2.5 millilamberts. It should also be pointed out that the stimuli in the scotopic tests were well above cone threshold in intensity; this probably resulted in a mixture of rod and cone functioning or in some form of rod-cone interaction.

With the lights separated, there were no statistically reliable differences between the photopic and scotopic judgments. In view of the large individual differences in this function, however, the findings should be viewed with caution since but two *O*s were used for the photopic tests. With a greater number of *O*s, it is possible that significant differences would occur.

For the adjacent lights, no valid comparison is possible since the scotopic criterion was different from the photopic ones. Yet an indirect inference could be drawn that the peripheral retina under the photopic condition was more sensitive to time differences. There movement was perceived with delays of the order of 4 m.sec. When these delays were traversed during the scotopic series, there were no indications of any movement being perceived though the *O*s were encouraged to describe and comment freely on what they saw.

*Comparison with critical flicker frequency.* An interesting feature of the data is that the temporal differences discriminated between the two adjacent flashes are less than would be indicated by the values of the critical flicker frequency. For example, a generous estimate of the critical flicker frequency

<sup>5</sup> G. S. Hall and Johannes von Kries, Ueber die Abhängigkeit der Reactionszeit vom Ort des Reises, *Arch. f. Anat. u. Physiol.*, Suppl., 1879, 1-10; A. T. Poffenberger, Reaction-time to retinal stimulation, *Arch. Psychol.*, 3, 1912, (no. 23), 1-73; A. Kästner and Wilhelm Wirth, Die Bestimmung der Aufmerksamkeitsverteilung innerhalb des Sehfeldes mit Hilfe von Reactionsversuche, *Psychol. Stud.*, 3, 1907, 361-392; 4, 1908, 139-200.



at the fovea for the conditions of this study would be  $25 \sim$  per sec.;<sup>6</sup> with a light-dark ratio of 0.5 this would mean that a 20-m.sec. interval between successive flashes can be just barely discriminated. In this investigation, discrimination was possible at the fovea when the two flashes were slightly more than 5 m.sec. apart. One probable reason for the more precise discrimination with the two-spot technique is that the two retinal positions are each excited once, whereas in experiments on the critical flicker frequency one retinal position is intermittently stimulated hence after-effects present from a previous response interfere with the discrimination of the succeeding response.

*Discrimination between adjacent flashes.* Complicating visual effects occur when two adjacent flashes are presented. The influence of summation has already been referred to. Moreover, there is an inhibition of the brilliance of a first flash produced by a second adjacent light flashing after the first.<sup>7</sup> This effect would produce a difference between the apparent brightnesses of the two lights at delays greater than 15 m.sec.

Visual acuity is another factor in discriminating between adjacent flashes when retinal position is varied. The criterion of 'same' or 'different' used in the scotopic condition required seeing two separate lights which could be compared for any temporal difference. If the visual acuity is poor, however, so that the two lights are not clearly distinct, a larger temporal difference might be required to give the impression of two lights flashing at different times. This may be one reason why the thresholds at  $20^\circ$  and  $40^\circ$  of the scotopic curve of Fig. 5 are so much larger than those at the less eccentric positions. Also, this curve is approximately parallel to the curve for the 'two-flash' discriminations (see Fig. 5) under whose conditions the two lights could not be spatially resolved.

Precise temporal resolution, however, between adjacent flashes can occur in the absence of spatial resolution. This was demonstrated in the discriminations by the light-adapted periphery which could not spatially resolve the two lights and yet could reliably detect the appearance of movement caused by a delay between the two flashes. The magnitude of the delays so detected were comparatively very small, being of the order of 4 m.sec. Probably one reason for this precision was the close proximity of the two lights. Accord-

<sup>6</sup> Selig Hecht and C. D. Verrijp, Intermittent stimulation by light: III. The relation between intensity and critical fusion frequency for different retinal locations, *J. Gen. Physiol.*, 17, 1934, 251-268.

<sup>7</sup> G. A. Fry, Depression of the activity aroused by a flash of light by applying a second flash immediately afterwards to adjacent areas of the retina, *Amer. J. Physiol.*, 108, 1934, 701-707.



ing to Korte's laws, the closer together two lights are, the smaller is the temporal interval needed for a perception of movement between them. It was also discovered that the discrimination of movement was reliably more precise in the periphery than in the fovea. This agrees with the previous finding of Cermak and Koffka.<sup>8</sup>

To perceive flicker in the light-adapted periphery required delays of more than 20 m.sec. These values were the highest of the three sets of thresholds for discriminating between adjacent flashes (Fig. 5). One reason for these comparatively high values lies in the requirement of the criterion. To see two successive flashes requires an interval of darkness between the two flashes. Since the duration of each light was 15 m.sec., this would necessitate a delay of more than 15 m.sec. to prevent over-lapping of the two exposures.

#### SUMMARY AND CONCLUSIONS

The temporal sensitivity of the human eye was measured by discriminations between two separate lights which flashed at different temporal intervals. Measurements were made at different retinal positions with dark- and light-adaptation.

Two electronically controlled flash-tubes, placed at different positions on a perimeter, served as the light sources. The two stimuli were physically equal except for a difference in time of onset which was systematically varied. The luminance of each light was 10 millilamberts and the diameter subtended a visual angle of  $14'$ .

Two procedures were used. (1) Two lights, separated in space, were used: one light stimulated the fovea and the other either the fovea, or a spot  $5^\circ$ ,  $10^\circ$ ,  $20^\circ$ , or  $40^\circ$  on the nasal retina of the right eye; (2) two lights, placed tangentially adjacent to each other, together stimulated the right eye at each of the positions mentioned. For each of these conditions, *O* judged whether the two flashes were 'similar' or 'different' in time as the difference in the time of onset of the two flashes was varied. From the reports obtained with the first procedure, a constant error was calculated which indicated the relative latency of one retinal position with respect to another. The probable error of the equality judgments made with the second procedure represented the value for a temporal interval which was just noticeable.

Both procedures were conducted with the eye dark- and light-adapted. Six *Os* were tested under conditions of dark adaptation and two of them

<sup>8</sup> P. Cermak and Kurt Koffka, Untersuchungen über Bewegungs- und Verschmelzungsphänomene, *Psychol. Forsch.*, 1, 1922, 66-129.



were also used for discriminations with the light-adapted eye.

The major findings are as follows:

(1) The latency of reaction in the periphery is greater than at the fovea. The amount of peripheral lag was, in general, of the order of 5-25 m.sec.

(2) Variability among individuals in judging the point of temporal equality between two lights separated in space was marked. The variability was much less when the lights were adjacent.

(3) At the fovea, for either the light- or dark-adapted eye, the just noticeable interval of time between two adjacent flashes was approximately 5 m.sec.

(4) In the light-adapted periphery, a highly precise discrimination of adjacent flashes occurred when movement was perceived by itself, independently of any spatial resolution of the two lights. When the criterion required seeing two separate lights flashing at different times, the thresholds were much higher.

(5) The perception of movement was more precise in the periphery than at the fovea.



## STIMULUS-SIMILARITY AND THE ANCHORING OF SUBJECTIVE SCALES

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The purpose of the present study is to investigate the effects of anchoring on subjective scales of judgment. An 'anchor' is here defined as a stimulus that acts as a standard referent for the stimuli under consideration. It has been generally assumed by workers in this area that the shift introduced by the anchor in the subjective scale of an observer depends on the magnitude of the anchoring stimulus. For example, when an observer judges a series of weights as to their heaviness, the introduction of an anchor is believed to act on the scale strictly in accordance with its degree of heaviness. Thus it is the physical value of the anchor to which the degree of shift in the subjective scale is related.

The above approach is open to question as a general theory of anchoring. In addition to the physical value of the anchoring-stimulus, the manner in which the anchor is perceived by *O* may play a rôle in its effect upon *O*. Above all, it may be crucial whether *O* perceives the anchor as belonging to the other stimuli being judged.

Except for hints at the importance of attitudinal factors by Rogers,<sup>1</sup> there has been no systematic investigation of the rôle of set toward the anchoring stimuli in the shift of subjective-scales. Johnson noted in passing that his *O*'s scales of weight-judgments did not seem to be altered by incidentally picking up cigarettes and other non-experimental stimuli while in the laboratory, but he failed to follow up this observation.<sup>2</sup> Thus, in summarizing our position, we may say that an *O*'s range of experience is organized into systems throughout his life. These systems are composed of experience with stimulus-complexes possessing similarity amongst other things. By similarity, we refer to stimuli which are seen by *O* as forming a class of objects. In other words, things are seen in the class of objects that are equivalent in *O*'s experience. We believe, with Helson, that at any given moment there exists a point of indifference or a level of adaptation

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<sup>1</sup>Spaulding Rogers, The anchoring of absolute judgments, *Arch. Psychol.*, 37, 1941 (no. 261), 1-42.

<sup>2</sup>D. M. Johnson, A systematic treatment of judgment, *Psychol. Bull.*, 42, 1945, 193-224.



located at the weighted mean of the series of experienced stimuli and that judgment of an incoming stimulus involves a comparison of the present stimulus to the present level of adaptation.<sup>3</sup> The weighting involved may be relatively simple—a geometric mean—if the stimuli vary along a single dimension; or it may be extremely complex, if the stimuli vary along numerous interacting dimensions.

What the properties of the system operating at any moment will be, and indeed what system will operate, may be influenced by the manipulation of such factors as the properties of the objects being judged, *e.g.* their color, size, shape; the use *O* made of the objects; and the instructions given *O*. Only those objects which are perceived by *O* as belonging together will be judged with reference to the level of adaptation and will affect that level of adaptation. Experience with objects perceived as unrelated to each other will form a separate system with its own level of adaptation. Between these two extremes of identity and complete separation of systems, there exists a whole realm of organizations involving an interaction of two or more systems each possessing its own level of adaptation but providing a superordinate level of adaptation which governs judgments of objects having some common property (such as weight) though heterogeneous as to such properties as color, shape, and use.

### THE EXPERIMENT

The experiment was designed to investigate systematically the rôle of the following variables on the subjective scale of judgment in a weight-lifting situation: (1) similarity of the anchor to the stimulus-series; (2) the effect of the distance of the anchor from the stimulus-series; and (3) the judging versus the non-judging of the anchors. The method employed was the classical Method of Single Stimuli.

*Experimental variables.* The experimental variables in this study were of three general kinds: (A) independent; (B) dependent; and (C) vitiating.

(A) *Independent.* The independent variables, four in number, were as follows.

(1) *Character of anchor.* The anchors used were of two types. Both were included that the effect of similarity on the subjective scale of judgment could be tested. One type of anchor was the same as the members of the stimulus-series except in physical weight. The other type was a tray instead of a weight, designed to be as different from the stimuli of the series as was feasible within the limits of the situation. Both of these types of anchors will be described in detail in the section on apparatus. This variable is referred to as Variable B; the weight anchor as  $B_1$  and the tray anchor as  $B_2$ .

(2) *Weight of anchor.* Anchors of three different weights were used. This

<sup>3</sup> Harry Helson, Adaptation-level as a basis for a quantitative theory of frame of reference, *Psychol. Rev.*, 55, 1948, 297-313.



variable was included to test the effect of the distance of the anchor from the stimulus-series on the displacement and extension of the subjective scale of judgment. Each value appeared as a tray and as a weight. The values in grams of these anchors will be given in the section on apparatus. This variable is designated by the letter C. Value one is  $C_1$ , value two is  $C_2$ , and value three is  $C_3$ , corresponding to positions 6, 9, and 12 in the stimulus-series.

(3) *Judgment versus non-judgment of anchors.* Two basic conditions obtained in respect to judgment of the anchors. In one,  $O$  was instructed to judge the anchors on the same scale as the stimulus-series, and in the other, he was not asked for judgments regarding the anchors. The specific wording of the instructions will be given later. This variable will be referred to as Variable A, with  $A_1$  standing for judged and  $A_2$  for non-judged.

(4) *Stimulus-weights.* Six weights identical in shape, size, and form comprised the stimulus-series. The value of these weights will be given, also, in the section on apparatus. This variable will be referred to as D. The six different values will be designated by  $D_1$ ,  $D_2$ ,  $D_3$ ,  $D_4$ ,  $D_5$ , and  $D_6$ .

(B) *Dependent variables.* The dependent variable was  $O$ 's judgment of each weight on a 5-point scale. The following indices were derived from these judgments.

(1) *Shift in category assignment.* Shift in assignment of weights to the various categories due to the introduction of the anchor.

(2) *Width of categories.* Change in width of category refers to the number of stimuli in the series judged as being in the same category and the number of categories used to cover the total range of the stimulus-series.

(C) *Vitiating variables.* The following variables which tend to have a vitiating effect upon results, were met and controlled.

(1) *Material-weight illusion.* The tendency to judge weight in accordance with past experience with materials of known specific gravity was controlled by uniformly covering all stimulus-objects and anchors with several coats of black lacquer that  $O$  would be unaware of their composition.

(2) *Time-error.* The time-error, whereby a light weight following in time a heavier weight tends to be judged as heavier than when judged alone, was controlled by having a long random series of presentations to balance out time-errors.

*Apparatus.* The stimulus-objects were a series of weights ( $D_1$ - $D_6$ ) increasing from 80 gm. by increments of one-eighth of their weight to 90, 101.25, 113.91, 128.15, and 144.17 gm., all of the same size, shape, and color, equipped with brass knobs for lifting. The weights are made of brass cylinders lacquered uniformly in black except for the lifting knobs which are not lacquered. They are  $1\frac{1}{2}$  in. in height,  $4\frac{1}{2}$  in. in circumference, and  $1\frac{3}{8}$  in. in diameter. They are filled with wax and lead.

The weight-anchors are exactly like the stimulus-weights in appearance. Anchor  $B_1C_1$  weighs 144.17 gm., Anchor  $B_1C_2$  weighs 205.27 gm., and Anchor  $B_1C_3$  weighs 292.23 gm. As is apparent the anchors increase by increments of three-eighths of their own weight and if the stimulus-scale were extended would be weights 6, 9, and 12 in the series. Anchor  $C_1$  is the same as stimulus-weight  $D_6$ .

The trays had four places designed to hold the stimulus-weights. They were equipped with a brass lifting knob so placed in the center of the tray that there are two weights on each side of the handle when the tray is full. The trays are the same weight as the three anchor-weights. The lifting knobs of the trays are identical with the knobs on the weights in size, shape, and height. The trays are also lacquered black except for the lifting knobs and are filled with lead. They are  $8\frac{1}{2}$  in. long and  $2\frac{1}{4}$  in. wide.



A screen, table, and protocol sheets complete the apparatus.

*Procedure.* The *O*s were run one at a time. *O* was brought into the experimental room and seated at a table opposite *E*. A screen shielded *O*'s view of *E*'s presentation of the stimuli. The instructions were read and a series of 10 weights were given *O*, to familiarize him with the procedure and which he was not required to judge. Then the stimuli were presented in 5 randomized orders of 6 weights each without anchors. *O* judged these on a 5-category scale of 'very light,' 'light,' 'medium,' 'heavy,' and 'very heavy.' After this control series, the anchors were introduced without being so designated to *O*. If he was in a no-judgment condition, he was instructed not to judge the anchor weight, or tray. Fifteen more randomized orders of the 6 weights comprising the stimulus-series were presented for a total of 90 presentations not counting the anchors which were included in the series after every fourth member of the stimulus-series.

*O*'s responses were recorded for every weight judged, by an assistant who set behind *E*. At the conclusion of the experiment, *O* was asked to guess how many

TABLE I

DESIGN OF THE EXPERIMENT: TWELVE EXPERIMENTAL CONDITIONS WITH THE NUMBER OF *O*s SERVING IN EACH CONDITION

		A <sub>1</sub> (Anchor judged)		A <sub>2</sub> (Anchor not judged)		Total
		B <sub>1</sub> (Weight- anchor)	B <sub>2</sub> (Tray- anchor)	B <sub>1</sub> (Weight- anchor)	B <sub>2</sub> (Tray- anchor)	
D (weights)						
C <sub>1</sub> (Anchor 6)	D <sub>1</sub> -D <sub>6</sub>	6	6	6	6	24
C <sub>2</sub> (Anchor 9)	D <sub>1</sub> -D <sub>6</sub>	6	6	6	6	24
C <sub>3</sub> (Anchor 12)	D <sub>1</sub> -D <sub>6</sub>	6	6	6	6	24
Total		18	18	18	18	72

different weights composed the stimulus-series. If they were in a no-judgment anchor series, they were asked to estimate the weight or tray, from memory and then they were asked to lift it and judge it. In the case of the tray, *O* was then asked to determine by the Method of Paired Comparisons the member of the stimulus-series that the tray equalled in weight. In this latter procedure, *O* was presented with the anchor-weights as well as the regular members of the series.

*Experimental design.* There was a total of 12 experimental conditions which were the 12 possible combinations of the three main variables: (A) judged versus non-judged; (B) character of anchor; and (C) weight of anchor. Variable (D), the weight of the stimulus-series weights, ran through all conditions. Six *O*s were run in each of the 12 conditions, giving a total of 72 *O*s. A given *O* participated in only one of the 12 conditions.

The conditions were as follows:

- (1) A<sub>1</sub>B<sub>1</sub>C<sub>1</sub>. Anchor weight at position 6 in the series judged.
- (2) A<sub>1</sub>B<sub>1</sub>C<sub>2</sub>. Anchor weight at position 9 in the series judged.
- (3) A<sub>1</sub>B<sub>1</sub>C<sub>3</sub>. Anchor weight at position 12 in the series judged.



- (4)  $A_1B_2C_1$ . Anchor tray at position 6 in the series judged.
- (5)  $A_1B_2C_2$ . Anchor tray at position 9 in the series judged.
- (6)  $A_1B_2C_3$ . Anchor tray at position 12 in the series judged.

Conditions 7 through 12 are the same as the above except that the anchors are not judged. In other words they are all the combinations of Variable B and Variable C under Variable A. Table I shows the design schematically with the number of Os in each of the 12 cells.

*Instructions.* The following instructions were read to O for Conditions  $A_1B_1C_1$ ,  $A_1B_1C_2$ , and  $A_1B_1C_3$ , in which the anchor is judged.

We are doing an experiment in the judging of the weight of a number of objects. The objects will be presented to you one at a time like this. [A weight is presented on a card in front of O.] You are to grasp each object by the knob with your fingers, using your right hand, and then lift and make a judgment. In making your judgments you are to employ the following five categories running from 'Very Light' to 'Very Heavy': that is, 'Very Light,' 'Light,' 'Medium,' 'Heavy,' and 'Very Heavy.' Judge in accordance with how much it seems to weigh in comparison with the other objects. After lifting each object, place it to your right on this card and proceed to the next object. They will be presented at intervals of 10 sec.

Do not be overly careful or try to be too accurate. Great accuracy is not required. Just relax. Make judgments quickly and spontaneously as to where each object seems to fall in the scale.

Instructions for conditions  $A_2B_1C_1$ ,  $A_2B_1C_2$ , and  $A_2B_1C_3$ , in which the anchor is not judged were the same as above except that after Trial 30 the anchor-weight is introduced with the following additional instructions:

From now on I am going to include another object in the series. [It is pointed out to O.] Please lift this object and put it in the same place as you put the other objects, but do not judge it. To make it easier, I will remind you each time it appears by saying, 'Don't judge.'

Instructions for conditions  $A_1B_2C_1$ ,  $A_1B_2C_2$ , and  $A_1B_2C_3$ , in which the tray is judged were the same as in the first conditions except that after Trial 30 the tray is introduced with the following additional instructions:

From now on I'm going to make this easier for myself by presenting the objects four at a time on this tray. Please lift the objects off the tray and judge them as before. Be sure to lift the objects from your left to right so we know which one you are judging. After judging the fourth object, please lift the tray and judge it just as you did the objects. Then put it on the card where the objects were placed.

Instructions for conditions  $A_2B_2C_1$ ,  $A_2B_2C_2$ , and  $A_2B_2C_3$  in which the tray is not judged were the same as in the last condition until Trial 30. At that point, the tray was introduced with the following additional instructions:

From now on I'm going to make this easier for myself by presenting the objects four at a time on this tray. Please lift the objects off the tray and judge them as before. Lift the objects from your left to right that we may know which object you are judging. After the fourth object, please lift the tray by the knob and put it on the card where the objects were placed in order to make it easier for me to remove and re-present the tray to you with four more objects.

*Observers.* The Os, 72 adults, were selected by the criteria of availability and naïveté as to the purposes of this experiment.



## HYPOTHESES

The following hypotheses were formulated as a basis upon which to test our results.

(A) Conditions in which the anchor is a commercial type weight and is judged, will result in the greatest shift in subjective scales as measured by the shift in category assignments between the control series and the anchor series. Also the width of the categories will be increased the greatest amount under this condition. The magnitude of the effect will be a direct function of the distance of the anchor from the stimulus-series.

(B) Conditions in which the anchor is a commercial type weight and not judged will show the next greatest effect. Again the magnitude of the effect will be a function of the distance of the anchor from the stimulus-series.

(C) Conditions in which the anchor judged is a tray will show less effect than in (B), and the effect will be to lower the level of adaptation for the  $A_1B_2C_1$  and  $A_1B_2C_2$  conditions from the control series due to the size-weight illusion. As before, the magnitude of the effect will be a function of the distance of the anchor in weight from the stimulus-series.

(D) Conditions in which the anchor is a tray and is not judged will show the least effect of all, if any effect.

## TREATMENT OF DATA

To treat the data statistically, it was first necessary to convert the verbal responses of the *O*s into numbers. This was accomplished simply by equating a judgment of 'very light' to '1,' of 'light' to '2,' of 'medium' to '3,' and so on, deriving five numerical categories (1, 2, 3, 4, 5) from the verbal responses.<sup>4</sup>

Every *O*'s responses were then tallied separately for the anchor series and the control series. Although there were 120 presentations of the stimuli in the stimulus-series, only 108 have been retained for statistical treatment. Six of the missing 12 presentations are accounted for by the first set of 6 during the control series which were discarded to insure that the subjective scale had become stable. The remaining were the set from 30 to 36 in the middle of which the first anchor appeared. Since part of this set was in the control series and part in the anchor series, it was felt best to discard it. The mean category-response for each member of the stimulus-series was computed, both for the control and the anchor-series.

Plots were made of the mean category assignments for each of the 12 experimental conditions by summing the mean category assignments for each of the 6 *O*s for any

<sup>4</sup> Converting the verbal responses into a 5-point numerical scale assumes that the verbal scale consists of equal units, and treating the numerical scale statistically assumes that the numbers are cardinal (see H. M. Johnson, Pseudo-mathematics in the mental and social sciences, this JOURNAL, 48, 1936, 342-351). If the assumption of equal units is not correct, then we are not justified in making the conversion nor in treating the converted data statistically. Fortunately, however, Rensis Likert (A technique for the measurement of attitudes, *Arch. Psychol.*, 22, 1932, no. 140, 1-52) reports that, in assigning a 5-point numerical scale to a 5-category verbal scale used with attitudinal questionnaires dealing with International Relations and Negroes, he obtained correlations between the verbal and the numerical scales of 0.987-0.995 with an *N* of 95. He concludes, therefore, that the results justify the use of the simpler scoring method.



given member of the stimulus-series. The control series and the anchor series thus could be plotted to provide a graphic illustration of the shift in the subjective scale of judgment in each of the experimental conditions. These plots are reproduced in Figs. 1-12.

## RESULTS

Hypothesis A predicted that the greatest shift in subjective scales would occur in those conditions in which the anchor was a commercial type weight

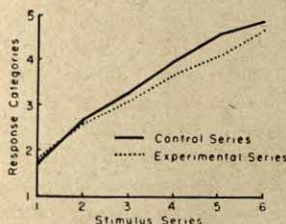


Fig. 1  $A_1B_1C_1$  (Weight Judged)

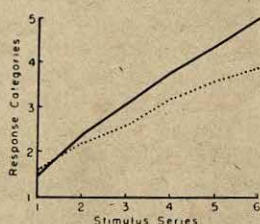


Fig. 2  $A_1B_1C_2$  (Weight Judged)

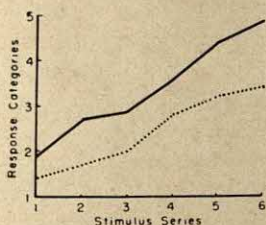


Fig. 3  $A_1B_1C_3$  (Weight Judged)

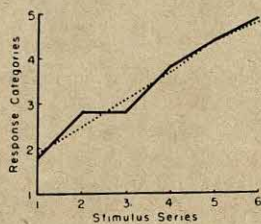


Fig. 4  $A_2B_1C_1$  (Weight Not Judged)

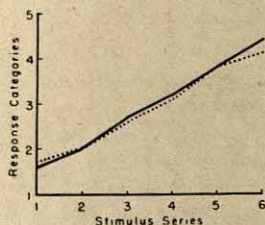


Fig. 5  $A_2B_1C_2$  (Weight Not Judged)

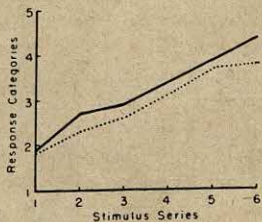


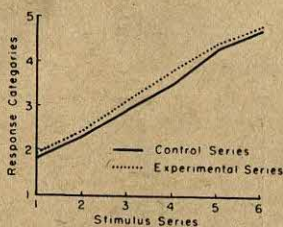
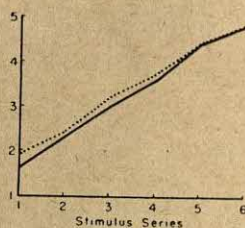
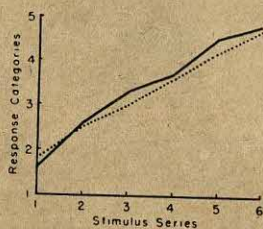
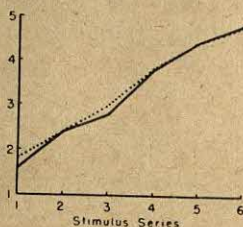
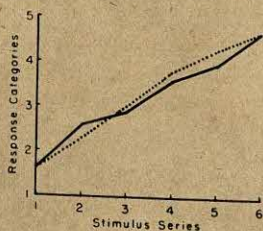
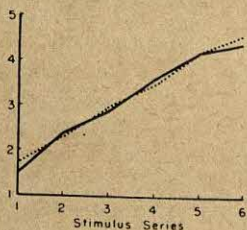
Fig. 6  $A_2B_1C_3$  (Weight Not Judged)

FIGS. 1-6. COMPARISON OF MEAN CATEGORY-ASSIGNMENTS FOR THE CONTROL AND THE EXPERIMENTAL SERIES

and was judged by the Os. Also the degree of shift, as represented by the category width and extension of the scale, would be a function of the distance of the anchor from the series. This hypothesis then refers to our conditions  $A_1B_1C_1$ ,  $A_1B_1C_2$ , and  $A_1B_1C_3$ . Figs. 1, 2, and 3 are the plots of



the mean category assignments made by the 6 Os in each of these conditions. The dotted line represents the mean category assignments for the anchor series and the solid line those for the control series. As is immediately apparent from the graphs, the tendency to judge the members of the stimulus-series nearest the value of the anchor as lighter increases as a

Fig. 7  $A_1B_2C_1$  (Tray Judged)Fig. 8  $A_1B_2C_2$  (Tray Judged)Fig. 9  $A_1B_2C_3$  (Tray Judged)Fig. 10  $A_2B_2C_1$  (Tray Not Judged)Fig. 11  $A_2B_2C_2$  (Tray Not Judged)Fig. 12  $A_2B_2C_3$  (Tray Not Judged)

FIGS. 7-12. COMPARISON OF MEAN CATEGORY-ASSIGNMENTS FOR THE CONTROL AND THE EXPERIMENTAL SERIES

function of the weight of the anchor. Also the greatest difference in category-assignment between the control and the anchor series is found at the upper end of the stimulus-range and increases as a function of the nearness of the stimulus to the weight of the anchor. Furthermore, as the anchor-weights increase—as one goes from Fig. 1 to Fig. 3—the width of the categories increases. This phenomenon also appears to be a function of



the extension of the judgment-scale and the effect again is greatest for the categories nearest to the anchor or, in other words, the categories at the heavy end of the scale. All of these results agree with the classical findings on anchoring effects.

To confirm our hypothesis, however, it is necessary to compare the effects obtained when the anchor-weight was judged with those found when the anchor-weight was not judged, and with those when the tray-anchors were judged and not judged. We shall first consider the comparison between series in which anchor-weights were judged and not judged. This comparison can easily be made by reference to Figs. 4, 5, and 6 which are plots of the mean category assignments by 6 Os in each of our three conditions  $A_2B_1C_1$ ,  $A_2B_1C_2$ , and  $A_2B_1C_3$ . Fig. 4 for the Anchor-Weight 6 shows no perceptible change in the subjective scale between the control and the anchor series. Comparison of Fig. 1 with Fig. 4 shows that the effect for this anchor-value is greater, as predicted, when the anchor is judged. Reference to Fig. 5 shows the expected effects, again, although not nearly so great as in Fig. 2, its comparable situation. Fig. 6 bears out the predictions in that it shows the greatest effects of the non-judged anchors but not so much as is evident in Fig. 3.

Inspection of the figures suggests that Hypotheses A and B have been partly verified. Conditions in which the anchor is a weight and is judged show greater shifts in subjective scales between the control and anchor series, greater increase in category-width, and a greater extension of the scale in the direction of the anchor than do those conditions in which the anchor-weight is not judged. It remains to be shown that these two conditions both show greater effects than those conditions in which the anchor is a tray.

Reference to Figs. 7, 8, and 9 show that the shift in scale produced by tray-anchors are less than in the first two conditions described. Indeed, not only is this the case but Figs. 7 and 8 show a reversal in that the control series is judged even lighter than the anchor series. At first consideration this seems paradoxical. When one considers, however, that the size-weight illusion operates, to lower the level of adaptation in the anchor-series, the results are easily explainable. The anchor affects the scale to the degree of its illusory or perceived weight rather than by its physical weight. Furthermore, comparison of the three Figs. 7, 8, and 9 shows that this illusion effect is a function of the weight of the tray since it decreases slightly in Fig. 8, and the usual effects are obtained in Fig. 9 where the control series is judged as lighter than the anchor series. In any case, the results show that Hypotheses A and B are confirmed in that the effects on the scale are



greatest for the anchor-weight judged, next greatest for the anchor-weight not judged, next greatest for the tray-anchor judged, and as Figs. 10, 11, and 12 show, least of all for tray-anchors not judged. Figs. 10, 11, and 12 show almost no consistent effect, if any at all, demonstrating that the unjudged trays probably were not operating as anchors at all. Evidence from the questionnaire administered at the end of the sessions indicates that not one of the 18 *O*s in conditions  $A_2B_2C_1$ ,  $A_2B_2C_2$ , and  $A_2B_2C_3$  thought of the tray as a member of the series. Most *O*s volunteered the information that they considered it as a tray and not as a weight and that it therefore had nothing to do with their judgments of the stimulus-series.

The graphic evidence indicates that all our hypotheses have been con-

TABLE II

RESULTS OF THE ANALYSIS OF VARIANCE OF THE  $3 \times 2 \times 2$  DESIGN AND THE SUMS OF TWELVE  $6 \times 6$  SIMPLE ANALYSES INVOLVING *O*s TREATED ALIKE

Variable	df	Sum of squares	Mean sum of squares	F Tested against <i>O</i> s within conditions	Significant	F Tested against highest first order interaction	Significant
Main effects:							
(A)—judged vs. non-judged	1	4.49	4.49	11.01	.01	.92	not
(B)—character of the anchor	1	15.53	15.53	38.06	.01	3.18	not
(C)—weight of the anchor	2	10.09	5.04	12.36	.01	1.12	not
First order interaction:							
A $\times$ B	1	4.88	4.88	11.96	.01		
A $\times$ C	2	3.56	1.78	4.36	.05		
B $\times$ C	2	4.18	2.09	5.12	.01		
Second order interaction:							
A $\times$ B $\times$ C	2	.55	.28	.67	not		
Variances from twelve $6 \times 6$ tables							
of weights by <i>O</i> s:							
D—weights	60	15.79	.26				
<i>O</i> s treated alike	60	24.46	.41				
Weights $\times$ <i>O</i> s	300	40.19	.13				
Total	431	123.72					

firmed. Furthermore, some interesting questions relating to the problem of illusion might have been raised in connection with the operation of the tray-anchors. Fortunately, further data are available regarding the operation of this illusion and it has been possible to deal with it statistically.

An analysis of variance was performed to test the significance of the three main experimental variables and their interactions. These variables were: (A) judged vs. non-judged; (B) character of the anchor; and (C) weight of the anchor. Table II gives a summary of the analysis. It was not possible to estimate the variance due to the interaction of the weights with other variables since these interactions are confounded with differences among *O*s. Therefore, it was found necessary to remove the total variance due to weights, and to weights and all its possible interactions with the other variables. This was accomplished by running a separate analysis of variance for each of the 12 experimental conditions in which the variance due to weights and to differences among subjects treated alike were estimated. As



is indicated in Table II, then, the variance due to weights confounded with all its interactions is summed for all 12 conditions and treated as part of the error variance. Estimates of the variances due to the *O*s treated alike and to the interaction of *O*s times weights were obtained by adding the appropriate sums of squares over the 12 conditions and dividing by the appropriate degrees of freedom.

Two tests of significance were carried out and are presented in Table II. First the mean sum of squares due to *O*s treated alike with conditions was used as the error term in the denominator of the *F*-ratio. This variance provides the largest estimate of error and is, therefore, the most conservative estimate of the three possible error variances. Under this circumstance, as can be seen in Table II, all of the main effects are significant at the 1-% level of confidence, two of the first order interactions are significant at the 1-% level and the third is significant at the 5-% level, and the second order interaction is not significant. Treating these results more in detail, we can say that all of our predictions of effects are borne out. The results of the analysis also show that none of the main effects is independent of the context of the other variables. This lack of independence is evident from the fact that the first order interactions are significant and is further demonstrated by the fact that none of the main effects is significant when tested against its highest first order interaction. Thus, under the most rigorous test possible and the one that would allow us to make the most general statements, the main effects by themselves prove not to be significant. We can, therefore, generalize from our results only to the specific conditions of this experiment in which the main effects appear in particular combinations with one another.

### DISCUSSION AND CONCLUSIONS

The fact that a subjective scale conforms to the range of stimulus-weights led earlier investigators to conclude that the scale is anchored by the end stimuli. The anchoring of the scale to the end (lowest and highest) weights seemed to be further confirmed by the fact that the width of the categories varies with the range of the weights. It has thus been pointed out that the width of the categories used, and thus the distribution of judgments over the subjective scale, can be varied by means of end anchors.

Some of our findings confirm previous results, others demonstrate the effects of variables not previously investigated. Our results indicate that the effects of anchors on subjective scales vary with the following conditions.

*Specifying the anchor.* In agreement with Postman and Miller,<sup>5</sup> we found that the usual anchoring effects are obtained even though the anchor is not defined as representing any specific category of the response-scale. In our weight-anchor conditions, the usual shifts in our *O*'s scales occurred even though the anchors did not define a specific category for them.

*Isolating the anchor.* The anchor-stimulus need not even be introduced as an

<sup>5</sup> Leo Postman and G. A. Miller. Anchoring of temporal judgments, this JOURNAL, 58, 1945, 43-53.



anchor or standard in any way that sets it off from the original series. In our series in which the weight-anchor was judged, and in which the anchor was merely another member of the series as far as the *O*s were concerned, the expected results were obtained. This fact, together with the one above, adds further support to Helson's level of adaptation-theory of determination of subjective scales.<sup>6</sup> End anchoring is only a special case of change in adaptation-level. In general when the stimulus-range and the frequency with which certain stimuli are presented are altered, a change in the level of adaptation results.

*Judging versus non-judging of the anchor.* An anchor will retain some of its effectiveness in shifting the subjective scale even though it is not judged or used as a standard by *O*. Thus in our series in which the weight-anchor was not judged, the usual shifts occurred but the magnitude of the effect was decreased.

*Similarity of the anchor.* The anchor, to be effective, must be perceived as a member of the same class of objects as the other weights and as having the attribute being judged by *O*. One factor that has been investigated here which influences the manner in which the stimulus-object is perceived is the dimension of similarity or the classification of the object as seen by *O*. This generalization, of course, is supported by the results of the tray-anchor conditions.

*Size-shape material-use illusion of the anchor.* To the extent that this illusion operates, it is the 'illusory' rather than the actual physical weight which will enter as an anchoring stimulus. It is not possible to demonstrate this conclusively from our results but the trend of the shifts in the series in which the tray-anchor was judged certainly suggest that such is the case.

*Similarity and definition of the stimulus-object.* The dimension of similarity merits special treatment here not only because demonstration of its relevance to judgment-scales was the prime aim of this experiment but also because of the light it casts on the problem of an independent definition of a psychologically adequate stimulus. Stimuli in psychology are not always usefully describable in terms of physical measurements. Most prominent amongst the psychologists who have insisted on independent physical descriptions of the stimulus in the stimulus-response unit is Spence. In an article on the nature of theory construction in psychology, Spence states that the variables among which psychologists attempt to establish functional relations fall into two main groups.<sup>7</sup> These groups are: (1) R-variables which are defined as "measurements of the behavior of organisms; attributes of simple response patterns (actones), complex achievements (actions) and generalized response characteristics (traits, abilities, etc.). These are sometimes referred to as the dependent variables"; and (2) S-variables defined as "measurements of physical and social environmental factors and conditions (present and past) under which the responses of organisms occur. These are sometimes referred to as the independent, manipulable variables."<sup>8</sup>

Spence goes on to discuss the four main types of theory construction in psychology through which we hope to arrive at statements of functional relationships between the environment and behavior. The first two, animistic conceptions and neuro-

<sup>6</sup> Helson, *op. cit.*, 309.

<sup>7</sup> K. W. Spence. The nature of theory construction in contemporary psychology, *Psychol. Rev.*, 51, 1944, 47-68.

<sup>8</sup> *Ibid.*, 48-49.



physiological theories, need not concern us here. It is his attack on "response inferred theoretical constructs" and his defense of "theoretical constructs as intervening variables between S and R variables" in which our interest centers.<sup>9</sup> The latter form of theory construction is typified according to Spence by the theorizing of Tolman and of Hull and is characterized by independent physical measurement of the stimulus variable. Spence refers to all theories which correlate behavior to a 'psychological field' or 'behavioral environment' and which consider the behavior as a function of the latter, without defining the environment independently of the subject's responses, as 'response-response' theories. He centers his attack against 'response-response' theories on the work of the late Kurt Lewin. Spence objects to the assertion that behavior change can not be meaningfully understood in terms of an objectively defined stimulus-variable. He claims that Lewin's approach is not capable of deriving functional relationships or laws between behavior and the stimulating environment. Such laws must be relations between independently defined variables. Lewin's system is based on the relationship between two response variables, neither variable being defined independently of the other. Therefore response-response theories lay themselves open to the criticism that they can not produce laws between independently defined variables; they rely primarily on introspection; and they can not provide the laws necessary to control and manipulate the behavior-determining psychological field to which they apply.

Spence's criticisms of Lewin are in the most part correct and justified. This does not mean that independent physical measurement of stimulus conditions is the only valid approach for psychology however. Rather, let us consider whether Spence's attack on Lewin can be justifiably generalized to all types of response-response approaches. It is our contention that not only is a response-response relationship legitimate in psychological theory but often it is the only type of functional relationship possible to state in simple terms. Of course, this assertion must be defended in the light of Spence's objections and we shall show that a response defined stimulus, such as a similar stimulus, can be defined independently of that response of the subject which is the dependent variable. Furthermore, it is possible to establish empirical relationships or laws which enable one to control and manipulate the behavior of an organism through the use of response defined stimuli as well as by the method of physical specification of the stimuli.

The clearest way to make our point is by defining what a stimulus is and must be in psychology and then to point out the way in which the functional similarity of a stimulus is independently arrived at.<sup>10</sup> A stimulus, then, is an event which is identifiable by the experimenter. To Spence this is no problem since he need only describe the immediate environment of the organism in terms of physical measurements such as the weight in grams of an object or the intensity of illumination on an object. Since it is possible, however, to determine accurately physical magnitudes of many attributes which physiologically can not be present for the organism, a serious problem arises at this point in deciding whether, in fact, such physical dimensions are the most profitable and meaningful for an analysis of the events to which the subjects respond. The alternative is a response-derived unit; *i.e.* a unit

<sup>9</sup> *Ibid.*, 53-62.

<sup>10</sup> I am indebted to Dr. Leo Postman, who influenced my thinking considerably on this point.



defined in terms of the perception of the object by *O*. In our experiment, for example, had we independently defined the tray-anchor stimulus by its physical weight in grams, we would not have been able to predict its differential effects on the subjective scales of our *O*s. By defining the tray-anchor in terms of *O*'s responses to it as a tray and not a weight we are, however, using a phenomenological description of the stimulus. Now we have a dimension of the stimulus to which the *O*'s behavior can be related. In the first case we used physical units and dimensions to identify the stimulus and in the second we used *O*'s responses as well as our own. Thus we have distinguished between response-derived definitions and physical definitions of the stimulus. Even physical definitions in psychology may be response-derived since the only way the effectiveness of a stimulus can be determined is in terms of its effectiveness in producing a response.

The latter point leads us to another characteristic of psychologically meaningful stimuli; *i.e.* whether an event is effective as a stimulus must always be determined by some response of the organism. Obviously, not all physically measurable events are discriminable to the organism either because his sense receptors are inadequate to discriminate them; the magnitude of the stimulus is below the organism's threshold of response; or, the organism is not attending to the stimulus. The latter case is precisely the situation which obtains when we instruct *O* not to judge the anchor-stimulus.

Another characteristic of a psychologically meaningful stimulus is that it should be an event which is specified independently of the response which the investigator wishes to explain by the stimulus. Spence claims that this is where so-called 'field theory' becomes tautological. However true this may be of Lewin's theory, the criticism does not hold for all response-derived definitions of stimuli if certain precautions are attended to. If by 'independent' we mean independent of that response of the *O* which is the dependent variable, then circularity may be avoided. To obtain a definition independent of a given dependent response, it is necessary to define the stimulus either in terms of the responses of other individuals under the same or comparable conditions or in terms of the response of the same individual under specified standard conditions. By independently defining a stimulus, then, we mean specifying it in terms which are independent of the particular experimental conditions to which changes in the response are to be related; we do not mean a specification which is independent of *O*'s responses at all times. Thus to arrive at the definition of our tray-anchor as a functionally dissimilar object to the weights of the series, we presented the tray to 36 *O*s and asked them if it was a weight and, if not, what was it? All 36 responded independently that it was not a weight but a tray. The same is true in arriving at the size-weight illusion. The *O*s judged the tray as lighter than a comparable weight regardless of the experimental conditions being manipulated.

A further restriction on what a stimulus may be is that it can only be a convenient abstraction from the total environment impinging on the organism at any one moment. Which of the multitude of stimuli are selected by the experimenter as relevant under any given conditions can only adequately be determined by an analysis of the response derived characteristics of the stimulus. In this manner the Gestaltists are led to talk about figure-ground characteristics and field relationships when describing a visual stimulus and not only the physical dimensions of the figure.



Naturally, the event we wish to describe as a stimulus must be repeatable in some operationally defined manner since we are interested, as scientists, in the regularities of nature. Due to the complexity of psychologically meaningful stimuli it is necessary to demand that we only repeat a stimulus-situation in its essential common characteristics. One of these common characteristics which define a repeatable class of stimuli is to produce a response of a certain quality and magnitude under standard conditions. In terms of similarity, if a given stimulus different from our tray-anchor, say, a balloon, used under the same experimental conditions produces a shift in subjective scale not significantly different than the shift following the tray, then we may say in response terms that the balloon and the tray are stimuli falling along a dimension of similarity.

In summary, we agree with Spence that the task of the psychologist is that of discovering general laws of behavior but disagree with his insistence that this purpose can only be accomplished by establishing relationships between observed responses and stimuli defined in physical units. Rather, as our dimension of similarity illustrates, we may usefully define the stimulus by a response-derived definition based on the responses of a 'standard' *O* under 'standard' conditions.

The foregoing discussion of the necessity of response-derived definitions may give the impression that physical definitions of stimuli are not possible at any stage. This notion would be extremely repulsive from a methodological viewpoint to many psychologists. Actually we do not make this claim. Rather, we agree that ideally one could proceed in psychology by testing a concept with a wide range of representative stimuli whose possible variations would be only limited by the experimenter's lack of imagination. Each of these stimuli could be physically describable in many dimensions other than say the classical ones of weight or size. The experimenter would then correlate the responses of *O* with each of the stimuli and their dimensions. Then by computing the best regression weights, it would be possible to define the stimuli for *O*. Ideally, then, one could remain at a completely physicalistic level and define all stimuli in all their possible dimensions independent of the response of any *O*. These dimensions could then be adequately dealt with by multiple correlation techniques which would inform us of the significant dimensions in which our interest as psychologists should center. In actuality, however, it is rarely possible to approach complex stimulus-situations in this manner and we prefer to take the shortcut of arriving at response inferred definitions which, once a relevant dimension has been isolated, can then lead to physical specification of the proper dimensions.

Thus the difference between these two approaches seems to be mainly a preference in temporal sequence. The response-derived definition has the advantage of being the most economical in that it first isolates the psychologically adequate dimension and then attempts a physical specification. The physicalistic approach claims to measure all possible dimensions of the stimulus by using a representative design and then, by multiple regression equations, determines the relative importance of each dimension in determining *O*'s response.

#### SUMMARY

A review of the literature on the effects of anchor-stimuli on subjective scales of judgment illustrated the need for further studies in which the



similarity of the anchoring agent to the original stimulus-series could be investigated.

Three variables were selected to investigate: (1) similarity of the anchor to the stimulus-series; (2) judging the anchor; and (3) weight of the anchor. The first variable was tested by two types of anchors: a tray dissimilar to a series of weights and a weight identical to the series of weights. The second variable was dealt with by having *O* either judge or not judge the anchor and the third variable was represented by having three physical values of anchors.

Seventy-two *O*s were randomly split into 12 groups of 6 each. Every group was run on one of 12 experimental conditions in which all possible combinations of the three variables were included. The *O*s judged a series of 120 weights by the Method of Single Stimuli using a five-category scale running from 'very light' to 'very heavy.'

It was hypothesized that the degree of shift in the subjective scale of the judgment would be a function of the three experimental variables and would go from greatest shift to least shift in this order: weight-anchors furthest from the series judged, weight-anchors furthest from the series not judged, tray-anchors furthest from the series judged, and no effect with tray-anchors not judged.

An analysis of variance was run on the  $3 \times 2 \times 2$  table resulting from the data and all predictions were confirmed with statistical significance. Graphic evidence was presented to demonstrate that the order of effect was as predicted and also to illustrate the operation of the size-weight illusion of the tray.

Furthermore, the dimension of similarity was used to illustrate the need for response-derived definitions of psychologically real stimuli.



## FURTHER ANALYSIS OF RESPONSE SEQUENCES IN THE SETTING OF A PSYCHOPHYSICAL EXPERIMENT

By VIRGINIA L. SENDERS, Antioch College

Psychophysical methods, and the statistical techniques used for analyzing the data obtained by them, are based on the assumption that in the absence of stimulus-differences the behavior of the Ss will be determined by chance factors alone. Experiments published as early as 1920 and reviewed in a previous paper indicate that this is not necessarily the case.<sup>1</sup> In the experiment reported in that paper uncertainty functions and autocorrelation functions were used to investigate the occurrence of patterns of response in a situation where no stimulus-differences were present. Results indicated that when the discrimination to be made is a difficult or an impossible one, there may be a tendency for the Ss to repeat the same response, and in some cases this tendency may lead to an S's giving a very long series of the same kind of response. This tendency may, in fact, become so important that actual stimulus-differences are not recognized when they occur, and response-determination (*i.e.* determination by previous responses in the series) has greater effect than stimulus-determination.

In the previous study only two Ss were used, and their results were analyzed intensively. It is the purpose of this experiment to investigate patterning of responses in a much larger group of Ss, and to investigate the effects of several experimental conditions on response patterns. Specifically, the problems which this experiment was designed to investigate, and the principal results found were as follows.

(1) *Correspondence between expected and obtained responses.* In the previous experiment it was found that if the Ss are told that a certain proportion of the stimuli will be of one sort and the remainder of another sort, they will distribute their responses accordingly and may be quite accurate in making their response-ratio correspond to the predicted stimulus-ratio. In the present experiment it was also found that the instructions had a profound effect on the ratio of 'yes' and 'no' responses; however, the correspondence between response-proportions and expected

\* Accepted for publication May 28, 1952. This study was carried out at Antioch College under USAF Contract No. AF 18-600-50 with the Aero-Medical Laboratory, Research Division, Wright Air Development Center. Many people helped with the computations involved in this study, but the assistance of Mrs. Dorothy Webber and Miss Eleanor Lewis, who obtained the data and computed many of autocorrelations, is acknowledged in particular.

<sup>1</sup> V. L. Senders and A. Sowards, Analysis of response sequences in the setting of a psychophysical experiment, this JOURNAL, 65, 1952, 358-374.



stimulus-proportions was not perfect, and the differences between expected and obtained proportions were all statistically significant.

(2) *Procedure employed.* Apparently the Ss oscillated throughout the series. Too many of one kind of response would be given, and then too many of the other kind, and so on.

(3) *Occurrence of response-patterns.* There was evidence of patterning in the results which showed up clearly in the distribution of autocorrelations, though less clearly in the uncertainty functions. In general it may be said that responses are significantly affected by the responses which precede them, although in some Ss the effect is to produce alternation; in others, repetition. Similarly, responses are affected by responses preceding them by three places in the series, in most cases the effect leading to alternation, in a few cases to repetition.

(4) *Ratios and patterns of response.* Response-ratios were affected by the instructions about stimulus-ratio and possibly by the interaction of these instructions with the frequency of the rest-periods. The number of significant autocorrelations seems to increase with the length of the response-series. No other striking effect of any of the experimental variables was found.

#### PROCEDURE AND EXPERIMENTAL DESIGN

The procedure used was essentially the same as that described in the previous paper. The Ss were told that the purpose of the experiment was to test the influence of certain factors on their ability to make a very difficult discrimination. They were asked to judge whether two stimuli, a light and a tone, were simultaneous in onset. Actually the two were always simultaneous. It was explained to the Ss that the discrimination was so difficult that they would seldom feel any confidence in their judgments, but that if they made the best guesses they could they would be more likely to be right than wrong. Considerable time and effort was devoted to convincing the Ss that there were actual stimulus-differences and that, even though they felt no confidence in their judgments, they were actually making discriminations rather than guesses. The Ss were always informed about the length of the response-series, the frequency of rest-periods, and the ratio of simultaneous to non-simultaneous stimulus-presentations which they might expect.

*Subjects.* The Ss were 48 students at Antioch College. They were divided into three groups of 16. Every S of Group 1 gave 100 judgments, of Group 2, 200 judgments; and of Group 3, 300 judgments. Every S made four series of judgments which differed (a) in the predicted ratios of simultaneous to non-simultaneous presentations and (b) in the frequency of rest-periods. The predicted ratios were 1N to 1S; 1N to 2S; 1N to 4S; and 1N to 8S in which N equals non-simultaneity and S equals simultaneity. Rest-periods were given after  $1/10$ ,  $1/4$ ,  $1/2$ , of the series; or none was given. It should be noted that for the three groups the rest-periods occurred after the same fraction of the series but not after the same numbers of responses. The order in which the series were presented was so counterbalanced that, within a group, each combination of ratio and rest-period occurred four times, once in every serial position.

The raw data consist of 64 series of 100 responses each, 64 series of 200 responses, and 64 series of 300 responses—a total of 38,400 responses. Each series was analyzed individually; the various series were also combined in different ways



and the group data analyzed for information about the ratios achieved by the Ss, for periodicity, and for other evidence of non-randomness.

## RESULTS

*Ratios.* An analysis of variance was performed to test the effect of the three main experimental variables (instructed stimulus-ratio, frequency of rest-period, and length of series) on the response-ratios of the Ss. To facilitate analysis of the data, the ratio of 'no' (non-simultaneity) to 'yes' (simultaneity) responses was converted into a 'mean' score, *i.e.* the number of 'yes'-responses minus the number of 'no'-responses, divided by the total number of responses. In other words, a 'yes'-response was arbitrarily called +1.00, and a 'no'-response -1.00, and an arithmetic mean was computed. The analysis of variance performed on the means of the 192 series indicated that only one of the main variables, *i.e.* the instructed ratio, contributed significantly to the total variance. Its contribution was enormous as  $F = 44.14$ . One interaction with ratios—frequency of rest-period—was also sig-

TABLE I  
THEORETICAL AND OBTAINED MEANS

Ratio	Theor. mean	Obt. mean	$\sigma$	Diff.	$\sigma_m$	$t$
1:1	.00	+.15	.15	+.15	.02	7.50†
1:2	+.33	+.27	.21	-.06	.03	2.00*
1:4	+.60	+.50	.13	-.10	.02	5.00†
1:8	+.78	+.66	.14	-.12	.02	6.00†

\* Significant at 5% level.

† Significant at 1% level.

nificant, but only at the 5-% level. In other words, the relative contribution of the interaction to the total variance was so small compared to the contribution of instructed ratios that it may safely be disregarded. The values of the means for different combinations of ratio and rest-period do not show any systematic variation. The group means for different ratios; standard deviations, and standard errors of the means are given in Table I along with the means which would have been found if the ratio of non-simultaneous to simultaneous responses had been what the instructions indicated. Also shown in this table are the  $t$ -ratios between the obtained and theoretical means, all of which are significant at at least the 5-% level. The  $t$ -tests between the various pairs of means show that, as might be expected, all differences are significant at better than the 1-% level.

From all these results we may conclude, then, that what the S was told about the ratio of non-simultaneous to simultaneous stimulus-pairs affected the proportions of 'yes'- to 'no'-responses. It did not completely determine



them, however, as both Table I and Fig. 1 show. In Fig. 1 are presented (a) the group means for each tenth of the series, (b) the cumulative means after every successive tenth, and (c) the means to be expected on the basis of instruction alone. This figure shows relatively little change from one tenth to the next, although in all cases the final mean is slightly higher than the mean of the first tenth.

These group curves, however, do not reflect the enormous fluctuations present in

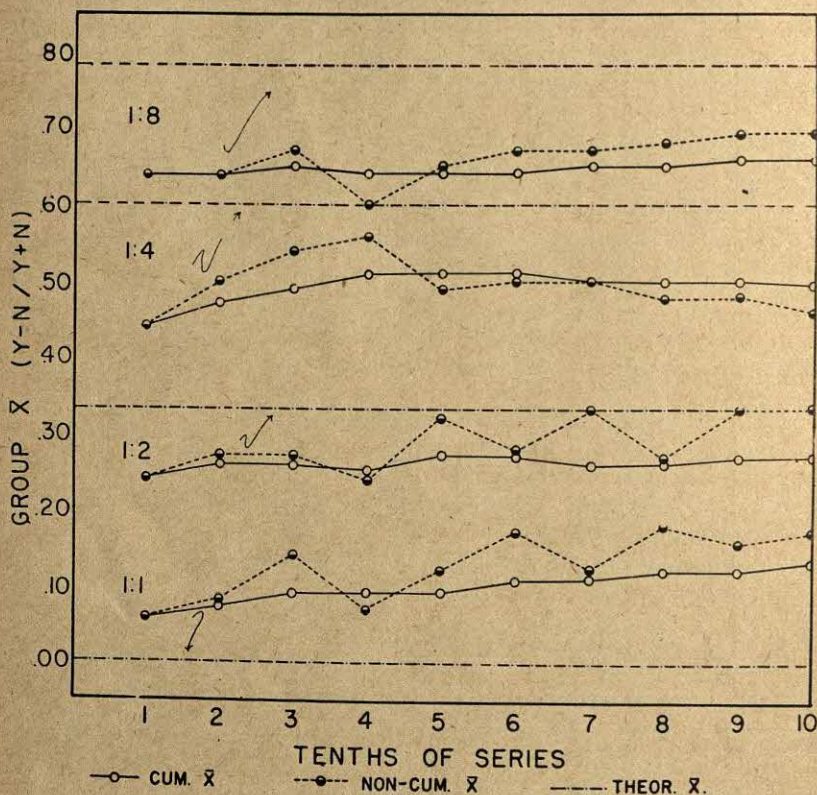


FIG. 1. CUMULATIVE MEANS, MEANS OF SUCCESSIVE TENTHS, AND THEORETICAL MEANS FOR EACH INSTRUCTED RATIO

the corresponding curves for the individual Ss. Because of this the curves give us no information about the process by which the Ss achieved their final means. One might wonder if they did it by a process of 'successive compensation'—giving a series of responses with a preponderance of 'no' followed by a series with a preponderance of 'yes' and the whole cycle repeated throughout the series. Whether the Ss did this can be tested in the following manner.



Suppose, as one limiting case, that every  $S$  was so consistent throughout the series that his mean for each tenth was identical with his means for all other tenths. Suppose, further, that each  $S$ 's mean was different from that of every other  $S$ . If we then compute a correlation between the cumulative mean after  $n$  tenths of the series and the mean of the  $(n + 1)$  tenth, we would obtain a perfect positive correlation.

Now suppose a situation precisely the opposite, that all 48  $S$ s achieve the *same* final mean. However, each  $S$ 's means vary widely from tenth to tenth—for a particu-

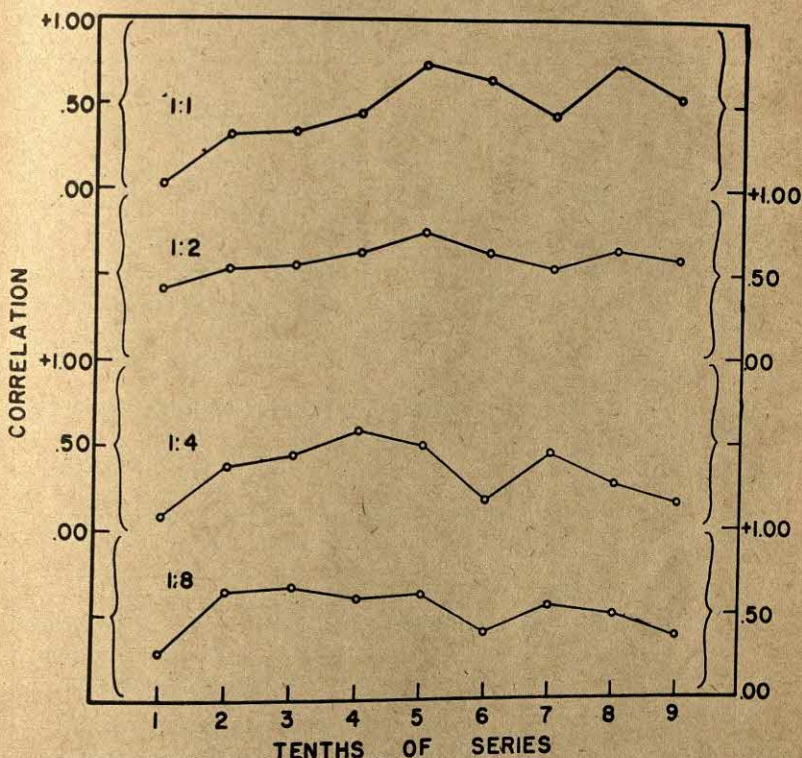


FIG. 2. PRODUCT-MOMENT CORRELATIONS BETWEEN CUMULATIVE MEANS AFTER  $n$  TENTHS OF SERIES AND MEAN OF THE  $(n + 1)$  TENTH FOR EACH INSTRUCTED RATIO

lar tenth one  $S$  may produce a mean which is high, but other  $S$ s will produce means which are low. Suppose now that the  $S$ s successively compensate: that is, the  $S$  whose cumulative mean is highest for the  $n$ th tenth will produce the lowest mean for the  $(n + 1)$  tenth. If we now compute a correlation between the cumulative mean after  $n$  tenths of the series and the mean of the  $(n + 1)$  tenth, we obtain a perfect negative correlation. This is the other limiting case.

On this basis, then, we can distinguish between two types of factors influencing such correlations: one, which would tend to make them high and positive, we can



label *individual difference consistency*; the other, which would tend to make them high and negative, can be called *successive compensation*. The correlations actually obtained reflect the balance between these two counteracting factors.

Product-moment correlations were computed and appear, separately for each ratio, in Fig. 2 as a function of successive tenths of the series. It should be noted that these curves are plotted, not as a function of number of responses, but in terms of fractions of the series, which means that for some Ss a tenth will include 10 responses, for other Ss, 20, and for still other Ss, 30 responses. These curves show the following results: (1) All the correlations are positive, indicating that there is a tendency toward individual difference consistency from tenth to tenth. (2) The highest overall correlations are obtained for the 1:2 ratio, which is also the ratio whose final means have the largest standard deviation. This would indicate that where individual differences are greatest, individual difference consistency will have most influence on the correlations. (3) All curves have two maxima and one minimum—*i.e.* all are alike in rising to a peak, then dropping, then rising again, and then dropping. (We are ignoring the small dip at the fourth tenth of the 1:8 ratio.) This would indicate that, up to a point the Ss become increasingly likely to do what they have done before; after this point they compensate, and then, having compensated, they continue to follow their individual preferences until, near the end, they again compensate. In other words, they behave like servo-mechanisms: behaving, then compensating for any overshooting they have done, overshooting in the opposite direction, compensating for this, and so on.

In order to explore the possibility that the true period of oscillation was in terms of *number of responses* rather than in terms of *fractions of a series* a procedure identical to the one described above was followed, except that, instead of computing means after successive *tenths*, means were computed after successive groups of ten responses. This was done for all ratios separately and for the combination of all four. These curves also showed increasing correlations for the first few points and then oscillation, with an average period of about twenty-five responses. The curves for the different ratios were, however, somewhat different in appearance and, except for the general tendency toward oscillation, no regularities were seen. The combined curve was quite flat. Thus it would appear that the Ss compensate, not in terms of any absolute number of responses, but in terms of the total task before them.

*Predictability.* Predictability refers to our ability to predict whether any given response made by an S will be a 'yes' or a 'no.' If S has an equal number of positive and negative responses, and if these are distributed quite randomly, predictability is nil. Several conditions may increase predictability: (a) a large unbalance of positive and negative responses, which would mean that a prediction of the more frequent response would be right more often than wrong; (b) any regularity in the manner in which 'yes' followed 'no,' or any longer sequence of preceding responses; and (c) any regular periodicity, for example, a sequence of 'yes' and 'no' repeated after every ten responses. Predictability of responses can be described quantitatively in two ways: the *uncertainty function* will show a drop if any in-



crease in predictability over chance is present, regardless of whether this increase is or is not due to periodicity; the *autocorrelation function* will show significant positive or negative values if periodicity is present. Both techniques were used in the present investigation.

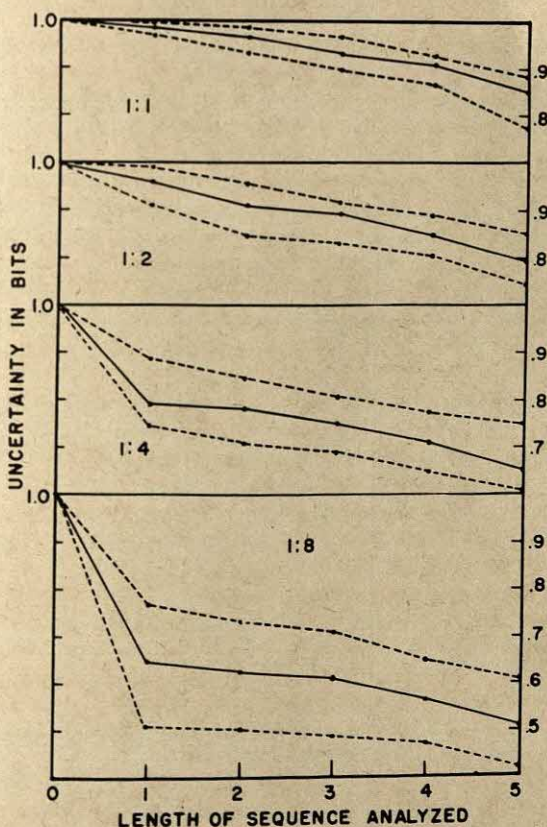


FIG. 3. MEDIAN UNCERTAINTY FUNCTIONS (SOLID LINES), AND  $Q_1$  AND  $Q_3$  (BROKEN LINES) FOR EACH INSTRUCTED RATIO

(a) *Uncertainty functions.* Uncertainty functions were computed by the techniques described by Miller and Frick<sup>2</sup> with the additional computational aids published by Newman.<sup>3</sup> This function describes the decrease in uncertainty of prediction as a function of the number of preceding responses which are known. Maximal uncertainty

<sup>2</sup> G. A. Miller and F. C. Frick, A statistical description of operant conditioning, this JOURNAL, 64, 1951, 20-36.

<sup>3</sup> E. B. Newman, Computational methods useful in analyzing series of binary data, *ibid.*, 252-262.



is one *bit*, which is the amount of information contained in one two-alternative choice. Minimal uncertainty is zero, and is equal to perfect predictability. If knowledge of past performance in any way increases, over pure chance, the ability to predict performance, these functions will show a drop from the horizontal straight

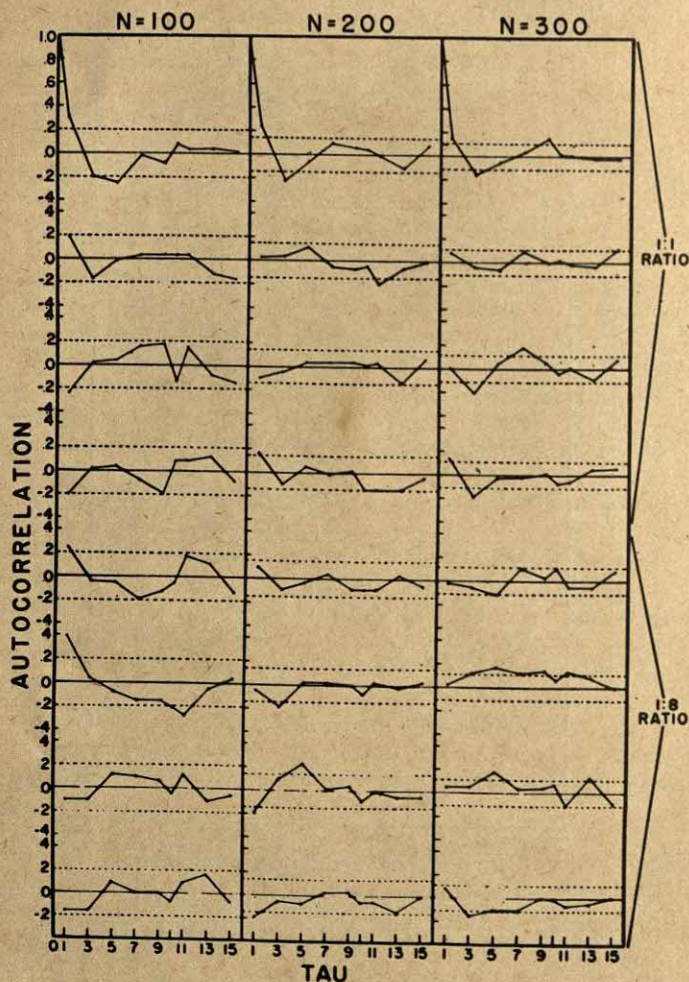


FIG. 4. SAMPLE AUTOCORRELATION FUNCTIONS FOR 24 SERIES

line which indicates maximal uncertainty. Any unbalance between the number of 'yes'- and 'no'-responses is reflected in the drop in uncertainty between abscissa values of zero and one, but a drop after that point indicates that certain responses tend to follow other responses. The longer the function continues to drop, the greater is the number of responses which is included in a regular pattern. Median uncertainty functions for the four different ratios are plotted in Fig. 3.



It will be noticed immediately that the drop in the first segment of the curve increases as the ratio changes from 1:1 to 1:8, and this fact is quite in accord with the increasing unbalance of 'yes'- and 'no'-responses. From a sequence length of one to a sequence length of five responses, the drop in uncertainty is about the same for all ratios—a drop of about 0.14 bits, indicating that some patterning is present but not enough for a great increase in predictability over pure chance. From the plotted values of  $Q_1$  and  $Q_2$ , however, it may be seen that variability of the uncertainties increases as we go from the 1:1 ratio to the 1:8 ratio, indicating that some Ss show more pronounced patterning in the latter ratio than in the former. Uncertainty functions were also plotted for different combinations of rest-period and length of series, but no other variables were found to affect these functions significantly.

(b) *Autocorrelations.* An autocorrelation is a correlation between each event (in this case an event is a response) in a series, and the event which preceded it by some given number of events. The number of events separating the two to be correlated is usually known as *tau*. Thus in our case, an autocorrelation for a *tau* of one would be a correlation between response one and response two, response two and response three, response three and response four, and so on. Binary autocorrelations were computed for all odd values of *tau* from one to fifteen and also for ten (on the supposition that creatures who habitually operate on a decimal system might show significant periodicity at this frequency). There were 192 series, and nine values of *r* were computed for each series, making a total of 1728 autocorrelations. Individual functions were plotted for each series, and 24 sample functions are given in Fig. 4. Careful inspection of the functions, both those presented here and the 168 which are not, reveals no consistent peaks or troughs—an occasional bump occurs, but such bumps would be expected by chance, and it appears at first that no periodicity is revealed by the autocorrelation functions. All appear to drop quickly to zero and to vary slightly above and below zero for the rest of the function. To test the null hypothesis that there were no deviations from zero except those which would be expected by chance, the following procedure was followed:

In the single case of binary autocorrelation, it can be shown that  $Nr^2 = X^2$ .<sup>4</sup> Therefore it is possible to determine for any *N* the value of *r* which is significant at each level. We would expect, of course, that if only chance factors were operating, 1% of the autocorrelations would be significant at the 1-% level, 5% at the 5-% level, and so on. Having 1728 autocorrelations, we can test this hypothesis directly. The fact that the correlations for different values of *tau* are themselves correlated (each autocorrelation function has nine values which may be intercorrelated) makes this test somewhat difficult to use. The autocorrelations may, however, be tabulated separately for each value of *tau*, and in this case, the only correlation which can exist between autocorrelations is for different series obtained by the same individual. Even this can be eliminated by not combining results for the same *S*.

This test was then made for the 5-% level of significance. The number of autocorrelations significant at the 5-% level was determined separately for each value of *tau*, for each ratio, and for each length of series. The results are given in Table II and are plotted in Fig. 5. They are indeed striking. Many more correlations are significantly high or low than would be expected by chance alone. The fact that some

<sup>4</sup> Miller and Frick, *op. cit.*, 20-36. Also personal communication from G. A. Miller.



are high and some are low means that visual examination of the autocorrelation functions would not reveal consistent tendencies because the tendencies are not, in fact, consistent. Thus we can say with some assurance that responses are affected by the responses which precede them, but we cannot predict in advance in which direction this effect will operate. For a  $\tau$  of one, more responses are significantly high than are significantly low, but many are low also. The reverse is true for a  $\tau$  of three. This effect increases greatly as  $N$  increases from 100 to 300, but it holds, nevertheless for all  $N$ s.

The differences between groups with different  $N$ s are puzzling, but the direction

TABLE II  
PERCENTAGE OF AUTOCORRELATIONS SIGNIFICANT AT 5-% LEVEL  
(Chance = 2.5%)

Ratio	N	Tau									
		1	3	5	7	9	10	11	13	15	All
1:1	100+	6.25	.00	.00	.00	.00	.00	6.25	6.25	12.50	3.47
	—	18.75	6.25	12.50	6.25	6.25	.00	12.50	6.25	.00	7.64
	200+	50.00	.00	12.50	.00	.00	.00	.00	.00	.00	6.94
	—	6.25	31.25	.00	12.50	.00	6.25	12.50	6.25	.00	8.33
	300+	50.00	.00	6.25	12.50	12.50	6.25	12.50	12.50	12.50	13.89
	All+	12.50	43.75	12.50	12.50	.00	.00	.00	6.25	12.50	11.11
1:2	100+	35.42	.00	6.25	4.17	4.17	2.08	6.25	6.25	8.33	8.10
	—	12.50	27.08	8.33	10.42	2.08	2.08	8.33	6.25	4.17	9.03
	200+	31.25	6.25	.00	.00	6.25	.00	6.25	.00	.00	5.56
	—	.00	25.00	6.25	.00	.00	6.25	12.50	.00	.00	5.56
	300+	31.25	6.25	6.25	6.25	.00	.00	.00	.00	.00	5.56
	All+	6.25	18.75	.00	.00	.00	.00	.00	6.25	.00	3.47
1:4	100+	50.00	6.25	6.25	6.25	12.50	12.50	6.25	6.25	.00	11.81
	—	18.75	25.00	18.75	6.25	6.25	.00	6.25	.00	.00	9.72
	200+	37.50	6.25	4.17	4.17	4.17	4.17	4.17	2.08	.00	7.41
	—	8.33	22.92	8.33	2.08	2.08	4.17	4.17	4.17	.00	6.25
	300+	25.00	.00	6.25	.00	6.25	.00	.00	.00	12.50	5.56
	All+	12.50	6.25	6.25	.00	.00	6.25	.00	12.50	.00	5.56
1:8	100+	43.75	25.00	12.50	12.50	25.00	12.50	12.50	6.25	18.75	18.75
	—	25.00	25.00	12.50	.00	12.50	6.25	6.25	.00	.00	9.72
	200+	37.50	.00	6.25	18.75	18.75	6.25	31.25	6.25	6.25	14.58
	—	25.00	31.25	6.25	.00	.00	6.25	.00	6.25	6.25	9.03
	300+	35.42	8.33	8.33	10.42	16.67	6.25	14.58	4.17	12.50	12.06
	All+	20.83	20.83	8.33	.00	4.17	6.25	2.08	6.25	2.08	7.87
All	100+	31.25	.00	.00	.00	.00	.00	.00	.00	6.25	4.17
	—	6.25	12.50	6.25	.00	.00	6.25	6.25	.00	.00	4.17
	200+	25.00	.00	6.25	6.25	.00	12.50	12.50	.00	6.25	7.64
	—	25.00	31.25	6.25	.00	.00	.00	.00	.00	.00	6.94
	300+	25.00	6.25	12.50	18.75	12.50	25.00	6.25	6.25	6.25	13.19
	All+	18.75	25.00	12.50	.00	.00	.00	6.25	6.25	.00	7.64
All	100+	27.08	2.08	6.25	8.33	4.17	12.50	6.25	2.08	6.25	8.33
	—	16.67	22.92	8.33	.00	.00	2.08	4.17	2.08	.00	6.25
	200+	23.44	1.56	1.56	.00	3.12	.00	3.12	1.56	7.81	4.69
	—	9.38	12.50	7.81	1.56	1.56	4.69	7.81	4.69	.00	5.56
	300+	37.50	7.81	9.38	6.25	6.25	6.25	6.25	1.56	6.25	9.72
	All+	15.62	26.56	4.69	3.12	3.12	3.12	4.69	1.56	.00	6.94
All	100+	40.62	3.12	7.81	14.06	14.06	12.50	14.06	7.81	6.25	13.37
	—	18.75	31.25	12.50	4.69	1.56	3.12	1.56	6.25	4.69	9.37
	200+	33.85	4.17	6.25	6.77	7.81	6.25	7.81	3.65	6.77	9.26
	—	14.58	23.44	8.33	3.12	2.08	3.65	4.69	4.17	1.56	7.20
	300+										
	All+										

of the trend is consistent with the results of the previous experiment. In that case it was found that as the experiment progressed one  $S$  gave longer and longer sequences of the same response. In this experiment there are, for the 300 group, many significant positive autocorrelations for high values of  $\tau$ . The notion that a response is influenced by a response which preceded it by 13 or 15 places seems absurd at first; but the positive autocorrelations in this case would actually mean that responses may occur in sequences as long as 13 or 15, which is fairly plausible. It should be noted further that the many significant negative autocorrelations at a  $\tau$  of three are not necessarily obtained from the same  $S$ s as the positive autocorrelations at higher values of  $\tau$ .



## DISCUSSION

Successive responses in a series are not independent. In situations where stimulus-differences, or stimuli themselves, are non-existent, subthresh-

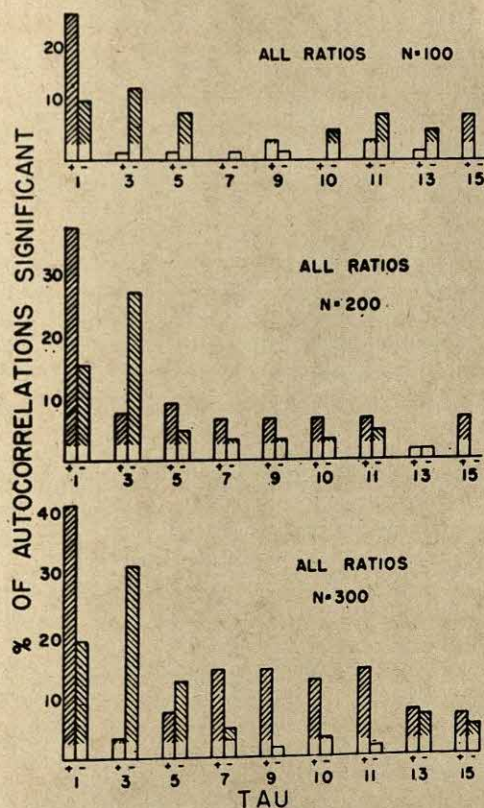


FIG. 5. PERCENTAGE OF AUTOCORRELATIONS SIGNIFICANT AT THE 5% LEVEL. Positive autocorrelations are plotted on the left of each pair of bars, and negative autocorrelations on the right. Since  $X^2$  is a two-tailed test, chance expectancy is 2.5% significant, and the excess is cross-hatched.

old, or near threshold, this non-independence has been demonstrated by Fernberger, Turner, Arons and Irwin, Preston<sup>5</sup> and recently by Collier.<sup>6</sup> Collier presented his Ss with series of liminal and subliminal flashes of light at intervals of 5 sec. under various combinations of monocular and

<sup>5</sup> The work of all of these investigators was reviewed by Senders and Sowards, *op. cit.*, 358-360.

<sup>6</sup> G. H. Collier, An investigation of the independence of successive responses for behavior at the visual threshold, *Amer. Psychol.*, 6, 1951, 278.



binocular fixation and found non-independence of successive responses under all experimental conditions.

Previous results and results in the present experiment are not in agreement about the nature of the non-independence of successive responses. Most of the previous findings suggested a significant tendency to avoid repetition, whereas results of the present experiment suggest a significant tendency to repeat. Most of the early work was done, however, with three categories of judgment permitted the *S*, and this difference may have accounted for the difference in results. Results of *ESP* experiments show neither a tendency to repeat nor a tendency to alternate,<sup>7</sup> but rather a tendency to avoid any regular patterning—to such an extent that 'asymmetrical' patterns become regular and predictable. These results may be explained, however, by the hypothesis that *Ss* make their guesses conform to what they conceive to be a chance or random distribution of events.

Several explanations of the results of the present experiment are possible; any one of these might account for the results, or several might be invoked together.

The simplest explanation is that *S* naturally tends to give certain patterns of responses and to avoid others. This tendency would presumably be either independent of *S*'s perceptions of the stimuli, or it might on occasion be dominant over them. This explanation is actually no explanation at all, since all it says is that certain things happen with more regularity than chance alone would allow. Furthermore, it does not explain why habits of guessing differ in *ESP* experiments, in psychophysical experiments with three categories of response, and in experiments with only two categories. It does not, furthermore, account for the effect of length of series found in this experiment, nor does it account for the finding of our earlier paper that the length of a series of repetitions increased as the experiment continued.

Skinner has suggested that, other things being equal, a response is more likely to be repeated than to be changed.<sup>8</sup> A previous repetition will, however, increase the probability of a change, and a previous alternation will increase the probability of a repetition. With this explanation Skinner was able to account for Goodfellow's 'asymmetrical' groups of responses. If the same explanation were to be applied to the present data, it would have to postulate a much stronger original preference for repetition since a backlog of many repetitions was required to produce a change or alternation.

A different type of explanation is based on the assumption that thresholds fluctuate from moment to moment. How long a moment is we do not know, but if a moment is of finite duration and is long enough to include several responses, then it would be expected that responses occurring within the same moment would be the same. A change of response might actually reflect a change of threshold. Such an explanation might well account for Collier's data but would be difficult to apply to the

<sup>7</sup> L. D. Goodfellow, A psychological interpretation of the results of the Zenith radio experiments in telepathy, *J. Exper. Psychol.*, 23, 1938, 601-632.

<sup>8</sup> B. F. Skinner, The processes involved in the repeated guessing of alternatives, *ibid.*, 30, 1942, 495-503.



present experiment where the judgment to be made was not truly a threshold discrimination. To account for these data, such an hypothesis would have to postulate differences in times of conduction in the visual or auditory pathways which fluctuate in the same manner as thresholds are supposed to.

Fluctuations in attention or set, rather than in thresholds, however, could produce similar results. If *S* attended to the auditory stimulus for several responses, and then to the visual stimulus for several responses, his judgment of simultaneity would be affected. That such differences in attention can significantly influence judgment was demonstrated by the effect of instructions in the present experiment: if *S* was told: "Listen, and tell which comes on first" he would usually respond 'tone,' while if the word 'watch' were substituted for the word 'listen' he would be more likely to respond 'light.' In the present experiment, however, his judgment was not 'tone vs. light' but 'simultaneity vs. non-simultaneity,' hence, if fluctuations of attention are to account for the results, they must be fluctuations between concentration on either stimulus, and concentration on both equally. Such fluctuations are harder to explain. One can imagine, however, an *S* sometimes expecting a simultaneous pair and sometimes a non-simultaneous pair and finding his expectations confirmed.

A more plausible explanation is one that includes some consideration of the fluctuations of set, plus the fact that *S* has an expectation, implanted by the experimenter, of the relative proportion of each kind of stimulus and of the known fact that one judgment in a series is made, not absolutely, but in relation to previous judgments. (In other words, anchoring effects occur.) *S* starts his judgmental series with the knowledge, let us say, that he will make 200 judgments of which, if he is to be 'correct,' two-thirds (about 133) will be 'simultaneous.' His set, therefore, is likely to be 'simultaneous unless proved otherwise.' He judges the first pair of stimuli 'simultaneous' and then asks himself: "Will the next pair be the same as the previous pair or detectably different?" If he can detect no difference between the second pair and the pair which has preceded it, he will also judge the second pair 'simultaneous.' This process may continue until, at some point, he says to himself: "I'm getting too many of these simultaneous. I'll have to try harder to detect the difference." With such a set, he is more likely to find a difference, even though none is present. Having found a difference, the next pair of stimuli is likely to be judged in terms of the pair in which a difference was found. If no detectable difference between the pairs of stimuli occurs, the next pair will also be judged 'non-simultaneous.' This will continue until *S* again decides that he is getting 'too many' non-simultaneous pairs. The successive compensation found supports this explanation. The fact that more significantly high autocorrelations were found for the longer response series is accounted for by the fact that more identical responses can be made without *S* feeling that he has given 'too many.' As Fig. 1 shows, however, more 'simultaneous' responses are given at the end of a series than at the beginning which is in direct contradiction to the predictions of the above explanation. (Incidentally, if this explanation is correct in its assumption that *S* makes his judgment about any stimulus at least partly in terms of the similarity of that stimulus to preceding stimuli, then the usual laboratory emphasis on *consistency* as the most desirable characteristic of a psychophysical observer would *maximize* the occurrence of non-random sequences of responses.)

Another possible explanation is that *S* may, in the course of the experiment, adopt an 'indifferent' attitude. Difficult discrimination follows difficult discrimination with



no reinforcement whatsoever, and *S* may decide that since he can discern no basis for judgment there is no use in trying to judge—one answer is as good as another. If one answer is as good as another, and if Skinner is right in saying that it is easier to repeat than to alternate, *S* would eventually give long series of responses. The length of the series would be limited principally by the knowledge that only a given fraction of the answers should be 'yes' or 'no.' This theory is in accord with the introspections of one of our *Ss* in the previous study who gave very long series of the same response followed by very long series of the other, and said that "I'd just say the first thing that came into my mind. Then after a while I'd decide I'd said too many of those, so I'd change." The explanation is also in accord with the conclusions about the successive compensation occurring during the series, and in accord with the fact that there were more significant autocorrelations in the long series. It is also supported by the fact that more stimuli were judged non-simultaneous at the beginning of a series than at the end. Such an explanation is a hopeful one for conventional psychophysics because it suggests that if the discrimination is not impossibly difficult, and if *S* is not fatigued or bored, and if no series is too long, the tendencies found in this experiment would not be in evidence.

Before we can know whether the problems raised by the finding of non-independence are really troublesome, we must know whether responses are non-independent when the discriminations required are above threshold. Certainly the periodicities found in this experiment would not be found in a situation where stimulus-differences were so great that their recognition by *S* was inevitable. It seems probable that a continuum exists between response-determination and stimulus-determination, a continuum on which psychophysical experiments, projective tests, and attitude studies may all find a place. In a search for periodicities or guessing habits, only one end of this continuum, the response-determined end, has been thoroughly explored. The investigation of other points on the continuum is a problem which should be undertaken—preferably by someone with access to an autocorrelator. If the results are positive, our techniques and theories must be modified to take account of response determination.

Finally, it may be argued that, although these results are significant, the magnitudes of the autocorrelations found were small. This is true but also inevitable. If human beings were such regular creatures that, when trying to generate random series they *always* inserted the same periodicities, we would have known about it long before now. Of course the tendencies are slight! The startling thing is that tendencies are present at all. If the slightness of the tendencies means that they do not affect results when real stimulus-differences are present, so much the better for all past and all future work, but the basic assumption of almost all our statistical treatments is in question, and until that question has been resolved by further research, complete confidence in our interpretation of results will not be possible.



## THE COIN PROBLEM: A STUDY IN THINKING

By MARIANNE L. SIMMEL, College of Medicine, University of Illinois

Previous investigators in the area of thinking and problem-solving have analyzed the processes of solution in terms of fairly general principles of behavior, e.g. *resonance* and *difficulty in re-centering*.<sup>1</sup> The present study is concerned with the peculiar aspects of problems which, at least in some cases, may be responsible for *resonance* or *difficulties in re-centering*.

In the course of a series of experiments on problem-solving some rather striking phenomena appeared, which called for more detailed examination. Specifically, we found that almost all our Ss made the same error in attacking the following problem.

A group of eight similar coins may or may not contain a single counterfeit coin, which is lighter than the good coins. By means of only two weighings on a balance S is to establish whether there is a counterfeit coin and, if so, which one is counterfeit.<sup>2</sup>

The common error consists in an initial division of the eight coins into two groups of four coins. No simple explanation in terms of habit, set or motivational needs can reasonably be adduced to account for this mistaken attack. Rather, it seems as if certain aspects intrinsic to the problem

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<sup>1</sup> Karl Duncker, On problem solving, *Psychol. Monog.*, 61, 1947, (no. 270), 1-58; A qualitative (experimental and theoretical) study of productive thinking (solving of comprehensible problems), *Ped. Sem.*, 33, 1926, 642-708; N. R. F. Maier, Reasoning in humans: I. On direction, *J. Comp. Psychol.*, 10, 1930, 115-143; II. The solution of a problem and its appearance in consciousness, *ibid.*, 12, 1931, 181-194; An aspect of human reasoning, *Brit. J. Psychol.*, 24, 1933, 144-155; L. Székely, Studien zur Psychologie des Denkens: Zur Topologie des Einfalls, *Acta Psychol.*, 5, 1940, 79-95; Die Bedeutung der Situation für das Denken, *Theoria* 9, 1943, 1-21; The dynamics of thought motivation, this JOURNAL, 56, 1943, 100-104; M. E. Bullbrook, An experimental inquiry into the existence and nature of 'insight,' this JOURNAL, 44, 1932, 409-453.

<sup>2</sup> The solution depends on the S's recognition of two methods of weight-determination: one, by *weighing* coins, i.e. by putting equal numbers of coins on the two arms of the balance; the other, by logical elimination. We may illustrate these two methods by taking the simplest case, that of three coins, known to include an underweight counterfeit which is to be discovered in one weighing. If one coin is put on the right and one on the left arm of the balance, the third coin remaining on the table, one of the following two alternatives can be read off from the behavior of the balance: (a) if one of the two coins on the balance is lighter, it is the counterfeit. (b) if the two coins on the balance are equal in weight, i.e. if the two sides remain in equilibrium, then the third coin which has not been weighed must be the counterfeit, identified here by means of logical elimination.



'snared' the Ss, luring them in the wrong direction. It is these intrinsic aspects which we decided to explore.

#### METHOD AND PROCEDURE

*Subjects.* Two groups of Ss took part in this study. Group I consisted of 21 undergraduates majoring in a variety of fields and selected by their tutors as being the most gifted in their respective departments. Their ages ranged from 17 to 29 yr., with a mean age of 22 yr. This group was studied intensively in the spring of 1948 when they served as subjects in a more comprehensive project.

Group II consisted of 37 Ss who were studied subsequently in the fall of 1948 and spring of 1949. This group was more heterogeneous than Group I, comprising undergraduate and graduate students as well as members of the teaching and research staff from a variety of fields. The ages of the members of this group ranged from 16 to 46 yr., with a mean of 24 yr.

*Procedure.* The experiments were conducted in individual sessions. S was seated at a table and the problem was presented to him typewritten on an index card with the following instructions: "Here is the problem; read it through and try to think aloud as you go about solving it." S was not allowed to use pencil and paper. 'Thinking aloud' did not appear to be of major difficulty for these rather verbal groups. Full protocols were taken in writing by E. Group I solved the 8-coin problem only, Group II solved three problems: an 8-, a 9- and a 25-coin problem. Alternate Ss in Group II were given 8- and 9-coin problems in alternate orders. Group IIa was given the 8-coin problem first, then the 9-coin problem. Group IIb was given the 9-coin problem, then the 8-coin problem. The 25-coin problem was always given last. The texts of the problems were as follows:

*Eight-coin problem.* You have eight similar coins and a balance. At most one coin is counterfeit and hence underweight. How can you determine whether there is a counterfeit coin and if so, which one, by using the balance only twice?<sup>3</sup>

*Nine-coin problem.* You have nine similar coins and a balance. You know that one coin is counterfeit and hence underweight. How can you find the counterfeit by using the balance only twice?<sup>4</sup>

*Twenty-five coin problem.* You have twenty-five similar coins and a balance. You know that one coin is counterfeit and hence underweight. How can you find the counterfeit by using the balance no more than three times?<sup>5</sup>

#### RESULTS

In the original experiments (Group I) only the 8-coin problem had been included. Analysis of the protocols of the solution processes revealed a surprising uniformity of first steps. Of the 21 Ss in this group 18 began by proposing to place four coins on each end of the balance. Two Ss of the remaining three had worked through the same problem recently and immediately gave the correct solution. If we add those Ss in later experiments who received the 8-coin problem as a first problem (Group IIa), we find

<sup>3</sup> Solution: 3 vs. 3 coins on first weighing; 1 vs. 1 on second weighing.

<sup>4</sup> Solution: same as the 8-coin problem.

<sup>5</sup> Solution: two possibilities on first weighing, 8 vs. 8 or 9 vs. 9; second and third weighings as in 8- or 9-coin problems.



that 34 out of a total of 39 Ss started with the four-four division and that all but one of the remaining Ss had had recent experience with the identical problem and gave the correct solution at once. Moreover, almost half of the Ss in Group I and similar proportions of the Ss of Group II had at one time or another heard the problem and solved it in the experimental situation without much difficulty, but *not* before verbalizing the four-four division which was occasionally given in the form 'I know four and four does not work.'

Why did such an overwhelming majority of the Ss begin in this fashion, which from the point of view of the final and only solution, is a mistake? Why is it that even Ss who know the problem have difficulty in resisting this four-four division? What is responsible for this almost universal error?

On purely a priori grounds it appears that three different factors individually and in their various combinations might account for this initial step: (1) symmetry; (2) totality; and (3) arithmetic divisibility or arithmetical physiognomy.

(1) *Symmetry*. The factor of symmetry, arising out of the very concept of a balance, would seem to account for one aspect of the initial four-four division; namely, the balancing of *equal* numbers of coins. The correct appreciation of the properties of a balance is a necessary though not sufficient condition for the solution of the problem. Not a single S, when first presented with the 8-coin problem, suggested initially the balancing of unequal groups of coins. While this factor accounts for the division into two groups of equal numbers of coins, it does not account for the almost universal choice of equal groups of *four* coins. On the basis of this factor alone, groups of one, two, three, and four coins should appear with approximately equal frequency. If no other factor were operating, only one-fourth of the Ss could be expected to begin their attack on the problem in terms of *equal groups of four* coins. Thus other factors must enter to favor so strongly one of the alternatives.

(2) *Totality*. The factor of totality appears to have two almost independent sources. One root lies in the overall, undifferentiated impression of the task which the S gains on first encountering it, in the overwhelmingness of 'so many coins to be determined in so few motions.' This means that a maximal number of coins should be handled in each motion—the maximal number at the first step being all eight coins. Implicit in this reasoning is the second source of the totality factor—the assumption that in order to determine the weight of a given coin it must be put on the balance. The possibility of the simultaneous *logical* elimination is not seen at this stage.

(3) *Divisibility*. In the 8-coin problem the two factors of symmetry and totality in combination could probably be considered sufficient reason for the initial four-four division. There appears, however, to exist a third factor which, if operating at all, could not be partialled out in the present problem since it would direct the initial attack in the same direction, away from the correct solution and towards a four-four division. This factor arises out of the arithmetical properties of the number eight, its *divisibility*. There is something 'fourish' about the number eight, which



might be called its arithmetical physiognomy. It is not unlikely that this factor did play a rôle in the universal four-four division.

It is impossible to evaluate the relative contributions of these three factors in the problem under discussion since all three work towards the identical, if mistaken initial step. This was the reason for the introduction of the 9-coin and 25-coin problems which, though identical in their logical-mathematical structure, contain numerical constellations in which the three factors can be unmasked by pitting them against each other. The 25-coin problem proved to be so difficult in pilot experiments that it was always

TABLE I  
INITIAL STEPS IN THE ATTACK ON THE THREE PROBLEMS AS DETERMINED BY THE  
THREE FACTORS AND THE FINAL CORRECT SOLUTION

Problem	Symmetry alone	Totality alone	Symmetry-Totality compromise	Divisibility	Correct solution			
8-coin $(N/2)^2=16$	Combinations	1 vs. 1 2 vs. 2 3 vs. 3 4 vs. 4	1 vs. 7 2 vs. 6 3 vs. 5 4 vs. 4	4 vs. 4	4 vs. 4	3 vs. 3		
	Number of combinations P	4 1:4	4 1:4	1 1:16	1 1:16	1 1:16		
	Combinations	1 vs. 1 2 vs. 2 3 vs. 3 4 vs. 4	1 vs. 8 2 vs. 7 3 vs. 6 4 vs. 5	4 vs. 4 5 vs. 4	3 vs. 3	3 vs. 3		
	Number of combinations P	4 1:5	4 1:5	2 1:10	1 1:20	1 1:20		
9-coin $N^2-1/2^2=20$	Combinations	1 vs. 1 2 vs. 2 3 vs. 3 4 vs. 4	1 vs. 8 2 vs. 7 3 vs. 6 4 vs. 5	4 vs. 4 5 vs. 4	3 vs. 3	3 vs. 3		
	Number of combinations P	4 1:5	4 1:5	2 1:10	1 1:20	1 1:20		
	Combinations	1 vs. 1 2 vs. 2 3 vs. 3 4 vs. 4 5 vs. 5 6 vs. 6	7 vs. 7 8 vs. 8 9 vs. 9 10 vs. 10 11 vs. 11 12 vs. 12	1 vs. 24 2 vs. 23 3 vs. 22 4 vs. 21 5 vs. 20 6 vs. 19	7 vs. 18 8 vs. 17 9 vs. 16 10 vs. 15 11 vs. 14 12 vs. 13	12 vs. 12 OR 12 vs. 13	5 vs. 5 OR 10 vs. 10	8 vs. 8 OR 9 vs. 9
	Number of combinations P	12 1:13	12 1:13	2 1:78	2 1:78	2 1:78		
25-coin $(N^2-1)/2^2=156$	Combinations	1 vs. 1 2 vs. 2 3 vs. 3 4 vs. 4 5 vs. 5 6 vs. 6	7 vs. 7 8 vs. 8 9 vs. 9 10 vs. 10 11 vs. 11 12 vs. 12	1 vs. 24 2 vs. 23 3 vs. 22 4 vs. 21 5 vs. 20 6 vs. 19	7 vs. 18 8 vs. 17 9 vs. 16 10 vs. 15 11 vs. 14 12 vs. 13	12 vs. 12 OR 12 vs. 13	5 vs. 5 OR 10 vs. 10	8 vs. 8 OR 9 vs. 9
	Number of combinations P	12 1:13	12 1:13	2 1:78	2 1:78	2 1:78		

given as the last problem, after both other problems had been solved. Undoubtedly this procedure is not ideal and it will be shown that some valuable information may have been lost due to the effect of whatever learning took place while S solved the earlier problems. At the same time it should be remembered that errors due to this procedure are presumably in the direction of underemphasizing the three factors posited.

Table I gives the initial steps as determined by the three factors individually and in combination, as well as by the final correct solution, and also the probability of occurrence by chance for each category.<sup>6</sup>

As mentioned before, all three factors work in identical direction in the



case of the 8-coin problem and away from the final correct solution. In the 9-coin problem arithmetical divisibility would determine an initial attack identical to that determined by the correct solution, as would one of the four possible cases of bilateral symmetry. Totality works against the correct solution and so does the symmetry-totality compromise which we meet for the first time in this problem. This 'compromise' appears in two forms—either with totality maximized at the expense of symmetry, or with symmetry maximized at the expense of totality. In the 25-coin problem totality, divisibility and the symmetry-totality compromise work against the solution. Only two out of the twelve possible symmetrical openings work here in the direction of the correct solution.

(1) *Eight-coin problem.* Table II below shows the initial steps taken by our Ss in the solution of the 8-coin problem.

TABLE II  
INITIAL ATTEMPTS IN THE SOLUTION OF THE 8-COIN PROBLEM

Groups	4 vs. 4	3 vs. 3	1 vs. 1	N
I (8-coin problem only)	18	2	1	21
Ila (8-coin problem first)	16	2	0	18
Ilb (8-coin problem preceded by 9-coin problem)	11	7	1	19
Total	45	11	2	58

(a) *Symmetry.* Among the 16 possible combinations of the coins only 4 (25%) are symmetrical. Thus, by chance alone only one-fourth of the initial divisions could be expected to be symmetrical. Yet every one of our 58 Ss attacked this problem by dividing the coins symmetricaly, *i.e.* by placing equal numbers of coins on each arm of the balance. In the face of this evidence it would be difficult to deny an extremely powerful tendency towards a symmetrical initial attack on the 8-coin problem.

(b) *Totality.* Since all the Ss started with a symmetrical division, we may consider a chance distribution with respect to totality one in which 1 vs. 1, 2 vs. 2, 3 vs. 3 and 4 vs. 4 openings occur with equal frequency. By chance the 4 vs. 4 opening should be chosen by 25% of the Ss. In fact 45 (78%) of the 58 Ss began with a 4 vs. 4 division of coins, and only 13 (22%) Ss chose one of the other three symmetrical attacks. The difference between chance and obtained distributions is statistically significant well beyond the 1% level of confidence.

The Ss of Group IIb had, however, previously solved the 9-coin problem, *i.e.* they had reached a solution to a similar problem in which totality did not work

\* The calculation of  $P$  is based on the following considerations: All our Ss initially tried to determine how many coins to place on each of the two arms of the balance, *i.e.* by making two groups to be balanced against each other. For a problem involving  $N$  coins either group can vary from 1 to  $N-1$ , the only limitation being that both groups together cannot exceed  $N$ . The number of possibilities so obtained is  $(N/2)^2$  for problems involving even numbers of coins, and  $(N^2-1)/2^2$  for odd numbers of coins.



either. In the case of 15 out of the total 19 Ss in Group IIb, this had meant actually to abandon a totality determined attack which they had attempted initially. When confronted with the present 8-coin problem, these Ss appear to have profited from their previous experience, although not to the extent one might have expected. Table III compares the results of Group IIb with those of Groups I and IIa. It may

TABLE III  
COMPARISON OF TOTALITY AND NON-TOTALITY OPENINGS FOR  
GROUPS I AND IIa VERSUS GROUP IIb

Groups	Totality openings (4 vs. 4)	Non-totality openings (1 vs. 1; 2 vs. 2; 3 vs. 3)	N
I and IIa	34	5	39
IIb	11	8	19
Total	45	13	58

$\chi^2 = 4.72$ ; d.f. = 1;  $P > .02 < .05$ . (Yates correction for continuity has been applied in the computations of  $\chi^2$  throughout this study.)

be assumed that the difference in the two distributions shown here is not due to random variation but represents a real difference in behavior of the Ss in the two groups. The decrease in degree of association as one goes from the Ss who attacked the 8-coin problem first to those who attacked it after having solved the 9-coin problem shows the weakening of the totality factor as a function of experience with the preceding 9-coin problem, but the very magnitude of association in Group IIb indicates that despite this experience the factor of totality is still at work.

(c) *The symmetry-totality compromise and the factor of divisibility cannot be*

TABLE IV  
INITIAL ATTEMPTS IN THE SOLUTION OF THE 9-COIN PROBLEM

Groups	4 vs. 4	4 vs. 5	3 vs. 3	1 vs. 1	8 vs. 1	N
IIa (8-coin problem first)	7	2	8	0	1	18
IIb (9-coin problem first)	8	7	2	2	0	19
Total	15	9	10	2	1	37

isolated in this problem since they determine identical opening moves. Their rôles will be discussed in the following sections on the 9-coin and 25-coin problems.

(2) *Nine-coin problem.* The opening steps taken by the 37 Ss in the solution of the 9-coin problem are indicated in Table IV.

(a) *Symmetry.* Symmetry, in the form of dividing the coins into two equal piles, appears powerfully here, though not as great as in the 8-coin problem. There are 20 possible ways of dividing nine coins into two groups and of these 20 only four are symmetrical groupings: 1-1, 2-2, 3-3, and 4-4. Thus by chance we would expect a symmetrical attack on one-fifth of all cases. In fact, 27 Ss (73%) started with a symmetrical division of coins. The difference between chance and obtained distribution is statistically significant well beyond the 1-% level of confidence.

Yet, as we compare the incidence of symmetrical attacks in the 8-coin and 9-coin problems, we find that they are far from identical. In the 8-coin problem the



tendency towards a symmetrical opening was universal; in the present problem it is far from being so. Table V shows this difference, which is statistically significant at the 1-% level of confidence. This difference seems to demonstrate that a correct

TABLE V  
COMPARISON OF SYMMETRICAL OPENINGS IN THE 8- AND 9-COIN PROBLEMS

Problem	Symmetrical opening	Non-symmetrical opening	N
8-coin	58	0	58
9-coin	27	10	37

$$\chi^2 = 14.7; \text{d.f.} = 1; P < .01.$$

recognition of the balance properties, *i.e.* the fact that only symmetrical groups can be handled by the balance is not the only reason for an initial symmetrical division of the coins. Rather, the easy symmetrical divisibility of the number eight—and for that matter, probably any even number, pushes towards a symmetry opening. Nine coins, on the other hand, are not divisible into two equal groups. Consequently, we find symmetrical openings only where the balance requirements of bilateral symmetry have been recognized. In other words, we are now arguing that the symmetry of the opening moves, if and when not determined by the correct recognition of the balance requirements, are determined by one or both of our other two factors: totality and arithmetical divisibility.

If this interpretation is correct, it might be assumed that previous experience with the 8-coin problem would strengthen the tendency towards symmetrical attack on the 9-coin problem, either because of the mere fact that the initial attack on the 8-coin problem had been symmetrical (as was the case in all Ss), or because a symmetrical attack (though not the same as the initial one) had led to the solution of the problem. Were it for the latter reason, we could then speak of balance properties having been learned and, once learned, would be easily recognized in a second

TABLE VI  
SYMMETRICAL OPENINGS IN GROUPS IIa AND IIb

Groups	Symmetrical opening	Non-symmetrical opening	N
IIa (previous experience with 8-coin problem)	15	3	18
IIb (no previous experience)	12	7	19

$$\chi^2 = 1.07; \text{d.f.} = 1; P = .30.$$

problem of similar type. Table VI shows the results of the two groups with and without previous experience.

The difference, though not statistically significant, is in the expected direction. All that can justifiably be concluded from it is that at least three of the 18 Ss did not learn enough about balance properties in the 8-coin problem to apply it successfully in the 9-coin problem.

(b) *Totality.* Table VII below shows that the incidence of totality determined attacks does not exceed chance expectations in either group, or for both combined. While there appears to be some difference between the two groups, this difference



is not statistically significant. We may only speculate that perhaps previous experience with the 8-coin problem and thus with a non-totality attack leading to the solution might have reduced the preference for totality-determined openings in the Ss of Group IIa.

(c) *Symmetry-totality compromise.* In the discussion of the 8-coin problem we accounted for the overwhelming predominance of 4 vs. 4 openings in terms of at

TABLE VII  
TOTALITY OPENINGS IN THE 9-COIN PROBLEM

Groups	Totality opening (4 vs. 5; 8 vs. 1)		Non-totality opening (4 vs. 4; 3 vs. 3; 1 vs. 1)		N
	Expected	Obtained	Expected	Obtained	
IIa (8-coin problem preceded)	3.6	3	14.4	15	18
IIb (no previous experience)	3.8	7	15.2	12	19
Total	7.4	10	29.6	27	37

least two, possibly three factors. Leaving aside the divisibility factor for the moment, the 4 vs. 4 opening represented the only possible integration of the factors of symmetry and totality. No such optimal integration is possible in the 9-coin problem. There are, however, two possible compromises. Either factor may be predominant, yet manifest itself in a fashion which is strongly co-determined by the secondary factor. Thus when symmetry reigns supreme, it may appear in the form of 4 vs. 4 (rather than 3 vs. 3, 2 vs. 2 or 1 vs. 1), which fulfills as closely as possible the totality requirements. On the other hand, if totality wins out, its appearance in the 5 vs. 4 form would approximate the requirements of a tendency towards symmetrical division better than would, for instance, an 8 vs. 1 division.

Table VIII below shows what the Ss in the two groups did about these two factors, with expectancies as computed from Table I.

TABLE VIII  
THE SYMMETRY-TOTALITY COMPROMISE IN THE 9-COIN PROBLEM

Groups	Frequency	Symmetry-Totality Compromise			Others	N
		4 vs. 4	4 vs. 5	Total		
IIa (8-coin problem preceded)	Expected	.9	.9	1.8	16.2	18
	Obtained	7*	2	9*	9*	
IIb (no previous problem)	Expected	.95	.95	1.9	17.1	19
	Obtained	8*	7*	15*	4*	
Total	Expected	1.85	1.85	3.7	33.3	37
	Obtained	15*	9*	24*	13*	

\*  $P < 0.01$ .

If we begin by looking at the results of Group IIb, we see that in the absence of previous experience the two forms of the compromise appear with about equal frequency and in each case far more often than would be predicted by chance. Thus we might suspect that initially the two factors exert approximately equal influence in determining the opening move. It is interesting to note what happens as a function of previous experience with the 8-coin problem, as demonstrated by the Ss in



Group IIa. Maximizing of symmetry at the expense of totality occurs as often as in Group IIb, *i.e.* as if there had been no previous experience. This is not true, however, of the 4 vs. 5 opening in which totality is maximized. The incidence of this attack in Group IIa has come down to the chance level. In other words, some Ss in this group learned something, though it is not altogether clear what they learned. Had they only learned that 'balance requires a symmetrical division,' *i.e.* to discard the totality tendency, then the four symmetrical divisions (4 vs. 4, 3 vs. 3, 2 vs. 2, 1 vs. 1) should have occurred with equal frequency. This is clearly not the case. In fact, the 3 vs. 3 division is the one which now occurs most often (see Table IV). This point will be discussed again in the next section. We should note, however, that where symmetry predominates over totality, it appears to be far more resistant to change as a function of experience than is the case in the totality-predominant 4 vs. 5 opening.

(d) *Divisibility*. In the 8-coin problem the factor of divisibility was posited as potentially contributing to the initial 4 vs. 4 opening, although it was impossible to isolate it in that problem. In the 9-coin problem, by contrast, it can be isolated from the other two factors. The divisibility-determined opening (3 vs. 3) also happens, however, to be the single opening which leads to the correct solution. In other words, the mere occurrence of this opening move does not necessarily indicate the presence of a divisibility factor. Table IX below shows the incidence of 3 vs. 3 openings as compared to chance expectancy for this step, as computed from Table I.

TABLE IX  
FREQUENCY OF 3 VS. 3 OPENINGS IN THE 9-COIN PROBLEM

Group	Frequency	3 vs. 3	all others	N
IIa (8-coin problem preceded)	Expected	.9	17.1	18
	Obtained	8*	10*	
IIb (no previous experience)	Expected	.95	18.05	19
	Obtained	2	17	
Total	Expected	1.85	35.15	37
	Obtained	10*	27*	

\*  $P < 0.01$ .

In the Ss without previous experience (Group IIb) 3 vs. 3 openings do not occur significantly more often than would be expected by chance. In Group IIa, on the other hand, the incidence of 3 vs. 3 openings is far above chance—so much so, that when both groups are combined, the total of 3 vs. 3 openings still exceeds the theoretical expectancy at the 1-% level of confidence.

The difference between the two groups seems to negate the hypothetical factor of divisibility. It appears that the occurrence of the 3 vs. 3 openings in Group IIa is a function of the specific past experience with the 8-coin problem which the Ss in this group solved before being presented with the 9-coin problem. Six of the eight Ss in Group IIa who began with a 3 vs. 3 opening gave immediately the correct solution. This was true also of one of the two Ss in Group IIb who started in this fashion. In other words, if we exclude solution determined 3 vs. 3 openings, the remainder does not exceed the chance level.

Since the factor of divisibility works in this problem towards the correct solution, it might be suspected that if operating it would manifest itself in faster solutions for



the 9-coin problem than for the 8-coin problem. A comparison of the mean solution times and their standard deviations does not, however, reveal any significant difference between the two problems, neither when given as first nor as the second problem in the series. We must therefore conclude that we have not been able to discover any evidence for a factor of divisibility determining the initial attack or the solution time for the 9-coin problem.

(3) *Twenty-five coin problem.* Table X shows the distribution of the initial steps of the 37 Ss presented with this problem after they had solved both the 8- and the 9-coin problems.

TABLE X  
INITIAL STEPS IN THE SOLUTION OF THE 25-COIN PROBLEM

Group	Pairs of								N
	12	11	10	9	8	6	5	4	
Ila	3	1	1	2	7	1	3	0	18
IIb	2	3	0	1	7	2	3	1	19
Total	5	4	1	3	14	3	6	1	37

As mentioned before, it appears that the previous experience of these two problems rather masks the effect of the three factors.

(a) *Symmetry.* This is now firmly established. Not a single S begins with a non-symmetrical attack on the problem. From Table I we would expect 1:13 or 7.7% of the openings to be symmetrical by chance. Instead, we obtain 100% symmetrical openings. In other words, after two preceding problems all Ss have learned about the bilateral symmetry requirements of the problem.

(b) *Totality.* In its pure and therefore essentially asymmetrical form totality has completely disappeared—presumably due to the symmetry-learning.

(c) *The symmetry-totality compromise.* This is still in evidence with the symmetry

TABLE XI  
INCIDENT OF VARIOUS OPENINGS

Frequency	Symmetry-Totality compromise			Others	N
	12 vs. 12	11 vs. 11	Total		
Expected from Table I	.24	.24	.48	36.52	
Obtained	5	4	9	28	
Expected on the basis of symmetry solutions only	3.1	3.1	6.2	30.8	37

factor predominant over the totality factor. Besides the 12 vs. 12 opening we should probably include also the 11 vs. 11 opening, since our Ss rather typically commented: "I know I can deal with three at the end, therefore I will start by dividing 22, putting 11 on each balance arm." Table XI below shows the incidence of these openings.

Two types of chance expectancies have been indicated in this table. In the upper



row we have listed such expectancies as computed directly from Table I. The obtained values in the second row are all different from these expectancies beyond the 1-% level of confidence. In the third row, by contrast, we have computed expectancies in terms of symmetrical combinations only, since as was mentioned above, all Ss began with a symmetrical attack. None of the differences between obtained values and this second set of expectancies is statistically significant. We must conclude that while at first glance the symmetry-totality compromise appears to be of considerable import it does not withstand closer scrutiny.

(d) *Divisibility*. This factor could be manifested in two ways in this problem: in a 10 vs. 10 opening—which is clearly insignificant, occurring but once in 37 Ss—and in the somewhat more popular 5 vs. 5 opening, which is produced by 6 Ss. As to statistical significance, essentially the same holds as for the symmetry-totality compromise. When matched against Table I expectancies, the increase in this column is significant beyond the 1-% level of confidence. If, however, we compare it to the chance expectancies computed for symmetrical openings only the difference is not statistically significant.

We must thus conclude that the quantitative results do not support the existence of a factor of divisibility or arithmetical physiognomy which we posited at the beginning. These quantitative data, however, do not tell the whole story.

Of the total of 37 Ss, 12 made more than three separate attacks on the problem, and of these 12 only two did not try a 10-10 or 5-5 division. Each of the remaining 10 attempted such an attack not just once, but came back to it repeatedly even though they had apparently discovered at a previous try that it did not work. Still more impressive are the performances of 2 Ss: one who had started with a 12-12 division and worked through the final solution quite efficiently was rather pleased about his own success and commented, "that was tough . . . how did I start, with ten and ten and five?" Whether this indicates that this S had not verbalized his first step, or whether the divisibility factor had been playing havoc with the traces is not clear, but it must have come in somewhere. Still another S, who had also worked through the problem rather efficiently and in a relatively short time, gave the answer and then said, "you know, that eight, eight and nine is not very nice. I would have expected a more elegant solution. I bet I can make it work with ten, ten and five." Whereupon, he settled down for another fifteen minutes and tried to prove that such a solution is possible—which in fact it is not. These observations make it difficult to deny the influence of the arithmetical physiognomy. It is quite likely that the susceptibility to such physiognomy is a matter of individual differences which might account for the non-significant quantitative results. Much further study is needed to clarify this problem.

*The special case of the 8 vs. 8 opening.* As can be seen from Table X, 14 of the 37 Ss began with an 8 vs. 8 opening. This opening is one of two leading to the correct solution, the other one being the 9 vs. 9 opening, which was produced by only 3 Ss. Were this opening a function of a genuine understanding of the problem, gained possibly through experience with the previously solved 8- and 9-coin problems, we should expect two things: First, we should expect approximately equal frequencies for 8 vs. 8 and 9 vs. 9 openings; secondly, we should expect that the Ss starting in either of these two ways would immediately proceed to the correct solution. Table XII below demonstrates the falsity of both these assumptions.



Furthermore, two of the Ss who started with an 8 vs. 8 opening and with delayed solution finally solved the problem in terms of an initial 9 vs. 9 division. Similarly, one of the Ss beginning with a 9 vs. 9 opening and delayed solution solved the problem with a first step of two groups of eight coins.

This demonstrates that an initial 8 vs. 8 opening (as well as 9 vs. 9 opening) is by no means necessarily due to immediate insight into the structure of the problem, although it appears to be due to just that in about half of the Ss beginning in this fashion. What determines this opening in the remaining Ss is not clear from the data. It may be that of all the possible ways of 'cutting down on the coins to be

TABLE XII

APPARENTLY SOLUTION-DETERMINED OPENINGS IN THE 25-COIN PROBLEM

Opening	Immediate solution	Delayed solution	Total
8 vs. 8	8	6	14
9 vs. 9	1	2	3

manipulated' this is selected because it occurred previously and involves fewer coins—or at least one coin less—than the other preceding problem. The fact that this opening can lead to the correct solution is not seen by the S and can thus be considered irrelevant. Further study is needed to clarify this behavior.

## SUMMARY AND CONCLUSIONS

The results of the experiments reported seem to indicate that the three factors posited for the coin problem do play a rôle in the Ss' solution, particularly in the initial attack on the problem. The three factors were called: (a) the factor of symmetry, (b) the factor of totality, and (c) the factor of divisibility or arithmetical physiognomy. The factor of symmetry arises at least in part from the logical constellation of the problem and thus may work in the direction of the solution. We found some evidence, however, that not all symmetrical attacks on the problem are due to a correct appraisal of balance properties. The experience of having solved one or two similar problems just previous to the problem under consideration strengthens the tendency towards symmetrical attack. The factor of totality, by contrast, which works necessarily against the solution in the problems we have dealt with, is reduced as a function of this kind of previous experience. The factor of divisibility could not be demonstrated very convincingly on the basis of the quantitative data, but it appeared as a very strong tendency in a few Ss. It was suggested that there may be greater individual differences of susceptibility with regard to this factor than with regard to the two other factors.

To the present writer it does not appear that these 'psychological properties' of the problems are a function of special motivational needs of the Ss or that they are the result of 'habit' or 'set' in the usual sense of these terms. No specific habit involving the division of coins has previously been estab-



lished, nor has a set been induced explicitly or implicitly within the experimental situation by instructions or example. Yet, these psychological properties of the problems undoubtedly do depend on past experience in the wider sense. The Ss' recognition of the bilateral symmetrical properties of the balance is one instance of this. It would seem also that the factor of arithmetical physiognomic depends crucially on our number system and on the 'overlearning' of some combinations rather than others within that system.

Much further research is needed to establish the range and action of this type of properties of problems. They will, of course, be different for different kinds of problems. Depending on the structure of a problem, other 'factors' may appear, or some of the same factors may play rather different rôles. At the present time we have some evidence already from another problem in which a 'symmetry factor' manifests itself in a way which does not appear to be identical with the one discussed in this paper.



# THE EFFECT OF AN IRRELEVANT RELATION ON DISCRIMINATIVE LEARNING

By CLAUDE B. ELAM and M. E. BITTERMAN, University of Texas

Although Spence's ingenious deduction of transposition from the principle of stimulus-generalization for some years made it unnecessary to postulate a process of relational perception in infra-human organisms,<sup>1</sup> a recent experiment by Saldanha and Bitterman has reopened the question.<sup>2</sup> The general plan of that experiment (Table I) is briefly reviewed.

Work was done with a two-window jumping apparatus and the symbols represent the stimulus-cards which were employed. *N* and *W*, which stand for narrow and wide, represent vertically striped black-and-white cards differing in stripe-width. *L* and *D*, which stand for light and dark, represent homogeneous gray cards differing in reflectance. The symbols for the reinforced cards of the illustrative problems are designated by the subscript *r*. In Problem I, the relational problem, the two

TABLE I

## DESIGN OF THE SALDANHA-BITTERMAN EXPERIMENT

*W* and *N* represent black-and-white vertically striped cards differing in width of stripes. *D* and *L* represent gray cards differing in reflectance. The reinforced cards are designated by the subscript *r*.

Problem I	Problem II
<i>W<sub>r</sub> N</i>	<i>W<sub>r</sub> L</i>
<i>N W<sub>r</sub></i>	<i>L W<sub>r</sub></i>
<i>D<sub>r</sub> L</i>	<i>D<sub>r</sub> N</i>
<i>L D<sub>r</sub></i>	<i>N D<sub>r</sub></i>

striped cards and the two gray cards are paired. In Problem II, the non-relational problem, reinforced stripe is paired with punished gray and punished stripe with reinforced gray. Animals trained by the non-correctional method on Problem I reached criterion and then quickly mastered Problem II. Two-thirds of the animals which began with Problem II did not reach criterion on that problem by the time the entire first group had mastered both problems. Relational presentation facilitated the mastery of these discriminations, and this result suggests the reality of relational perception in the rat.<sup>3</sup>

While the attention of Saldanha and Bitterman was focused on the

\* Accepted for publication March 16, 1952.

<sup>1</sup> K. W. Spence, The differential response in animals to stimuli varying within a single dimension, *Psychol. Rev.*, 44, 1937, 430-444.

<sup>2</sup> E. L. Saldanha and M. E. Bitterman, Relational learning in the rat, this JOURNAL, 64, 1951, 37-53.

<sup>3</sup> Although Spence has recently extended his theory to deal with problems of compounding and transverse patterning (The nature of response in discrimination learning, *Psychol. Rev.*, 59, 1952, 89-93), it is still inadequate to deal with these results. See Bitterman, Spence on the problem of patterning, *Psychol. Rev.*, 60, 1953, 123-126.



manner of presentation of the stimulus-components to be discriminated, the experiments to be reported here were designed for the study of irrelevant components. If the perception of the rat is selective and relational, it should be possible to demonstrate that under certain conditions the relational presentation of *irrelevant* components will *retard* discrimination.

### EXPERIMENT I—PART I

The design of the first part of Experiment I, which is illustrated in Table II, will clarify the problem. All stimulus-cards contained black and white striations. *H* represents thick horizontal stripes; *V*, thick vertical stripes; *h*, thin horizontal stripes; and *v*, thin vertical stripes. In the sample problems illustrated in the table, the animals are trained to respond to thick stripes irrespective of direction (reinforcement indicated by the subscript *r*). On each trial the animals trained on Problem I find an uncomplicated thickness relation—thick and thin horizontal stripes or thick and thin vertical stripes. For the animals on Problem II, however, the thickness rela-

TABLE II

#### DESIGN OF EXPERIMENT I—PART I

*H*, thick horizontal stripes; *h*, thin horizontal stripes; *V*, thick vertical stripes; *v*, thin vertical stripes. The reinforced cards are designated by the subscript *r*.

Problem I	Problem II
<i>H<sub>r</sub> h</i>	<i>H<sub>r</sub> v</i>
<i>h H<sub>r</sub></i>	<i>v H<sub>r</sub></i>
<i>V<sub>r</sub> v</i>	<i>V<sub>r</sub> h</i>
<i>v V<sub>r</sub></i>	<i>h V<sub>r</sub></i>

tion is confounded with a directional relation—that is, thick horizontal is paired with thin vertical, and thick vertical with thin horizontal. In both problems the relevant dimension is thickness and the irrelevant dimension is direction. In Problem I the two members of each pair of cards differ only with respect to the relevant dimension, while in Problem II they differ with respect to both dimensions. The question to be answered is whether the relational presentation of the irrelevant variable will retard learning.

*Subjects.* Twenty-eight male, experimentally naïve Albino rats were studied. Bred in the laboratory from Wistar stock, the animals were 3-4 mo. old at the start of the experiment.

*Apparatus.* A two-window Lashley jumping stand was employed. The entire apparatus was painted flat black.

*Preliminary training.* The animals were adjusted to the apparatus and trained to jump in the usual fashion. After being fed on the feeding platform for several days, they were encouraged to jump gradually increasing distances (to a maximum of 9 in.) first through open windows and then through unfastened gray cards. Manual



guidance was employed to ensure equal experience with both windows. Throughout the experiment the animals were maintained on a 24-hr. feeding schedule. At the conclusion of the preliminary training they were divided randomly into two groups (I and II), each of which contained 14 animals.

*Experimental training.* Group I was trained on Problem I illustrated in Table II. Half the animals were reinforced for responding to the thick stripes (width of each stripe =  $\frac{1}{2}$  in.) irrespective of direction, and half the animals were reinforced for responding to thin stripes (width of each stripe =  $\frac{1}{4}$  in.) irrespective of direction. Group II was trained on Problem II, half the animals being reinforced on thick stripes and the other half on thin stripes. Each animal was given eight trials per day, two to each of the four card-pairs of its problem which were presented in random orders. Training was by the correctional method. On each trial the animal was permitted a maximum of three free jumps, the trial being terminated by a correct response. After three successive errors the animal was manually guided in the correct direction. The criterion of learning was two errorless days, and as each animal mastered its problem it was shifted to the corresponding problem of the other group (same positive and negative cards) and trained to the same criterion.

*Results.* The course of learning for each group on each problem is plotted in Fig. 1. The main curves show performance on the initial problem (Prob-

TABLE III  
RESULTS OF EXPERIMENT I—PART I

	Group I	Group II	Diff.*
Days	36.9	37.5	0.6
Initial errors	91.9	104.9	13.0
Total errors	124.5	134.4	9.9

\* Tested by Wilcoxon's method for unpaired replicates, none of the differences approached statistical significance.

lem I for Group I and Problem II for Group II), while the insert shows performance on the second problem (Problem II for Group I and Problem I for Group II). The curves suggest that the two problems are functionally equivalent—they have the same initial difficulty and there is considerable transfer from each to the other. This impression is confirmed by the mean values presented in Table III. The performance of the two groups on both problems combined, measured in terms of mean days and errors, did not differ significantly by Wilcoxon's test for unpaired replicates.<sup>4</sup> It may be concluded, therefore, that the thickness discrimination was not retarded by the relational presentation of the directional difference.

#### EXPERIMENT I—PART II

The question of the effect of a relationally presented irrelevant variable was pursued further in the second part of the experiment. At the conclusion

<sup>4</sup> Frank Wilcoxon, *Some Rapid Approximate Statistical Procedures*, American Cyanamid Co., Stamford, Conn., 1949, 1-16.



of Part I, the animals had developed a tendency to respond in terms of thickness irrespective of direction. In Part II, the same animals were trained on the directional discrimination, and the effect of a relationally presented difference in thickness was studied. The design of this part of the experiment is illustrated in Table IV. Each card-pair of Problem I A presents an

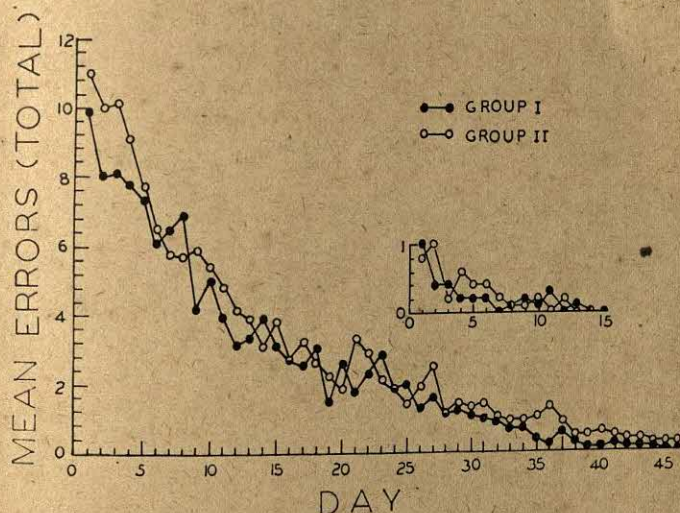


FIG. 1. THE COURSE OF LEARNING IN EXPERIMENT I—PART I

The main curves show performance on the first problem, while performance on the second problem is shown in the inset. Group I learned the problems in the order I-II and Group II in the reverse order.

uncomplicated directional relation, while in each pair of Problem II A the directional relation is confounded with a thickness relation.

TABLE IV

DESIGN OF EXPERIMENT I—PART II

$H$ , thick horizontal stripes;  $h$ , thin horizontal stripes;  $V$ , thick vertical stripes;  $v$ , thin vertical stripes. The reinforced cards are designated by the subscript  $r$ .

Problem I A

$H_r V$   
 $V H_r$   
 $h_r v$   
 $v h_r$

Problem II A

$H_r v$   
 $v H_r$   
 $h_r V$   
 $V h_r$

*Subjects.* From the animals used in Part I, two new groups (I A and II A) of 10 animals each were selected. The groups were carefully matched both for the order in which Problems I and II had been learned and for rate of learning in Part I.

*Training.* Group I A was trained on Problem I A and Group II A on Problem



II A (Table IV); that is, Group I A learned an uncomplicated directional discrimination, while for Group II A the irrelevant difference in thickness was relationally presented along with the critical directional difference. Half the animals in each group were reinforced for jumping to vertical stripes and the other half for jumping to horizontal stripes. Since initial position preferences had been broken in the course of training in Part I, the animals were trained by the non-correctional method in Part II. Twelve trials per day were given, three to each of the four card-pairs. The criterion of learning was two errorless days, and as each animal mastered

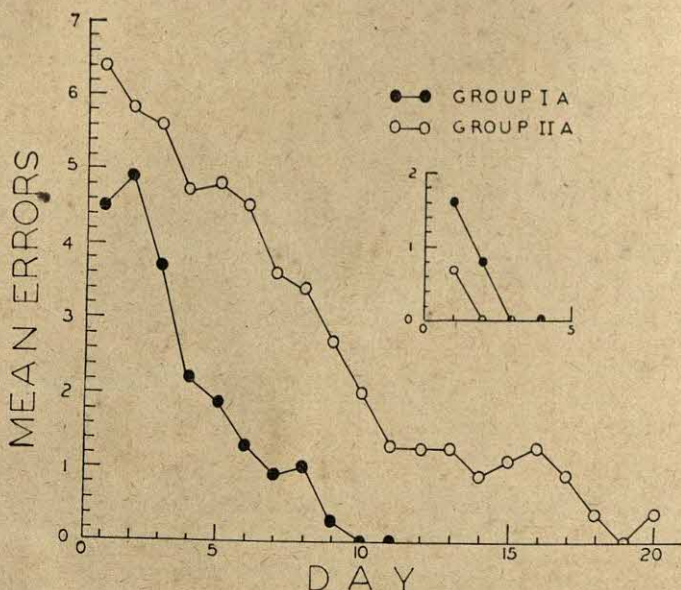


FIG. 2. THE COURSE OF LEARNING IN EXPERIMENT I—PART II.

The main curves show performance on the first problem, while performance on the second problem is shown in the inset. Group I A learned the problems in the order I A-II A, and Group II A in the reverse order.

its problem it was shifted to the corresponding problem of the other group (same positive and negative cards) and trained to the same criterion.

*Results.* In this part of the experiment, a marked difference appeared between the two groups. The course of learning on both problems is plotted in Fig. 2. Group II A progressed less rapidly than did Group I A on the initial problem, although once having mastered the initial problem both groups rapidly reached criterion on the second. In Table V, the performance of both groups on the two problems combined is summarized in terms of mean days and errors. The performance of Group I A was signifi-



cantly superior to that of Group II A in both respects (Wilcoxon's method for paired replicates). These results therefore suggest that, given the set for width established in Part I, the subsequent mastery of a directional discrimination was retarded by the relational presentation of width components.

TABLE V  
RESULTS OF EXPERIMENT I—PART II

	Group I A	Group II A	Diff.	Signif.*
Days	10.9	17.5	6.6	$P < 0.02$
Errors	23.1	53.0	29.9	$P < 0.01$

\* Tested by Wilcoxon's method for paired replicates.

It should be noted that Spence's theory can readily account for the fact that Group I A mastered Problem I A more rapidly than Group II A mastered Problem II A. Consider, for example, the case of an animal reinforced on thick stripes in the first part of the experiment. Since the card-pairs of problem I A (Table IV) present no difference in thickness, solution of that problem requires only enough differential reinforcement to produce a difference between the directional components which are equal in value at the outset of training. In Problem II A, pairs  $H_r v$  and  $v H_r$  should present no difficulty; but the response of the animal to pairs  $b_r V$  and  $V b_r$  should be consistently incorrect, and considerable training should be necessary to overcome the thickness preference. By the same token, however, the animal trained on Problem II A should have little difficulty with Problem I A, while the animal trained on Problem I A should still encounter difficulty with II A. Spence's theory leads to the prediction that the *combined* difficulty of the two problems should be the same irrespective of the order in which they are undertaken. The experimental results do not confirm this deduction and seem, therefore, to require the assumption of selective, relational perception.

## EXPERIMENT II

The fact that the irrelevant relation retarded learning in Part II of the first experiment but did not retard learning in Part I led to the design of a second experiment. Its purpose was to determine whether this difference in results could be attributed to the use of the correctional method of training in the first part and the non-correctional method in the second part. The design of Experiment II was the same as that of Experiment I except that the non-correctional method was used throughout.



*Subjects:* Twenty-four male, experimentally naïve Albino rats, bred in the laboratory from Wistar stock, were studied. The animals were 3-4 mo. old at the beginning of the experiment.

*Apparatus.* The apparatus of Experiment I was again employed.

*Preliminary training.* The preliminary training was similar to that given in Experiment I. In the final stage each animal was trained to reverse its initial position preference in responding to a pair of identical gray cards. On the basis of these tests the animals were divided into two groups roughly equated for flexibility.

*Experimental Training.* In the first part of the experiment all animals learned the thickness discrimination as in Experiment I (Table II). The design of the experiment was the same as before, except that the non-correctional method was used.

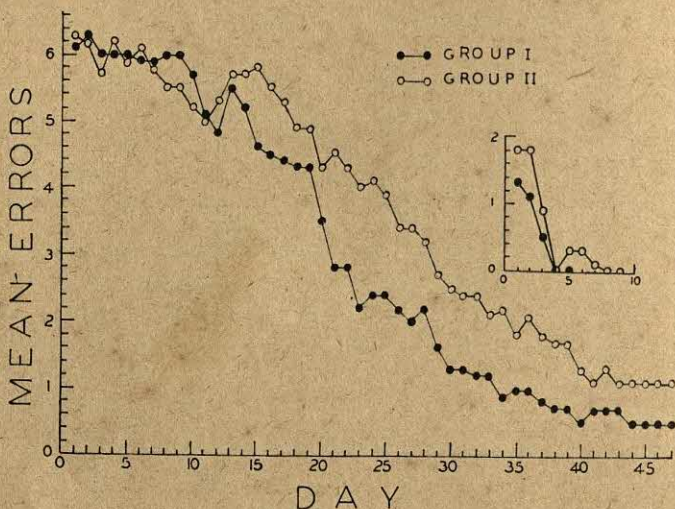


FIG. 3. THE COURSE OF LEARNING IN EXPERIMENT II—PART I

The main curves show performance on the first problem, while performance on the second problem is shown in the inset. Group I learned the problems in the order I-II and Group II in the reverse order.

Each animal was given 12 trials per day, three to each of the four card-pairs. The criterion of learning was two errorless days. As before, when it reached criterion each animal of Group I was shifted to Problem II, and each animal of Group II was shifted to Problem I.

The design of the second part of Experiment II was exactly the same as in Experiment I (Table IV). Two matched groups were trained on the directional problems, one in the order I A-II A and the second in reverse order.

*Results.* The course of learning in Part I is plotted in Fig. 3, and the results are summarized in Table VI. Although there was some tendency in this experiment for Group II to be retarded, differences in days and errors fell short of statistical significance (Wilcoxon's method for unpaired



replicates). In Part II, however, marked differences appeared again. The course of learning is shown in Fig. 4, and the results are summarized in Table VII. On the two problems combined the performance of Group I A was significantly superior in terms of both days and errors (Wilcoxon's method for paired replicates). A comparison of Tables V and VII shows striking agreement in the results of the second portions of each experiment.

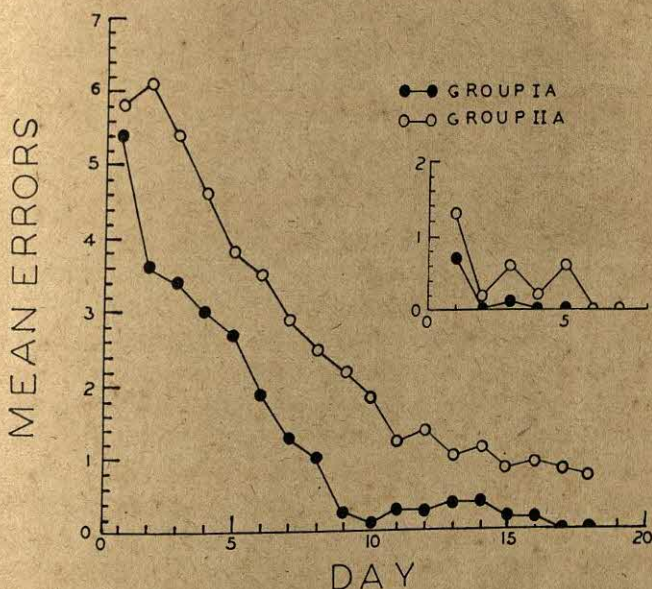


FIG. 4. THE COURSE OF LEARNING IN EXPERIMENT II—PART II

The main curves show performance on the first problem, while performance on the second problem is shown in the inset. Group I A learned the problems in the order I A-II A and Group II A in the reverse order.

TABLE VI  
RESULTS OF EXPERIMENT II—PART I

	Group I	Group II	Diff.*
Days	35.9	39.9	4.0
Errors	144.6	180.4	35.8

\* Tested by Wilcoxon's method for unpaired replicates, both differences fell short of statistical significance.

The two experiments lead, therefore, to the same conclusion. The relational presentation of the irrelevant directional difference did not retard the initial learning of the thickness discrimination. Given the set for thickness, however, the relational presentation of the thickness difference signifi-



cantly retarded the subsequent learning of the directional discrimination. It seems likely that retardation in the first part of such an experiment could be demonstrated by the selection of a more striking irrelevant relationship.

TABLE VII  
RESULTS OF EXPERIMENT II—PART II

	Group I	Group II	Diff.	Signif.*
Days	11.6	16.5	4.9	$P=0.02$
Errors	25.7	48.2	22.5	$P<0.01$

\* Tested by Wilcoxon's method for paired replicates.

In any event, the results obtained in the second part of these experiments provide further evidence for selective and relational perception in the rat.

#### SUMMARY

Two experiments were designed to study the effect of a relationally presented irrelevant variable on the rate of discriminative learning. In the first part of each experiment two groups of rats were trained on black-and-white striped cards differing in thickness and direction (horizontal and vertical), with the thickness components differentially reinforced. In one problem each pair of cards differed only in thickness, while in the second problem each pair differed both in thickness and direction. No significant differences in rate of learning were found. In the second part of each experiment the animals were trained on the directional discrimination. In one problem the two members of each pair of cards differed only in direction, while in the second problem each pair differed both in thickness and direction. In both experiments the irrelevant thickness relation significantly retarded learning. These results are interpreted as evidence for selective and relational perception in the rat.



## THE COLOR OF ULTRAVIOLET LIGHT

By ALBERT BACHEM, College of Medicine, University of Illinois

The visibility of ultraviolet rays has been well established through the partly independent discoveries by Herschel,<sup>1</sup> Matthiessen,<sup>2</sup> Stokes,<sup>3</sup> and Helmholtz.<sup>4</sup> These early discoveries were forgotten, however, and frequent rediscoveries occurred in recent years.

Concerning the color of these rays, Herschel used the term 'lavender-gray.' Stokes stated that the rays lack "the luminousness of the blue and the ruddiness of the violet." Helmholtz pointed out that the spectrum reverses itself in the ultraviolet towards indigo and blue, before it becomes predominantly grayish. He explained this phenomenon through the superposition of two sensations, a violet sensation which weakens towards shorter wave lengths, and a greenish white sensation due to the fluorescence of the retina. Helmholtz observed such a fluorescence experimentally on a human retina prepared 18 hr. after death and exposed to ultraviolet rays. An additional observation, made by Helmholtz but not explained by him, was a color change with a variation of ultraviolet intensity. An intensity increase changed the lavender into a white-blue, a decrease changed it into an indigo-blue. Helmholtz's explanation of the appearance of ultraviolet light was criticized by Fechner,<sup>5</sup> who did not believe in the proper superposition of the focused ultraviolet rays and the somewhat dispersed fluorescent light.

*The color of ultraviolet light.* An explanation of the color of the ultraviolet rays is still missing, although much experimental material has been collected which may be useful for a final decision. Additional material was

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\* Accepted for publication May 6, 1952. From the Department of Physiology.

<sup>1</sup> J. F. W. Herschel, On the chemical action of the rays of the solar spectrum on preparations of silver and other substances, both metallic and non-metallic, and on some photographic processes, *Phil. Trans. Roy. Soc.*, 1840, 1-59, partic. §56. Extension of the visible prismatic spectrum: A new prismatic colour, 19-20.

<sup>2</sup> Matthiessen, Mémoire sur le spectre solaire optique; sur le lentiprisme perfectionné . . . *Compt. Rend.*, 19, 1844, 112.

<sup>3</sup> G. G. Stokes, On the change of refrangibility of light, *Phil. Trans. Roy. Soc.*, 1852, 463-562, Note B, §105, 558 ff.

<sup>4</sup> Hermann von Helmholtz, Über die Empfindlichkeit der menschlichen Netzhaut für die brechbaren Strahlen des Sonnenlichtes, *Pogg. Ann. d. Phys. u. Chem.* 94 IV, 1855, 205-211.

<sup>5</sup> G. Th. Fechner, *Elemente der Psychophysik*, 1859; 3rd Ed. 1907, 247 ff.



obtained through psychological and physical observations reported in this paper.

*Method.* The author remembers from his early spectroscopical work, that overlapping first order red and second order violet rays of a spectrum, obtained by a grating, were visible to the dark-adapted eye. The red (and infrared) rays faded out towards longer wave lengths, preserving their full saturation. The violet (actually ultraviolet) rays lost their saturation progressively towards shorter wave lengths. Thus dark saturated red and silver-like spectral lines alternated in the area of overlapping.

*Observers.* Since the author is now completely blind for ultraviolet rays, experiments were conducted on normal *Os*, about 20 yr. of age, and on aphakial *Os* of advanced age.

The light of a water-colored quartz mercury arc was filtered through a noviol glass, which transmitted only the violet and near ultraviolet rays from 405 to 302  $m\mu$ . The light was observed through a quartz monochromator and an adjustable quartz ocular in the dark room after sufficient dark-adaptation. The light intensity could be reduced through a photographically prepared gray wedge.

*Instructions.* The *Os* were asked to describe their observations in familiar terms, e.g. bright, dull, purple, violet, blue, gray, silver, pure, washed out, faded, etc. Students were expected to use the scientific terms of intensity, saturation, and hue-components. Several students were capable of synthesizing the color by means of a rotating disk with variable sectors of standard colors and gray in dim light immediately after the spectral observation.

*Results.* The reversal of the spectrum was verified by every *O*—but differences occurred concerning the point of reversal. Half of the *Os* described the 405 and 391  $m\mu$ -lines as of the same violet or purple hue. The other half described 391  $m\mu$  as blue or blue-violet in comparison with the violet or purple of 405  $m\mu$ . All rays of shorter wave length were seen in a bluish hue.

The saturation change was noticed by all the *Os*. The aphakial *Os* and a few others considered 391  $m\mu$  as more saturated than 405; most of the *Os* claimed a decline of saturation at 391  $m\mu$ . All the *Os* agreed on the brightness and lack of saturation of 365  $m\mu$ . One aphakial *O* described it as very bright and almost white. At reduced intensity, however, the color became 'quite blue,' or 'more saturated,' in agreement with Helmholtz's observations. Wave length 334  $m\mu$  was described as highly unsaturated blue, bluish gray and silver; 313  $m\mu$  was given as light without color, almost colorless, gray with a trace of blue, and by one aphakial *O* as light blue; 302  $m\mu$  seen by only one aphakial *O*, was as very light blue.

In order to evaluate the rôle of retinal fluorescence, as postulated by Helmholtz, it may be stated first that the retina exhibits two types of fluorescence: the green fading fluorescence of vitamin A; and the non-



fading bluish white fluorescence of cytoplasm. The former occurs only after strong light-adaptation<sup>6</sup> and disappears rapidly under ultraviolet exposure.<sup>7</sup> Since ultraviolet light can be seen only by the dark-adapted eye, this fluorescence cannot play any rôle in ultraviolet vision.

The non-fading, almost white, fluorescence is independent of adaptation and ultraviolet exposure. It occurs not only in the retina, but in all the eye media. The strong fluorescence of the lens is responsible for the peculiar halo-phenomenon upon exposure of the dark-adapted eye to ultraviolet rays. The retinal fluorescence is relatively weak, as evidenced through quantitative measurements on eye media and other organic substances by means of an electronic photometer (Photovolt 514 + E-phototube) under ultraviolet-uviolet excitation, and shown in Table I.

TABLE I  
RELATIVE INTENSITIES OF FLUORESCENCE  
(Equal areas, variable thickness)

Reflected		Av. reflected and transmitted	
Ivory	100	Lens (monkey)	20
Paper	58	Cornea (monkey)	12
Fingernail (human)	20	Retina (monkey)	6
Skin (human)	11	Vitreous (monkey)	2
Sclera (monkey)	26		

The fluorescence appears mainly proportional to the collagen-density (sclera) and the excitation thickness (lens). The retinal fluorescence is relatively weak due to both factors. It is restricted to the cytoplasm of the rod and cone layer, the outer molecular layer and the pigment layer. Due to its high concentration of cones, the fovea exhibits a minimal amount of fluorescence, as first stated by Brücke.<sup>8</sup>

*The fluorescent spectrum.* The spectrum of fluorescent action (the relative power of excitation through ultraviolet rays of different wave lengths) was studied.

*Method.* This was done by means of a water-cooled quartz mercury arc and a Hilger quartz spectrograph. The purified and isolated ultraviolet lines were measured by means of the B-tube, the excited fluorescence was measured through the E-tube of the photometer. The experiments were conducted on the retinas of ox, pig, and Rhesus monkey, and on the lenses of pig, dog, and rabbit. The retinae were carefully cut at the ora serrata and at the optic disk, and were floated, trimmed and,

<sup>6</sup> N. von Jancsó und H. von Jancsó, Fluoreszenzmikroskopische Beobachtung der reversiblen Vitamin-A-Bildung in der Netzhaut während des Schaktes, *Biochem. Zschr.*, 287, 1936, 289-290.

<sup>7</sup> F. R. von Querner, Der mikroskopische Nachweis von Vitamin A im animalen Gewebe, *Klin. Wchnschr.*, 14 II, 1935, 1213-1217.

<sup>8</sup> Ernst Brücke, Über einige Empfindungen im Gebiete des Sehnerven, *Sitzgsber. Math. Naturw. Kl., Kais. Akad. Wiss.*, 77, 1878, III, 39-71, partic. 69 ff.



under physiological saline, attached by means of short pins to a centrally perforated thin plate of hard rubber. The lenses were separated with their capsules and suspensory ligaments intact and attached through the suspensory ligaments. The experimental arrangement and the obtained results are shown in Figs. 1 and 2.

*Results.* The two action spectra upon retinas and lenses are not signifi-

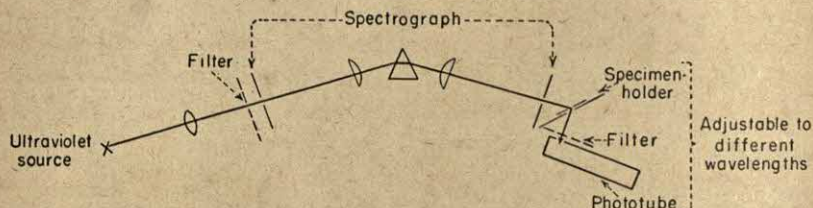


FIG. 1. APPARATUS FOR THE MEASUREMENT OF FLUORESCENCE

cantly different. They have a maximum between 365 and 391  $m\mu$ , with a sharp decline towards longer and a gradual decline toward shorter wave lengths.

The more accurate position of the maximum was determined by means of the carbon arc, which gives a powerful band spectrum below 388.3  $m\mu$ . The maximum appears to occur at 377  $m\mu$ .

*Corollary observations.* Two more subjective, but convincing observa-

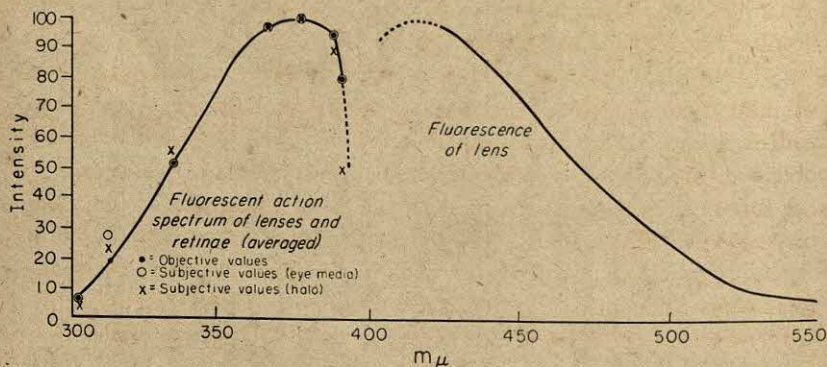


FIG. 2. FLUORESCENT SPECTRUM OF THE MEDIA OF THE EYE AND ITS SPECTRAL EXCITATION

tions by means of an ultraviolet monochromator and a noviol filter, corroborated closely these results.

(1) The eye media (retinas or lenses) were held in front of the exit slit



and the threshold of fluorescence was established for the various wave lengths by the dark adapted eye of an *O*, who was blind for ultraviolet rays. The ultraviolet threshold energy was then measured by another *O*.

(2) The threshold of halo-formation through exposure of the human, ultraviolet-blind, dark adapted eye to the various wave lengths was found, and the threshold energy again measured afterwards.

Variations from the objectively obtained figures are indicated in Fig. 2.

The retinal fluorescence was too weak for an accurate measurement of its spectral distribution. The fluorescence spectrum of rabbit- and sheep-lenses has been accurately measured by LeGrand between 425 and 620  $m\mu$ .<sup>9</sup> It extends practically through the whole visible area with a maximum near the shortest violet rays, as indicated in Fig. 2. This distribution accounts for the white appearance of the fluorescent light with a trace of a bluish hue. There is no doubt that the non-fading fluorescence is the same for all the eye media as far as spectral distribution and bluish white appearance are concerned.

*Discussion.* From the facts presented it seems unlikely that Helmholtz's explanation of the bluish white appearance of ultraviolet light is correct.

(1) The changes of color and saturation from 405 to 391  $m\mu$  are insignificant, although the fluorescence begins below 405 and reaches its maximum close to 391  $m\mu$ .

(2) Changes in color and saturation increase progressively over 365 and 334 to 313 and (in aphakial *O*s) 302  $m\mu$ , while the retinal fluorescence decreases at 365 and 334  $m\mu$  and becomes very small at 313 and 302  $m\mu$ .

(3) Helmholtz's assumption in connection with Fechner's argument would lead to the postulate that sharp violet lines should appear accompanied by bluish white margins. The only indications of such margins were obtained for 405 ('green at the periphery') and 334  $m\mu$  ('greenish edge'); i.e. for wave lengths outside of maximal fluorescence. These observations may be explainable as contrast phenomena.

(4) Helmholtz's assumption in combination with Brücke's statement of weak foveal fluorescence would postulate higher peripheral than foveal sensitivity for ultraviolet light. No evidence of such difference was obtained.

An explanation of the bluishness of the near ultraviolet light through its identification with scotopic vision (which appears bluish- or silver-gray) is also untenable. The line 365  $m\mu$  appeared rather blue at medium intensity

<sup>9</sup> Yves LeGrand, Nouvelles recherches sur la fluorescence du cristallin, *Compt. Rend.*, 225, 1947, 1031-1032.



and almost bright white at high intensity. At extremely low intensity the scotopic silver-gray was obtained occasionally.

The author is inclined to consider the mainly bluish (or indigo-blue) appearance of the spectrum from 500 to 300 m $\mu$ . as the normal (in close analogy to the extended red appearance from 650 to 1000 m $\mu$ .) and to look for an explanation of the violet 'disturbance' around 410 m $\mu$ . Two possible explanations offer themselves.

(a) On the basis of the trichromatic theory of color vision, one may postulate an 'inadequate' stimulation of the long wave red receptors through short wave rays. Most proponents of the trichromatic theory of color vision postulate red, green, and blue as the primary colors and they consider orange, yellow and yellow-green as mixtures of red and green, and blue-green as a mixture of green and blue. The only explanation of violet in that scheme could be its identification with a mixture of blue and red. This explanation requires the assumption that the red cones are sensitive to light rays of wave lengths, not only above 580 m $\mu$  (yellow), but also around 410 m $\mu$ . Several authors (König and Dieterici, recalculated by Ives,<sup>10</sup> Exner and others, quoted by Tschermak<sup>11</sup>) have plotted red-curves, either fading out or presenting a secondary maximum below the maximum of the blue-curve. This theory is supported through the following facts.

(i) Protanopes confuse violet with blue (Pickford).<sup>12</sup> Observing the spectrum, they do not notice a red component in the violet.

(ii) Deuteranopes, who, according to Tscherning and Larsen,<sup>13</sup> lack proper red sensation, have the same difficulty.

(iii) Hering,<sup>14</sup> discussing Von Vintschgau's case of yellow-blue blindness,<sup>15</sup> proved that the spectrum beyond the second neutral area (in the blue) becomes progressively reddish and remains reddish with a low degree of saturation down to the short wave end of visibility. Piper also reported a reddish grey appearance of violet in a case of tritanopia.<sup>16</sup> Levy, however, observed no reddishness at the short wave end, but a darkening of the blue in a case of bilateral tritanopia.<sup>17</sup>

(iv) Willmer stated that 'tetartanopes' see green from 470 to 580 m $\mu$  and red below 470 and above 580 m $\mu$ .<sup>18</sup>

<sup>10</sup> H. E. Ives, The transformation of color mixture equations from one system to another: II. Graphical aids, *J. Franklin Inst.*, 195, 1923, 23-44, partic. 25 and 34.

<sup>11</sup> Armin Tschermak, Theorie des Farbensehens, *Bethe's Handbuch*, 12, I, Chart p. 560.

<sup>12</sup> R. W. Pickford, *Individual Differences in Colour Vision*, 1951, 122.

<sup>13</sup> M. Tscherning and Harald Larsen, Colour-vision and its anomalies, *Acta Ophth.*, 4, 1927, 289-337, partic. 292.

<sup>14</sup> Ewald Hering, Ueber einen Fall von Gelb-Blaublindheit, *Arch. f. d. ges. Physiol.*, 57, 1894, 308-332, partic. VIII, 327-331.

<sup>15</sup> M. von Vintschgau, Physiologische Analyse eines ungewöhnlichen Falles partieller Farbenblindheit, *ibid.*, 48, 1891, 431-528; 57, 1894, 191-307.

<sup>16</sup> Hans Piper, Beobachtungen an einem Fall von totaler Farbenblindheit des Netzhautzentrums im einen und von Violettblindheit des anderen Auges, *Zsch. f. Psychol.*, 38, 1905, 155-188, partic. 163.

<sup>17</sup> Max Levy, Ueber einen Fall von angeborener beiderseitiger Tritanopie, Graefe's *Arch. f. Ophthalm.*, 62, 1905, 464-480, partic. 469.

<sup>18</sup> E. N. Willmer, *Retinal Structure and Colour Vision*, 1946, 173.



(v) Several red-blind and red-green-blind students, who were observed spectroscopically, could not distinguish violet from blue. They described blue and violet as light and dark blue respectively, but they could not set a dividing mark between these two shades of blue. The dark-blue range was narrower than the violet range of normal vision. This hue did not change in the ultraviolet range, except for decreased saturation.

(vi) The narrowing of the violet could be experimentally produced by red-adaptation of one eye and comparison with the other eye as control. An adjustable hair-mark was set by *O* at the centers and borders of the various hues, and direction and amount of the shift were observed. In this experiment the red component of the violet was markedly weakened. In control experiments, conducted on red-blind *O*s, the shift through adaptation and the spectral shortening were missing or negligible, as should be expected.

(vii) A reddening of the violet and its shift into the blue could be experimentally produced through blue-adaptation of one eye and comparison with the other eye as control.

Table II illustrates the observations reported in (v), (vi), and (vii). That the results of exposure to red light are due to retinal adaptation and not to cortical fatigue and contrast is evidenced through the unaltered control observation with

TABLE II  
COLOR SHIFTS THROUGH ADAPTATION AND IN COLOR BLINDNESS

Spectral mark	Red-blind	Normal red-adapted	Red-blind red-adapted	Normal blue-adapted	Red-green- blind
infra red border	↓	↓	—	↑	↓
red center	↑	↑	—	—	↑ difficult
yellow	↑	↑	—	—	↓
green center	—	↑	—	↓	↓ almost blue border
green-blue border	↓	—	↓	↓ narrow	↓ far into violet
blue center	↓	↓	↓	↓	none
blue-violet border	none	↓	none	↑	none
violet center	none	↓ less	none	↑ more	none
violet-ultraviolet border	↑	↑ reddish	↑	↑ reddish	↑

the unexposed eye. The simplest explanation of the observations reported in (i)-(vii) is the assumption that the same red receptors are responsible for both red vision and the red component of violet vision.

(b) One may secondly postulate a violet, purple, or reddish modulator of a rather narrow spectral width superimposed upon a blue modulator of greater spectral extent and upon a photopic and a scotopic dominator. The trichromatic theory of color vision has been strongly criticized for its impossibility of explaining a spectral yellow and the proper saturation of secondary colors. Tschermak in particular considers three primary colors as insufficient and postulates at least four, better five.<sup>19</sup>

<sup>19</sup> Armin Tschermak, *Theorie des Farbensehens*, *Bethe's Handbuch*, 12, I, 550-584, partic. 556, 566, 580.



Edridge-Green proposes six fundamental colors for normal color vision and even seven for the occasionally observed heptachromatic vision.<sup>20</sup> The experimental work of Granit supports the polychromatic theories.<sup>21</sup> So far, Granit has established six modulators in addition to the photopic and scotopic dominators. His last discovery was a violet-blue modulator around 440 m $\mu$ , which may support the alternative theory proposed by the author.

A decision between the two possibilities of explaining the violet discoloration at the short wave end of the spectrum requires a final judgment on the trichromatic or polychromatic nature of color vision. Such an attempt is not planned here in a discussion limited to the color of ultraviolet light.

The assumption of a fundamentally indigo-blue appearance of the short wave part of the spectrum, though disturbed through a red component or a purple-modulator is in agreement with some other facts.

(i) Kohlrausch observed maximal saturation at 450 m $\mu$  in the indigo-blue region of the spectrum.<sup>22</sup> According to Steindler<sup>23</sup> and Houstoun<sup>24</sup> the hue discrimination curve has maxima near 440 and 490 and a minimum near 460 m $\mu$ . All these facts point to indigo-blue as a primary color and to violet and blue as mixtures with other fundamental colors.

(ii) The Brücke-Betzold phenomenon of the shift of violet to blue (and of orange red to yellow) with increased illumination finds its explanation through the limited response of a secondary red maximum or of a narrow violet modulator to the adequate violet stimulus and through the less restricted response of a wider modulator or a broad dominator.<sup>25</sup>

A comparison of the photopic sensitivity of the eye<sup>26</sup> with the light incidence upon the retina<sup>27</sup> and the photosensitivity of visual purple in the ultraviolet spectrum,<sup>28</sup> as illustrated in Fig. 3, postulates the existence of photopic mechanisms, declining sharply over the whole range from 400 to 300 m $\mu$ . The fluorescence with its maximal excitation between 365 and 391 m $\mu$  does not fit quantitatively into this scheme. The assumption of declin-

<sup>20</sup> F. W. Edridge-Green, *The Physiology of Vision*, 1920, 173-175, 263-266.

<sup>21</sup> Ragnar Granit, *Sensory Mechanisms of the Retina*, 1947, 266-343, partic. 314.

<sup>22</sup> Arnt Kohlrausch, Tagessehen, Dämmersehen, Adaptation, *Bethe's Handbuch*, 12, II, 1499-1594, partic. 1558.

<sup>23</sup> Olga Steindler, Die Farbenempfindlichkeit des normalen und farbenblinden Auges, *Sitzgsber. Math. Naturw. Kl., Kais. Akad. Wiss.*, II A, 1906, 39-62, partic. 49, Fig. 2.

<sup>24</sup> R. A. Houstoun, *Vision and Colour Vision*, 1932, 125, Fig. 59.

<sup>25</sup> Ernst Brücke, Über Ergänzungsfarben und Contrastfarben, *Sitzgsber. Math. Naturw. Kl. Kais. Akad. Wiss.*, 51, II, 1865, 461-500, partic. 470 ff.

<sup>26</sup> N. I. Pinegin, Absolute photopic sensitivity of the eye in the ultra-violet and in the visible spectrum, *Nature*, 154, 1944, 770.

<sup>27</sup> V. E. Kinsey, Spectral transmission of the eye to ultraviolet radiations, *Arch. Ophthalmol.*, 39, 1948, 508-513.

<sup>28</sup> C. F. Goodeve, R. J. Lythgoe, and E. E. Schneider, The photosensitivity of visual purple solutions and the scotopic sensitivity of the eye in the ultra-violet, *Proc. Roy. Soc.*, B 130, 1942, 380-395.



ing excitation of blue and violet or red modulators and the photopic dominator may comply with the requirements, however.

Recently, Granit has decided that "receptors lying functionally between the rods and the cones would . . . be mainly sensitive to the shorter wave lengths." He assumes that "the underlying 'blue' modulator-substance differs

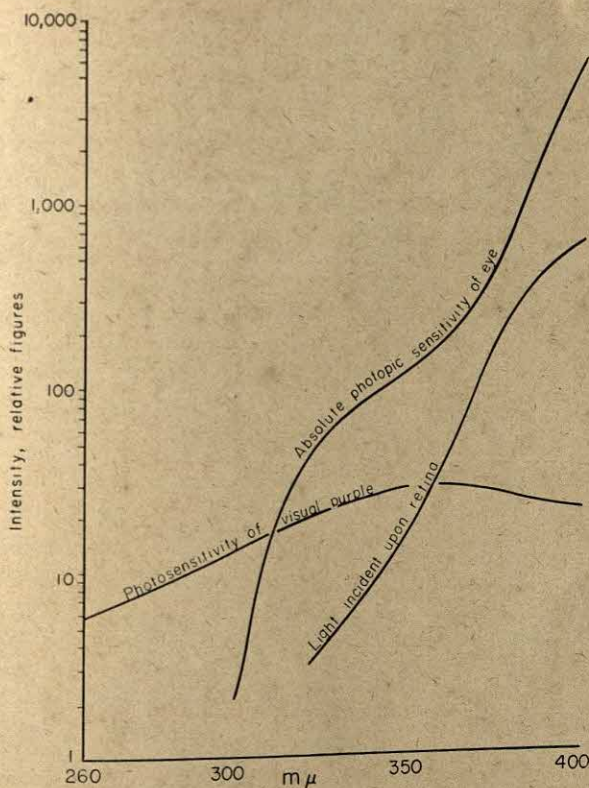


FIG. 3. APPARATUS FOR THE MEASUREMENT OF FLUORESCENCE

less from visual purple than does that which mediates the reception of red."<sup>29</sup> According to this concept, the sensitivity of visual purple, down to 254  $m\mu$ , as established by Goodeve and his collaborators, may be significant for ultraviolet perception.

<sup>29</sup> Ragnar Granit, *op. cit.*, 342-343.



## SUMMARY

(1) Experimental studies on short-wave color vision of normal and aphakial *Os* verify Helmholtz's statements of the reversal of the spectrum and of decreasing saturation towards shorter wave lengths.

(2) Spectroscopic examinations of the fluorescence of the eye-media refute Helmholtz's assumption that the retinal fluorescence is responsible for the 'lavender-gray' of the ultraviolet light.

(3) Experimental and theoretical investigations lead to the assumption that the fundamental color of short-wave light is indigo-blue, but that this hue is discolored into violet either by a secondary maximum of Helmholtz's red curve or, in Granit's terminology, by a red or purple modulator.

(4) The progressive lack of saturation towards shortest wave lengths may be due to the overlapping of the tails of Helmholtz's red, green, and blue curves or Granit's dominators and modulators.



## CRUCIAL EXPERIMENTS IN COCHLEAR MECHANICS

By MAX F. MEYER, Miami, Florida

The term 'vibratory theory' of the cochlea is used here to refer to a large variety of mechanical theories which hold in common that sinusoidal vibrations within the cochlea are the *essential stimulations* of the sensory hair-cells. The 'hydraulic theory' on the other hand,—though not denying the possibility of the existence of faint vibrations within the cochlea, *i.e.* of standing or propagated waves and of faint liquid eddies accompanying waves—regards all those features (which have fascinated several physiologists) as *irrelevant*, as powerless to stimulate the hair-cells in mammalian species.<sup>1</sup> The hydraulic theory is a mechanical theory which identifies *essential* stimulation of the hair-cells with piezo-electric charges received by these cells only through the squeezing jerks which result from the hydraulic (push-pull) functioning of the cochlea.<sup>2</sup> To clarify the distinction between *essential* and *irrelevant* the following analogy is offered. Consider a student of cutaneous sensations. To him, weight, velocity, and temperature of a drop of water are essential for stimulation; but to him it is irrelevant whether the admixture of heavy water to the ordinary water in the drop is, as determined by a physicist, of greater or lesser degree than normal.

*Apparatus.* The fundamental equipment for the experiments described here consisted of three audio-frequency generators of that type in which the resonating circuit is a combination of a capacitance with a resistance (not with an inductance).<sup>3</sup> Each sinusoidal electronic wave was then somewhat amplified and put either into its own plain loud-speaker mounted in a baffle or into its own small loud-speaker mounted on an air resonator. The latter was built in the manner of a stopped organ pipe of extremely wide scale. That is, the length of the pipe (made of cypress wood,  $\frac{3}{4}$  in. thick) was only a trifle greater than the width, so that the resonator could function like the near-spherical resonators made famous by Koenig and Helmholtz a century ago. Of course there was this difference of such a resonator from a real organ pipe. The latter is excited by blowing over the hole. My resonator is excited

\* Accepted for publication May 12, 1952.

<sup>1</sup> M. F. Meyer, The hydraulic theory of the cochlea and comparative anatomy, this JOURNAL, 65, 1952, 288-293.

<sup>2</sup> Meyer, *How We Hear*, 1950, 36-40.

<sup>3</sup> I built these generators out of the material composing Heathkit Model AG-7, which may be obtained from the Heath Company, Benton Harbor, Michigan. I am indebted to Professor Frank B. Lucas and Professor J. M. Eubanks for adding verniers to these generators, which increases the accuracy of tuning, and for eliminating the 60~ and 120~ hums of the amplifiers.



by the small loud-speaker which serves as stop to the pipe; the hole may be varied by a cover to obtain maximal resonance.

### EXPERIMENT I

The first experiment here described is virtually a repetition of an experiment made long ago. It is described by Stumpf as follows: "The reed-tone 100~ combined with the fork-tone 205~ behaved extraordinarily, to our bewilderment. After filtering out the reed's second harmonic with interference tubing (expecting to destroy the beats of this higher tone) there still were the beats; and they were now heard at *double* the rate previously heard, which seemed the only possible rate."<sup>4</sup> This discovery has remained totally forgotten in the psychoacoustical literature. Was it disregarded because it simply does not fit into the theory of alleged vibrations acting as stimuli in the cochlea, or because it is not known?

*Procedure.* To imitate the reed-tone used by Stumpf in his experiment of 1895 (at which I was present) I now combined two of my audio-generators to form the sum  $(\sin x + \sin 2x)$  with great accuracy. To this sum was added from a third generator a mistuned tone  $\sin(2x + d)$ . The mistuning  $d$  was chosen to facilitate the pitch-recognition of rather slow beats. The absolute pitch of the tonal group was made a Minor Seventh higher, 180~ being substituted for 100~ used by Stumpf. The loudness was chosen to be about that of string instruments playing mezzo forte. While now 180~ and  $(360 + d)$ ~ were left sounding undisturbed, the tone 360~ was turned on and off *ad libitum* by simply turning the volume control knob of the generator between a very high and a very low loudness, without any changes in the electronic conditions of either its generator or its amplifier.

*Result.* Whenever the knob was turned to lowest volume, the high pitched beats were noticeably less conspicuous but unmistakably double; at the strong volume position they were more impressive but single, meaning by single the very rate at which they were heard when 360~ and  $(360 + d)$ ~ sounded together during the extinction of 180~. Stumpf's discovery was thus verified also for this higher pitch region.

*Theoretical explanation.* With due credit to Stumpf, the theoretical explanation of the phenomenon reported above consists of two parts. The first part (*Part A*) shows what the theory predicts for combining the sum  $(\sin x + \sin 2x)$  with the mistuned tone  $\sin(2x + d)$ . The second part (*Part B*) shows what the theory predicts for combining the pure tone  $\sin x$  with the mistuned tone  $\sin(2x + d)$ .

*Part A.* A reed-tone  $(\sin x + \sin 2x)$  beats with a fork-tone  $\sin(2x + d)$ . A

<sup>4</sup> Carl Stumpf, Ueber die Ermittlung von Obertönen, *Ann. d. Phys. u. Chem.*, 57, 1896, 670.



slight mistuning,  $d\sim$ , of the higher tone forces the two curves to slip by one another like competing runners on a race track.<sup>5</sup> Such a mistuning forces the two constituents to present themselves to the listener in all possible successive phase relations. From these infinitely many, we have merely to select the characteristic ones to explain the observations of interest to the listener. The following four equations do this for a fundamental bearing a second harmonic (Stumpf's reed-tone) combined with a mistuned pure tone of the higher octave (Stumpf's tone of a fork on a resonance box).

$$\begin{aligned}\text{Phase I,} & y = \sin x + \sin 2x + \sin 2x \\ \text{Phase II,} & y = \sin x + \sin 2x + \sin(2x + 90) \\ \text{Phase III,} & y = \sin x + \sin 2x + \sin(2x + 180) \\ \text{Phase IV,} & y = \sin x + \sin 2x + \sin(2x + 270)\end{aligned}$$

The hydraulic theory requires the computation of all the maxima and minima of each selected phase. I have made the computations with the aid of a two-place trigonometrical table, which is accurate enough for our purpose. The hydraulic

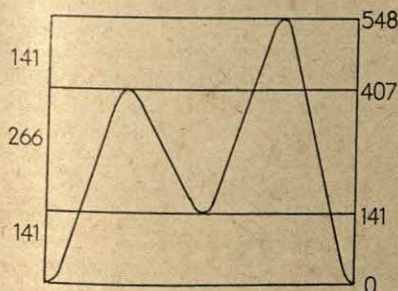


FIG. 1. DATA FOR PHASE I

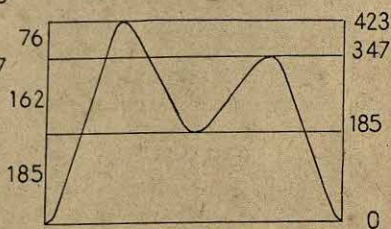


FIG. 2. DATA FOR PHASES II AND IV

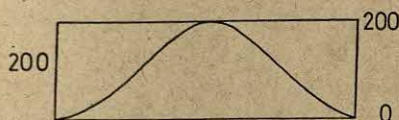


FIG. 3. DATA FOR PHASE III

theory is finally concerned only with the *ordinate differences*, not directly with ordinate values. These numerical data would be more tiresome to read in abstract *tabular* form than when attached to the points of the compound curve in question. I therefore present the numbers here printed at the right ends of horizontal lines which pass through the very maxima and minima of the curves; and the differences in the spaces on the left. Fig. 1 gives these data (without which the whole discussion would leave the reader in the air) for the above equation called Phase I; Fig. 2 is for Phase II; and Fig. 3 for Phase III. I do not give a figure for Phase IV because the ordinate differences happen to come out identical with those of Phase II.

<sup>5</sup> Cf. Meyer, Beats from combining a unit frequency with a mistuned multiple, this JOURNAL, 62, 1949, 424-430. Notice should be taken that pp. 428 and the first half of 429 have been withdrawn in a correction, *idem*, 592-593.





The hydraulic theory next requires that the successive (high-low) positions of the regions of the cochlear phragma be drawn corresponding to Phases I, II, III, and IV. I leave that task to the reader if he wants to see the positions. How it is done is amply described in my book *How We Hear* with numerous examples. The present reader is chiefly interested in the derived loudness results, which I give in Table I.

TABLE I  
LOUDNESS FOR SUCCESSIVE PHASES  
(A reed-tone  $x$  with a fork-tone  $2x$ )

	Phase I	Phase II	Phase III	Phase IV
High pitch	266	162	zero	162
Low pitch	282	261	200	261

The hydraulic theory thus predicts that the high pitch 'beats' very conspicuously, since the loudness is reduced from (relatively) 266 to zero in Phase III; and the total time of the phases of the table indicates the rate of  $d$  beats when the fork tone is mistuned by  $d$ . The lower pitch makes its  $d$  beats less conspicuously; its loudness is varying only between 282 and 200 in Phase III. Stumpf states that he in-

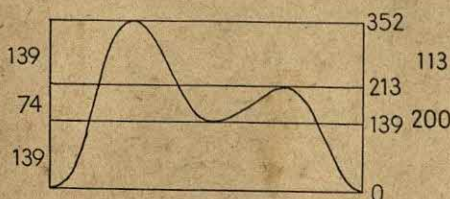


FIG. 4. DATA FOR PHASES I AND III

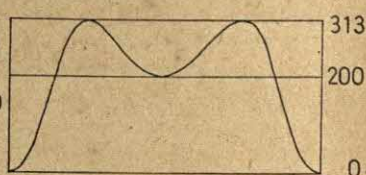


FIG. 5. DATA FOR PHASES II AND IV

entionally disregarded the lower pitch while making his observation. To disregard it is possible for a musical person, but impossible for an unmusical person.

I strongly warn against assigning to an unmusical person the task of performing such an experiment. Table I shows that Phase I and Phase III must in perception differ as an *enormous change* of 'timbre,' as if a shriller drum was struck at Phase I and a duller drum at Phase III. The unmusical person can hardly escape from counting these as 'two drums' instead of a single change of 'timbre.' *Counting thus falsely* would vitiate the whole investigation of the rate problem. (There would be no difference of that false rate from the rate of Part B.) The listener must be a person with the ability clearly to distinguish a change in timbre from a *repetition* of a particular (higher) pitch.

*Part B.* A reed-tone (deprived of the second harmonic) beats with a fork-tone of the octave. The following equations cover the ground for the same four phases as in (A).

$$\begin{aligned}
 \text{Phase I,} & \quad y = \sin x + \sin 2x \\
 \text{Phase II,} & \quad y = \sin x + \sin(2x + 90) \\
 \text{Phase III,} & \quad y = \sin x + \sin(2x + 180) \\
 \text{Phase IV,} & \quad y = \sin x + \sin(2x + 270)
 \end{aligned}$$

The computed ordinate differences are displayed in the same manner as for Part A; in Fig. 4 corresponding to Phase I and in Fig. 5 corresponding to Phase II. By



geometrical logic the data for Phase III are now found to be identical with those of Phase I, and the data for Phase IV identical with those of Phase II, so no Figures are needed for Phase III and Phase IV. Again I save presenting the positions of the cochlear phragma drawn in illustrative figures but give at once in tabular form the loudnesses derived by me.

We learn from Table II that the hydraulic theory predicts beats which are not very conspicuous, since the relative loudness of the high pitch varies only between 74 and 113. Remarkably enough, however, within the full restoration of the original

TABLE II  
LOUDNESS FOR SUCCESSIVE PHASES  
(A pure tone  $x$  with a pure tone  $2x$ )

	Phase I	Phase II	Phase III	Phase IV
High pitch	74	113	74	113
Low pitch	278	200	278	200

phase relations the same variation occurs *twice*. Phases III and IV simply repeat Phases I and II. This means of course the rate of  $2d$  beats of the higher pitch per second when the higher tone is mistuned by  $d\sim$  per sec. In other words, the rate is double that of Part A. Thus the hydraulic theory amply explains the experimental facts.

It is interesting that from phase to phase in Table II the low and the high pitches reach their maximal (and minimal) loudness *alternately*. This fact was mentioned for multiple-frequency beats, with his tuning forks on resonance boxes, by the excellent musical observer and famous acoustic manufacturer Rudolf Koenig.<sup>6</sup> In the literature written by the adherents to the vibratory theory of cochlear mechanics Koenig's observation should have been but *is not* noted as an obstacle to the vibratory theory. The latter predicts an entirely *fortuitous temporal* relation.

*Failure of the vibratory theory.* I remember how distressed Stumpf was when we heard the beats of the higher octave at the rate of  $2d$  although it was mistuned only by  $d\sim$ . He had none other than the vibratory theory at his disposal and the totally unexpected perception of the multiple frequency beats was in conflict with it.<sup>7</sup> Honor, however, is due Stumpf for reporting

<sup>6</sup> *Poggendorff's Annalen*, 157, 1876, 188.

<sup>7</sup> In desperation Stumpf hit upon (and printed in his article) the unproved notion that the box of his fork *might produce* its own second harmonic strong enough to beat with the fourth harmonic of his reed-tone and that their beats at the rate of  $2d$  per sec. might bring about the *illusion* that these were the double beats of the fork's fundamental. As a young student, I did not have the temerity to contradict the authority of my professor and to debate the likelihood of such an *illusion*. In repeating the experiment as described above, I was careful, however, so to arrange my objective conditions that the alleged illusion became extremely improbable. I mounted the loudspeaker giving the (mistuned) higher octave ( $360\sim$ ) on an *air resonator* so built that it *might have* strong resonance for  $180\sim$  and very weak resonance also for 3 times  $180\sim$  and for 5 times  $180\sim$  but no resonance for the (even numbered and by Stumpf suspected) harmonic  $720\sim$ . I have no reason, therefore, to believe that I became a victim of Stumpf's *illusion*; nor do I believe that we were actually victims of such an illusion in 1895.



the facts as he sensed them, not letting prejudice hide the truth.

Multiple-frequency beats continue to this day to be a stumbling block to the partisans of the vibratory cochlear theory. To avoid it they offer the hypothesis that 'in the ear (either inner or middle, preferably the inner) when they experimentally, *exploratively*, add a mistuned physical *multiple* to the unit frequency (like 702 added to 350 in footnote 10), that unit frequency *mysteriously* creates an *aural harmonic of itself*—and the alleged result is a beating. This attempted explanation has two flaws. No aural harmonic has ever been heard by a musical observer; and the beats would not be doubled as in Stumpf's experiment.<sup>8</sup>

The adherents to the vibratory theory (also called 'place' theory) state that the very existence of multiple-frequency beats demonstrates the existence of the (by mere listening unperceivable) *aural harmonic*.<sup>9</sup> They explain the phenomenon of multiple-frequency beats on the ground of an *aural harmonic* which beats through intra-physiological wave interference with the 'exploring' (as they call it) higher tone.<sup>10</sup>

An attempt to 'prove aural harmonics' otherwise than by direct appeal to musical listeners is made by referring to the fact that one may amplify the faint electric potential frequencies which are caused by strongly shaking animal or vegetable tissues. It is a fact that from the head of a cat, or of a cabbage, electric harmonics can be derived, but what justifies identifying such electronically amplified harmonics with pitches actually heard by listeners?<sup>11</sup> The needed musical listeners fail.

<sup>8</sup> S. S. Stevens and Hallowell Davis, *Hearing*, 1938, 184: "It is obviously impracticable to measure the intensity of an aural harmonic simply by listening to it. In fact many times listening does not even reveal the presence." The authors fail to say how many times the presence has been positively revealed by listening.

<sup>9</sup> Stevens and Davis state: "An aural harmonic . . . can be discovered . . . when an auxiliary tone is . . . allowed [sic!] to beat," *ibid.*, 184. On a later page we read: "Our limited experimental evidence indicates that . . . the magnitudes [of aural harmonics] show considerable variation among different observers. . . . Why . . . is not clear," (p. 206). I am confident that this 'why' can be answered: Different people are differently prone to suggestion!

<sup>10</sup> J. P. Egan and R. G. Klumpp (Error due to masking in the measurement of aural harmonics by the method of best beats, *J. Acoust. Soc.*, 22, 1951, 285) state: "There is no doubt that the beats belong to the aural harmonic [pitch]." Neither do I doubt the scientific reality of the beating pitch, but I still doubt the scientific reality of the aural harmonic. The authors avoid a discussion of its aural *creation*, obviously accepting the authoritative statements of Stevens and Davis as unquestionable. On the other hand, the question whether their two beats heard are double or single (in the sense of Stumpf) is never raised by them. Neither do they offer any proof whether their tone 'exploring' the fundamental 350~ was really 702~, as they believe, or 701~, as it naturally must have been.

<sup>11</sup> Stevens and Davis (*op. cit.*, 199-201) report that through shaking living tissues with 700 and 1200~ combined at 90 db. above threshold, no fewer than 66



## EXPERIMENT II

The second experiment made with my three audio-generators consisted in adding to the unit frequently a slightly mistuned triple frequency. He who has discovered the remarkable doubling of the rate of beating might not even think of the possibility that this be *restricted* to combining an odd with an even term. I confess that I was long in thinking of it. When I did, I immediately performed the following experiment.

The simplest case of combining an odd with another odd number is of course that of combining 3 with 1. I produced with great accuracy the sum  $(\sin x + \sin 3x)$ . To this sum was added from a third generator a mistuned tone  $\sin(3x + d)$ . While  $x$  was made approximately 180~,  $d$  was kept in the neighborhood of 2~. Now 540~ and  $(540 + d)~$  were sounded together without 180~;  $d$  beats of course were heard per second. When 540~ was silenced and 180~ was sounding together with  $(540 + d)~$ , the pitch 540~ was again heard beating only  $d$  times per sec. There was no doubling of the multiple frequency beats. Was this due to the fact that an odd number was combined with an odd number, in this case 3 with 1? (The answer to this question is given below.) I observed also that the swellings of the higher and the lower pitches were always heard alternately, just as mentioned in the reference made to Koenig with his forks. No fortuitous temporal relation was ever apparent, as it would be with that ghost of an aural harmonic.

*Theoretical explanation.* The following equations cover the ground for an exact restoration of the original phase relations between 3 and 1.

$$\begin{array}{ll} \text{Phase I,} & y = \sin x + \sin 3x \\ \text{Phase II,} & y = \sin x + \sin(3x + 90) \\ \text{Phase III,} & y = \sin x + \sin(3x + 180) \\ \text{Phase IV,} & y = \sin x + \sin(3x + 270) \end{array}$$

I computed the maxima and minima. The ordinate values and (most important) the resulting ordinate differences are shown in Figs. 6, 7, and 8 at the ends of the auxiliary horizontal lines which pass through the maxima and minima of the compound curve and at the spaces separating these lines. The data for Phase IV are found to be identical with those for Phase II; therefore no Fig. 9 is needed.

As before I omit the figures which would show the successive positions

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"multiples of 100~" are detected by sufficiently high electronic tube amplification, and that they range up to 7600~. That is an interesting observation in biological physics, but is that a 'psychological auditory' experiment? Where is the musical listener testifying for the wonderful concert from the rather dissonant chord of the 66 tones?



of the phragma in these four cases. I give in Table III the data for the loudnesses I derived.

Table III demonstrates that the high pitch steadily shrinks from Phase I to Phase III and then swells again to the new Phase I. There is here neither in *theory* any doubling; nor was there any in the *experimental*

TABLE III  
LOUDNESS FOR SUCCESSIVE PHASES  
(A pure tone with its duodecima)

	Phase I	Phase II	Phase III	Phase IV
High pitch	154	121	110	121
Low pitch	154	255	290	255

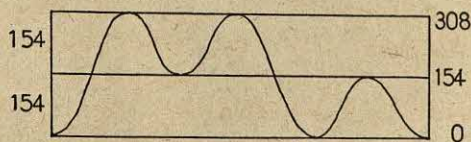


FIG. 6. DATA FOR PHASE I

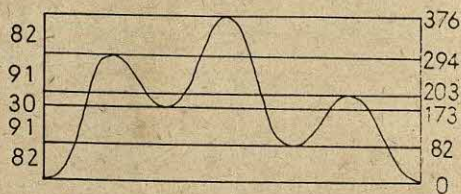


FIG. 7. DATA FOR PHASES II AND IV

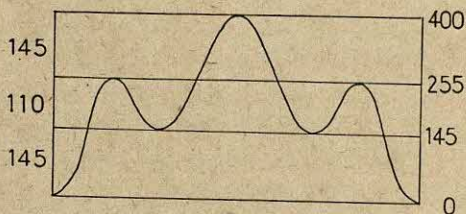


FIG. 8. DATA FOR PHASE III

observation. The low pitch swells and shrinks alternating with the high pitch in theory and experimentally.

The reader may ask: Why should it make a difference whether two odd numbers are combined or an odd number is combined with an even number? The answer is unrelated to anatomy and physiology. It lies in the field



of pure mathematics. It can be summarized in the following two theorems which I abstain from proving because of a lack of space.

*Theorem I: Case of odd with odd.* If the two constituent curves at the *beginning* steer in different directions (one right, one left), in the *middle* of the period they *also* steer in different directions; and if in the *beginning* they steer alike, in the *middle* they *also* steer alike.

*Theorem II: Case of odd with even.* If the two constituent curves at the *beginning* steer in different directions (one right, one left), in the *middle* they *wrangle* and steer alike; and if in the *beginning* they steer alike, in the *middle* they *wrangle* and steer in opposite directions.

The latter has that peculiar significance for the cochlear phragma which the former lacks.



# A COMPARISON OF THE REINFORCING VALUE OF A NUTRITIVE AND A NON-NUTRITIVE SUBSTANCE UNDER CONDITIONS OF SPECIFIC AND GENERAL HUNGER

By JAMES W. CARPER, Johns Hopkins University

The present experiment was designed to compare the reinforcing value for rats of a nutritive and non-nutritive substance under different conditions of hunger. The reinforcing agents used were glucose and saccharin. Glucose is known to reduce hunger whereas saccharin passes through the body unchanged.<sup>1</sup> Both, however, are preferred over water by rats in a free-drinking situation.<sup>2</sup> Furthermore, Sheffield and Roby have shown that saccharin has reinforcing value for learning of a simple T-maze under conditions of general hunger.<sup>3</sup>

Most experiments designed to examine the effect of different conditions of hunger upon the reinforcing value of a stimulus or upon the performance of learned behavior have defined the hunger in terms of the amount of food deprivation. Simple food deprivation gives rise to what we may call a general hunger. There are, however, other techniques of manipulating the hunger-drive. These usually consist of removal of some specific nutritive substance from the diet. This produces a specific hunger. The present experiment compares the effects upon reinforcement and performance of a general hunger for food with that of a specific hunger for calories.

## METHOD

*Subjects.* Eighty male rats of the Lashley-strain, raised in the colony of the Department, served as Ss. Their ages at the beginning of the experiment ranged from 90 to 110 days.

*Apparatus.* The behavior studied in this experiment was the instrumental bar-pressing response. Four Skinner-boxes were used. Each was equipped with an auto-

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<sup>1</sup> J. G. Beebe-Center, Percy Black, A. C. Hoffman, and Marjorie Wade, Relative per diem consumption as a measure of preference in the rat, *J. Comp. Physiol. Psychol.*, 41, 1948, 239-251.

<sup>2</sup> *Op. cit.*, 251.

<sup>3</sup> F. D. Sheffield and T. B. Roby, Reward value of a non-nutritive sweet taste, *ibid.*, 43, 1950, 471-481.



matic liquid reinforcing mechanism. A timing device in the reinforcement circuit permitted an aperiodic reinforcement schedule. The reinforcement circuit was closed half of the time. On the average, 67% of all responses were reinforced.

*Procedure.* A  $2 \times 2 \times 2$  factorial design was used. There were two conditions of general hunger and two conditions of specific caloric hunger. Two reinforcing substances were used. The resulting eight groups may be summarized as follows:

Group	Condition
I	12 hr. hungry, hungry for calories and reinforced with saccharin.
II	0 hr. hungry, hungry for calories and reinforced with saccharin.
III	12 hr. hungry and reinforced with saccharin.
IV	0 hr. hungry and reinforced with saccharin.
V	12 hr. hungry, hungry for calories and reinforced with glucose.
VI	0 hr. hungry, hungry for calories and reinforced with glucose.
VII	12 hr. hungry and reinforced with glucose.
VIII	0 hr. hungry and reinforced with glucose.

(a) *Deprivation.* All animals were on a 12-hr. feeding schedule in which they received 7 gm. of food every 12 hr. Hunger drive was then manipulated in four ways. (1) Groups III and VII were tested at the end of every other 12-hr. period *before* their regular feeding, and are the *12-hr. hungry groups*. (2) Groups IV and VIII were tested on the same schedule but *after* they had eaten their 7-gm. ration. These groups are the *0-hr. hungry groups*. Under both these conditions the groups were fed a fully adequate diet. For the third and fourth conditions the groups were treated just as in the first and second conditions, but they were always kept on a diet inadequate in calories. (3) Thus, Groups I and V were both *12-hr. hungry and hungry for calories*, and (4) Groups II and VI were *0-hr. hungry and hungry for calories*.

The adequate diet used was developed by Maier and Longhurst.<sup>4</sup> The inadequate diet was the same as the adequate diet except that cellulose was substituted for 50% of the cornstarch that provided the caloric value of the diets. Thus, the inadequate diet contained about one-half the calories of the adequate diet.

Body weight served as an index of the specific hunger produced by deprivation of calories. During the preliminary training when animals had free access to the adequate diet, all animals gained weight. After the preliminary training all animals were deprived of food for two days. Then they were put on the feeding schedules described above. The animals were adapted to the schedules for one week before the beginning of the experiment. During this period the animals on adequate diet gained back the weight lost during deprivation while the animals fed the inadequate diet did not.

After conditioning and before extinction half of the animals on adequate diet were switched to the inadequate diet, and half the animals on the inadequate diet were switched to the adequate diet. This switch in diet was accompanied by a switch in weight-levels.

(b) *Training.* All animals were given preliminary training on bar-pressing by being reinforced 250 times with water after 8 hrs. of water deprivation. They were then extinguished to a criterion of 12 or fewer responses per half-hour period for

<sup>4</sup>N. R. F. Maier and J. U. Longhurst, The effect of a lactose-free diet on problem solving behavior in rats, *ibid.*, 43, 1950, 375-388.



two consecutive periods. This preliminary training was administered to reduce variability in experimental conditions. Also, it was thought that the measures furnished by this preliminary training might prove useful in equating experimental groups by the method of covariance. As it turned out, none of the measures showed a significant correlation with the experimental data.

During the actual training trials solutions of chemically pure glucose and saccharin (sodium-o-sulfobenzoic imide) were used as reinforcements. Groups I-IV were reinforced with 0.13% solution of saccharin and Groups V-VIII with a 7.0% solution of glucose. A preliminary test showed that these solutions elicited approximately the same rate of responding in satiated rats.

Animals were put into the Skinner-boxes for one-half hour periods daily. Water bottles similar to those in their living cages were in the boxes during all experimental sessions.

Following training all animals were given half-hour extinction periods for 12 consecutive days. For the first eight days water was substituted for the saccharin and glucose solutions. During the last four days nothing was presented in the reinforcing dipper.

### RESULTS

*Conditioning.* To get the most reliable index of performance during conditioning, the number of responses per half-hour experimental period were averaged for the last two days of training. Table I shows the analysis

TABLE I

THE ANALYSIS OF VARIANCE FOR NUMBER OF RESPONSES PER HALF-HOUR PERIODS (RATE OF RESPONSE) DURING THE LAST TWO DAYS OF CONDITIONING

Source	Variance estimate	df.	F-ratio	Significance
Diets (conditioning)	541.0	1	—	—
0 hr. and 12 hr. hungry	605.0	1	—	—
Saccharin-Glucose	41,648.0	1	18.99	<.001
0 hr. and 12 hr. hungry×Diets	8,905.0	1	4.06	<.05
0 hr. and 12 hr. hungry×Saccharin-Glucose	22,245.0	1	10.44	>.001
Saccharin-Glucose×Diets	62,161.0	1	28.34	<.001
Saccharin-Glucose×Diets×0 hr. hungry	4,410.0	1	—	—
Residual	2,193.0	72		

of variance for these data. The Bartlett test showed that the variances may be considered homogeneous. Fig. 1 shows the mean response for groups of rats trained under the eight experimental conditions (I-VIII) and the significance of differences between these groups.

Table I shows that the only significant main effect is between glucose and saccharin ( $p = <0.001$ ). The three first order interactions are also significant. Since the saccharin and glucose solutions were selected by equating for rate of response for satiated animals, the difference between the saccharin and glucose can be attributed to the interactions.



Fig. 1 shows that the mean rate of response is not significantly different for glucose and saccharin reinforcements when animals are conditioned under complete satiation (Group IV vs. Group VIII). This is also true

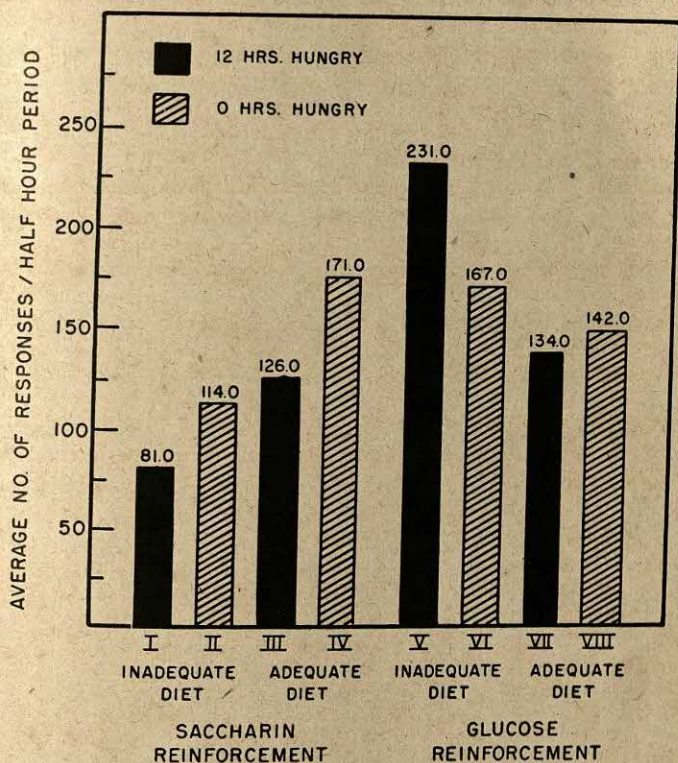


FIG. 1. MEAN NUMBER OF RESPONSES PER HALF-HOUR PERIOD FOR THE LAST TWO DAYS OF CONDITIONING

(For each group  $N = 10$ . Following are the probabilities of the  $t$ -values for the mean differences. Probabilities above the 5% level are omitted.)

Group	II	III	IV	V	VI	VII	VIII
I	<.05	<.02	<.01	<.01	<.01	.01	.02
II		—	<.01	<.01	<.01	—	—
III			.05	<.01	<.05	—	—
IV				>.01	—	—	—
V					<.01	<.01	<.01
VI						—	—
VII							—

for animals fed the adequate diet and conditioned under 12 hr. hunger (Group III vs. Group VII). There is, however, a large difference between glucose and saccharin reinforcement under inadequate diet and both condi-



tions of general hunger (I vs. V, and II vs. VI). Under these conditions the rate of responding for glucose is much higher than for saccharin. These differences account for the interaction and in part for the main effect difference between glucose and saccharin.

Let us now consider the effects of need-state upon rate of response separately for saccharin and glucose reinforcements. For saccharin the rate of responding under 12 hr. hunger is less than that for rats under 0 hr. hunger (I and III vs. II and IV). Also, the rate of responding is less for rats under caloric deprivation than for animals receiving the adequate diet.

In general, these results are just the opposite for glucose reinforcement. With the inadequate diet animals respond more rapidly under 12 hr. than

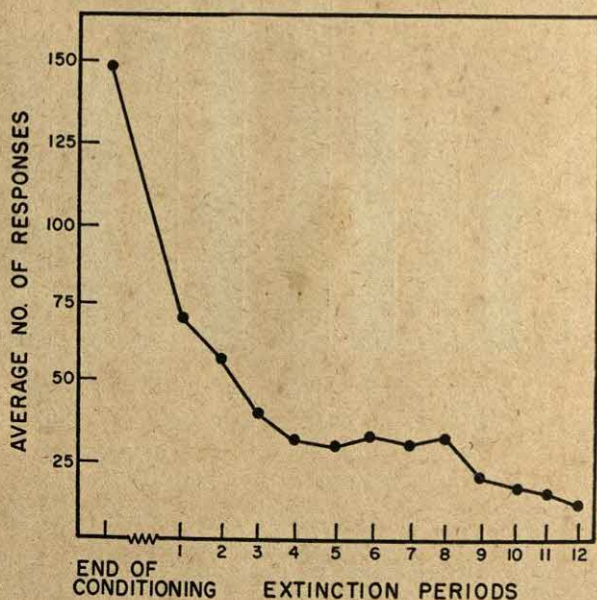


FIG. 2. AVERAGE NUMBER OF RESPONSES PER DAILY HALF-HOUR EXPERIMENTAL PERIOD

Results for all groups combined on the last day of conditioning and on each of the 12 days of extinction. During days 1-8 animals received water in the reinforcing dipper; on days 9-12 they received nothing.

under 0 hr. of deprivation. Under the adequate diet there is no significant difference. If we compare the rates of responding between the adequate and inadequate diets, we find that rate of response is higher for the inadequate diet.



Thus, the rate of responding to glucose behaves in the manner predicted from classical studies of rate of response under different conditions of need,<sup>5</sup> whereas the rate of responding for saccharin follows exactly the opposite course.

*Extinction.* During extinction, half the animals conditioned under the inadequate diet were switched to the adequate, and half the animals condi-

TABLE II

ANALYSIS OF VARIANCE FOR THE AVERAGE NUMBER OF RESPONSES PER HALF-HOUR PERIOD FOR THE TOTAL 12 DAYS OF EXTINCTION

The general hunger variable was eliminated since it showed no significant differences.

Source	Variance estimate	df.	F-ratio	Significance
Conditioning diets	29.0	1	—	—
Extinction diets	1,232.0	1	4.58	<.05
Saccharin-Glucose	58.0	1	—	—
Conditioning diets×Extinction diets	135.0	1	—	—
Saccharin-Glucose×Extinction diets	541.0	1	2.01	>.05
Saccharin-Glucose×Conditioning diets	938.0	1	3.49	>.05
Saccharin-Glucose×Extinction diets ×Conditioning diets	0.0	1	—	—
Residual	269.0	72		

tioned on the adequate diet were switched to the inadequate. The average number of responses per half-hour period for the 12 days of extinction is taken as a measure of resistance to extinction. Fig. 2 shows the average number of responses for each of the 12 days. Except for the slight but abrupt reduction in the number of responses between days 8 and 9, as a result of removing the water from the reinforcing situation, the curve shows negative acceleration typical of the extinction of the bar-pressing response.

Table II shows the analysis of variance for the data on extinction. The Bartlett test indicates that these variances are homogeneous. The only significant main effect difference is diets *during extinction* ( $p = <0.05$ ). The mean number of responses for animals on adequate diet was 35 and for those on inadequate diet 27. These results show that caloric deprivation during extinction reduced resistance to extinction.

The average within groups correlation between rate of response during conditioning and resistance to extinction was 0.49. This correlation indicates that rate of responding during conditioning affects resistance to extinction. In comparing the effect of hunger-reducing glucose on resistance to extinction with saccharin which provides no hunger reduction, it is necessary to remove the variance due to rate of response during condition-

<sup>5</sup> B. F. Skinner, *The Behavior of Organisms*, 1938, 390-405.



ing. For this purpose an analysis of covariance was used. The results showed that the main effect between diets *during extinction* became more significant, while the interactions approaching significance in the analysis of variance were reduced. None of the other variances were effectively changed.

Briefly, the conclusions are that resistance to extinction was not affected by the conditions of need during conditioning or with the need-reducing properties of the reinforcement. Also, the conditions of general hunger *during extinction* did not affect resistance to extinction, whereas calorie deficiency reduced resistance to extinction.

### DISCUSSION

The results of this experiment show that saccharin reinforces the bar-pressing response for both satiated and deprived animals. This is in agreement with the study of Sheffield and Roby which demonstrates that rats learn a T-maze for saccharin reinforcement.<sup>6</sup> In their experiment, however, all animals learned under conditions of general hunger.

For glucose reinforcement, the effect of need on performance is consistent with other studies.<sup>7</sup> The results show that the greater the need, the higher the rate of response. There is one exception. Animals maintained on the adequate diet show no difference in rate of response when 0 hr. and 12 hr. hungry. For saccharin reinforcement, however, the reverse is true. With the introduction of either specific caloric or general hunger, the rate of responding is decreased. Thus, we see that introduction of a hunger need affects saccharin reinforcement differently than glucose reinforcement.

In line with previous findings need *during conditioning* does not affect resistance to extinction.<sup>8</sup> The effect of need *during extinction*, however, is not in agreement with other studies.<sup>9</sup> First, in this experiment the level of general hunger controlled by hours of deprivation does not affect resistance to extinction. Secondly, the animals on the inadequate diet (high drive) show less resistance to extinction than animals on the adequate diet (low drive).

Another point of interest is that there is no difference in resistance to

<sup>6</sup> Sheffield and Roby, *op. cit.*, 471-481.

<sup>7</sup> Skinner, *op. cit.*, 380 ff.

<sup>8</sup> R. C. Strassburger, Resistance to extinction of a conditioned operant response as related to drive level at reinforcement, *J. Exper. Psychol.*, 40, 1950, 473-487.

<sup>9</sup> C. T. Perin, Behavior potentiality as a joint function of the amount of training and the degree of hunger at the time of extinction, *J. Exper. Psychol.*, 30, 1942, 93-113; H. G. Yamaguchi, Drive (D) as a function of hours of hunger (h), *J. Exper. Psychol.*, 42, 1951, 108-117.



extinction between glucose and saccharin reinforcements. If primary need is a basic factor in learning, we should expect glucose which has primary reinforcing value (reduction of hunger need) and secondary qualities (taste) as well, to be a more effective reinforcement than saccharin which has only the secondary reinforcing qualities. This assumption is not borne out by the data on resistance to extinction in this experiment.

In general, the questions raised by this study are: (1) What factors in glucose and saccharin reinforcements account for the difference in reinforcing value under conditions of need? (2) Although specific calorie deprivation increases the rate of responding during conditioning, what factors cause the decrease in resistance to extinction when caloric deprivation is introduced? (3) Does the specific technique of general hunger deprivation account for the lack of difference in resistance to extinction between animals 0 hr. and 12 hr. hungry?



# STIMULUS-CHARACTERISTICS AND RATE OF LEARNING VISUAL DISCRIMINATIONS BY EXPERIMENTALLY NAÏVE MONKEYS

By KAO LIANG CHOW, Orange Park, Florida

The rate that monkeys learn to discriminate visually varies with the characteristics of the stimulus-object. This has been demonstrated by Harlow who found that monkeys, both experimentally naïve and experienced, learn to discriminate objects much faster than patterns, and that there is no significant difference between their rate in learning stereometric and planometric objects.<sup>1</sup> He also demonstrated that monkeys discriminate colors better than patterns.<sup>2</sup> Whether they learn color differences easier than pattern or brightness differences, when the stimulus-objects differ only in one dimension, is not, however, clear. The purpose of the present study was to determine whether monkeys can discriminate between pairs of planometric objects, which differ only in one of the following aspects: color, pattern, brightness, or contour.

*Procedure.* Thirty-one monkeys were trained to discriminate color. Two plaques, one red and the other green and each 8 cm. sq. and 0.5 mm. thick, were placed horizontally over two food-wells. S was required to displace the correct plate to obtain a food-reward. Red was the positive stimulus for 28 of the Ss and green for 3.

For pattern-discrimination, two 8-cm. sq. plaques, one white with a black diamond painted upon it and the other black with white striations, were used as the stimulus-objects. The white plaque with a black diamond was the positive stimulus-object for 28 of the Ss.

\* Accepted March 21, 1952. From the Yerkes Laboratories of Primate Biology.

<sup>1</sup> The terms used to describe the stimulus-variables in discriminative learning have not been generally accepted. For the present report the following definitions are adopted: Stereometric designates three dimensional objects differing in size, color, and form; planometric designates two dimensional objects. Since it is impossible to have an object with only two dimensions, planometric objects refer to any thin plates with the third dimension (thickness) held at a constant minimum. Color is the chromatic aspect of the visual stimulus. Pattern is the figure painted on the stimulus-plate—in the present case, only black and white figures are used. Brightness is the difference between shades of gray. Contour is the form cut out of the metal plate.

<sup>2</sup> H. F. Harlow, Studies in discrimination learning by monkeys: III. Factors influencing the facility of solution of discrimination problems by rhesus monkeys, *J. Gen. Psychol.*, 32, 1945, 213-227; V. Initial performance by experimentally naïve monkeys on stimulus-object and pattern discrimination, *ibid.*, 33, 1945, 3-10; VI. Discriminations between stimuli differentiating in both color and form, only in color, and only in form, *idem*, 225-235.



Twenty of the 31 Ss were trained to discriminate brightness. The stimulus-objects for this part of the study were two 8-cm. sq. gray plaques painted to match 53% and 25% white, respectively, the former being the positive stimulus.

Twelve of the 20 Ss served in a fourth problem: one on contour-discrimination. The stimulus-objects, a disk and a rectangle, were painted the same color (green) and brightness. The disk was the positive stimulus-object for all the Ss. This problem differs from the other three in one marked respect: the *outlines* of the stimulus-objects were the differential cues. The outlines of the stimulus-objects in the other three problems were the same (all being 8-cm. sq. plaques); the differential cues were what had been painted on these plaques: *i.e.* colors; patterns; or shades of gray. It should be noted that these discriminations are confined to differences in one stimulus-dimension; no objects with multiple cues were used. Furthermore, the difference between each pair of stimulus-objects is relatively large, being well above the threshold. The results reported below are based on discriminative learning of large differences in color, pattern, brightness, and contour; they may not, however, be applicable to differences approaching threshold values.

*Subjects.* All the monkeys (*Macaca mulatta*) were experimentally naive. Their weights ranged from 1.8 to 4.0 kg. They were tamed and handled for a period of 11-14 days prior to the formal training. The general procedure, the apparatus, and the specifications of the stimulus-objects used have been described in another report.<sup>3</sup> The animals were given 30 trials per session, and the criterion of learning was 20 successive errorless trials within a 30-trial session. The non-correction method was used.

It has been shown that under certain conditions prior experience with visual discriminations greatly influences the efficiency of later learning.<sup>4</sup> Since my procedures were not primarily designed for the present analysis, both the number and the order of the problems learned by individual animals are not balanced. Because of the small number of discriminations used, the effect of facilitation between problems may not be significant (see below). Of the 31 monkeys, 22 were trained to discriminate colors as their first problem; the remaining 9 as their second problem. Four monkeys learned to discriminate the patterns first; 21 animals, second; and 5 animals, third. Five animals were trained to discriminate differences in brightness first; 3 animals, third; and 12 animals, fourth. All 12 monkeys learned the discrimination of contours as their third problem.

*Results.* Table I gives the averages and standard deviations of the number of trials (excluding the criterion trials) and errors for the monkeys to learn each of the four discriminations. It is immediately apparent that monkeys learned to discriminate contours much faster than the other three problems. The sizes of the standard deviations indicate that individual variations in learning rate are fairly large. The data are first treated as unrelated scores; *t*-values obtained between each pair of mean numbers of

<sup>3</sup> K. L. Chow, Effects of partial extirpations of the posterior association cortex on visually mediated behavior in monkeys, *Comp. Psychol. Monog.*, 20, 1951, 187-217.

<sup>4</sup> Harlow, Primate learning, in *Comparative Psychology*, Ed. by C. P. Stone, 1951, 183-238.



trials and errors are given in Table II. Only the differences between the discrimination of contours and each of the other three problems, both in trial score and error score, are statistically significant beyond the 0.1-% level of confidence. The differences among the other three problems (color,

TABLE I  
AVERAGE NUMBER OF TRIALS AND ERRORS, WITH THEIR SDs,  
REQUIRED BY THE Ss TO REACH CRITERION

	Color (31 Ss)	Pattern (31 Ss)	Brightness (20 Ss)	Contour (12 Ss)
Trials	137±70	154±63	143±71	65±27
Errors	47±29	51±28	44±27	14±8

pattern, and brightness) are not significant, being far below the 5-% level of confidence.

Since all the monkeys learned more than one visual discrimination, the *t*-values of the differences of paired scores of the same animal on any two discriminations are also obtained. The differences between paired scores of the 31 Ss on discriminations of colors and patterns yield a *t*-value of 1.50 for mean number of trials, and 0.80 for mean number of errors (df. 30). For the 20 Ss who learned three problems, the *t*-values are: color *vs.*

TABLE II  
THE *t*-VALUES OBTAINED FROM A COMPARISON OF THE RESULTS OF THE  
DIFFERENT DISCRIMINATIONS

The *t*-values at the upper right of the table are based on trial averages, those at lower left are based on error scores. The figures in parentheses indicate the degree of freedom.

	Color	Pattern	Brightness	Contour
Color	—	1.02 (60)	0.33 (49)	5.52* (41)
Pattern	0.50 (60)	—	0.54 (49)	6.45* (41)
Brightness	0.37 (49)	0.89 (49)	—	4.49* (30)
Contour	5.47* (41)	6.33* (41)	4.42* (30)	—

\* The *t*-value has a *P* smaller than 0.001.

pattern, 1.10 for mean number of trial scores, 0.95 for error score; color *vs.* brightness, 1.46 for trial score, 1.89 for error score; pattern *vs.* brightness, 1.88 for trial score, 1.80 for error score. Although the *t*-values tend to increase, none of them gives a *P* reaching the 5-% level of confidence (19 df.).

The scores of the 12 Ss who were trained on all four problems (all in the following order: color, pattern, contour, brightness) are also treated by



the paired score method. The mean number of trials and errors of these 12 Ss are shown in Table III, and the *t*-values obtained for each pair of the discriminations are given in Table IV. Again, the discrimination of contours is the easiest problem; the differences in both trial score and error score between contour and each of the other three discriminations are

TABLE III

AVERAGE NUMBER OF TRIALS AND ERRORS, WITH THEIR SDs, REQUIRED BY THE 12 Ss TRAINED ON ALL THE PROBLEMS TO REACH CRITERION

	Color	Pattern	Brightness	Contour
Trials	174±77	165±77	139±33	65±27
Errors	62±30	54±33	41±8	14±8

significant beyond the 0.1-% level of confidence. In addition, the *t*-value of the difference between the mean error scores of learning brightnesses and patterns gives a *P* at the 2-% level of confidence.

These results suggest that monkeys may be benefited by earlier experience, but it is not sufficient to change significantly their learning rate of these problems. The first part of this statement is supported by the slightly better score of learning to discriminate brightnesses than colors or patterns; although only 1 out of 10 such comparisons gives a significant *t*-value, *i.e.*

TABLE IV

THE *t*-VALUES OBTAINED FROM A COMPARISON OF THE RESULTS OF THE DIFFERENT DISCRIMINATIONS BY 12 Ss WHEN THE INDIVIDUAL SCORES ARE TREATED AS PAIRED SCORES

The *t*-values at the upper right of the table are based on trial averages, those at lower left are based on error scores. The degree of freedom is 11.

	Color	Pattern	Brightness	Contour
Color	—	0.81	2.16	5.38*
Pattern	1.32	—	1.68	4.92*
Brightness	2.79†	2.08	—	5.35*
Contour	4.91*	4.66*	6.20*	—

\* *t*-value has a *P* smaller than 0.001; † *t*-value has a *P* between 0.02 and 0.01.

the error score between brightness and color based on the data of 12 monkeys. The fact that the Ss learned the contours, the third problem, before the brightnesses, and yet only the former showed a statistically significant, faster learning rate, strongly argues against the view that experience in the first two habits (color and pattern) is responsible for the better performance on contour problem. Further evidences on the lack of facilitation within the limit of the four problems tested is furnished by the insignificant *t*-values of the following comparisons. Comparing the scores of the 22 Ss, who learned the color problem first against the nine Ss who learned



it second, the  $t$ -value between mean number of trials is 0.53, of errors is 0.86. Between the scores of the five Ss who learned the brightness first against the 12 Ss who learned it last, the  $t$ -value for trials is 1.86; for errors is 2.04.

*Summary.* The present data show that with planometric objects, discriminative problems involving a difference in contour are learned much more readily by experimentally naïve monkeys than those in which the cue is a difference in color, pattern, or brightness. The reason for this faster learning is not clear. It may be due to the relatively more prominent figure-ground relationship of the contours produced by different outlines of the objects as compared to that of colors or patterns painted on similarly contoured objects. Among the discriminations of colors, patterns, and brightnesses, at least, for the particular stimuli used, there is probably no significant difference in the rate of learning by monkeys.



## COLOR AS A VARIABLE IN THE JUDGMENT OF SIZE

By WILLIAM BEVAN and WILLIAM F. DUKES, Emory University

Particular aspects of perceptual responses have been experimentally anchored to various points along the stimulus-organism continuum. Though theories have differed in their region of reference (peripheral, central, distal), a major conceptual pattern has been traditional: psychological variables are correlated with their physical or physiological parallel. Theories of spatial perceptions are, for example, framed in terms of objective size or the spatial extent of the retinal or brain process. Within the past two decades, however, another trend in perceptual experimentation has become prominent. Striking relationships have been demonstrated between selected characteristics of perception and variables generally regarded to be of a widely different sort—values, needs, drives. A relatively neglected problem, it seems, concerns the effects on perceptions of variables in between these two extremes—of being parallel to and of being drastically different from the response-dimension under scrutiny. One may, for example, raise the question: Do variables like wave-length or intensity—properties of the physical stimulus other than spatial extent—affect perceived size?

A few observations have been directed to problems of this sort. Helson mentions the anecdotal evidence that a pronounced inequality in perceived size of equal areas of blue and red in the French flag resulted in an official recommendation to change the proportions of the two colors.<sup>1</sup> A few systematic studies conducted under strict conditions of laboratory control also yield data which indicate that wave-length and intensity of light are significant determiners of perceived size.<sup>2</sup> While appreciating the value of both anecdotal report and strict laboratory control, one may wonder if the influence of color on perceived size may be verified under conditions more controlled than those employed in the anecdotal approach but more closely resembling daily experience than those of the usual laboratory situation.

Accordingly, the present writers sought to demonstrate that, in making

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\* Accepted for publication July 2, 1952.

<sup>1</sup> Harry Helson (ed.), *Theoretical Foundations of Psychology*, 1951, 356.

<sup>2</sup> A. H. Holway and E. G. Boring, The dependence of apparent visual size upon illumination, this JOURNAL, 53, 1940, 587-589; Kurt Koffka and M. R. Harrower, Colour and organization, *Psychol. Forsch.*, 15, 1931, 145-275. For the converse problem, effect of area on color, cf. R. W. Burnham, The dependence of color upon area, this JOURNAL, 64, 1951, 521-533.



perceptual estimates under such conditions, observers are noticeably influenced by 'non-parallel' stimulus-properties. The hypothesis tested is that *if colors of otherwise identical objects differ, differences may be found among the ratios of estimated size to actual size for these objects.*

*Subjects and materials.* Sixteen students, volunteers from an introductory course in psychology, served as Ss. The stimulus-objects were 16 different rectangular cards, cut from poster board, one each of four colors in four different sizes ( $11 \times 22$ ,  $14 \times 28$ ,  $17 \times 34$ , and  $20 \times 40$  cm.). The physical characteristics of the various colors are summarized in Table I.<sup>3</sup>

*Experimental design and procedure.* Mounted at distances varying from 18 to 52 m., the 16 cards were so distributed that by the end of the experiment each card had appeared once in every position. Since the primary interest was not in individual

TABLE I

SPECTROPHOTOMETRIC ANALYSIS OF COLORED CARDBOARDS USED AS STIMULUS-OBJECTS

Color	Dominant wave length (hue)	Visual efficiency (brightness)	Purity (saturation)
Red	619 m $\mu$	14%	69%
Yellow	578 m $\mu$	68%	82%
Green	508 m $\mu$	15%	22%
Blue	457 m $\mu$	6%	76%

differences or in positional effects, no attempt was made to separate these, through replication, as sources of variance. Order of presentation was systematically randomized by means of a  $16 \times 16$  Latin square.

The experiment was conducted in bright sunlight in a relatively open space on the University campus. Stimulus-targets were attached at eye-level to poles, buildings, and trees in this area.

To insure a reasonably common and objectified framework for judging, 14 neutral gray reference cards, progressively varying 1 cm. in width and 2 cm. in length, the smallest being  $9 \times 18$  cm., the largest  $22 \times 44$  cm., were spread out on a large laboratory table placed behind S. From this spatial arrangement, randomly varied before each judgment, S was asked to select a card which he regarded to be the same size as a designated stimulus-card. When S had made judgments in this fashion for all 16 cards, he repeated the procedure, each S thus making a total of 32 judgments.

Responses were recorded in terms of an identifying number on the unexposed surface of the reference card. These were then translated into area scores, already computed for the reference series. The mean area-score for each pair of judgments

<sup>3</sup> The posterboard is marketed by the Chicago Cardboard Company as Scarlet Red No. 666, Oriental Yellow No. 660, Green No. 653 and Dark Blue No. 647. The writers are grateful to this company for the physical analysis. Their data were derived from reflectance curves made on a General Electric recording spectrophotometer. Tri-stimulus values were determined by means of the Hardy Method of selecting ordinates, using 10 ordinates (cf. A. C. Hardy, *Handbook of Colorimetry*, 1936, 49-60). The data of Table I represent a conversion of these values obtained from Hardy's chromaticity diagrams. Purity refers to excitation purity, an approximate correlation of saturation; visual efficiency to relative luminosity.



was considered *S*'s estimate for a particular card. To reduce the error in judgments to a common basis, ratios between estimated size and actual size were determined.

The ratios described above were subjected to a triple classificatory analysis of variance which indicated that relative deviations of estimated size from actual size vary with both color ( $F = 3.34$ ,  $df. = 3/225$ ,  $P < .05$ ) and objective size ( $F = 43.64$ ,  $df. = 3/225$ ,  $P < .01$ ), and, furthermore, do so independently (color  $\times$  size  $F = .98$ ,  $df. = 9/225$ ,  $P > .05$ ). In addition, as might be anticipated, the confounded source, *S*s and position, is highly significant ( $F = 10.20$ ,  $df. = 15/225$ ,  $P < .01$ ). Since this might be expected from a knowledge of individual differences, no attempt is made to more precisely identify its nature.

**Results.** Table II reveals that reds and yellows are each estimated differently from either blues or greens, no significant difference being found between red and yellow or between blue and green. Mean ratios for red and yellow represent significant overestimations for size judgments, accu-

TABLE II

MEAN RATIOS OF ESTIMATED SIZE TO ACTUAL SIZE FOR VARIOUS COLORS WITH THEIR *t*-VALUES FOR PERFECT ACCURACY (1.00) AND FOR MEAN DIFFERENCES BETWEEN THE COLORS ( $df. = 15$  in all cases)

Color	Mean ratio	$t_{M-1.00}$	<i>P</i>	<i>t</i> -values above diagonal; their <i>P</i> -values below diagonal			
				R	Y	G	B
Red	1.13	3.00	< .01	—	.60	2.00	2.36
Yellow	1.16	2.82	< .02	> .10	—	3.90	2.43
Green	1.06	1.41	> .10	< .07	< .01	—	.50
Blue	1.07	1.65	> .10	< .05	< .05	> .10	—

racy being defined by a ratio of 1.00. Meanwhile, those for blue and green show no reliable difference between estimated size and object size. Since the intent was not to systematically vary object and contextual properties, but instead to obtain a sample of judgments of well-defined stimulus-objects under other than conventional laboratory conditions, comparison with previous studies could have only limited meaning. No attempt is made, furthermore, to establish these particular colors as *universally* operating in the manner just described. The general principle deemed demonstrated is, rather, that color does influence estimations of size made outside the confines of the university laboratory.

One may first seek to explain the present data in terms of irradiation. If, however, irradiation is considered to be solely a function of stimulus-intensity, then this explanation must be rejected, for inspection of the spectrophotometric analysis of the colors used here (cf. Table I) shows little brightness difference between red and either blue or green, but great difference between red and yellow. Purity, similarly, cannot be accepted as



the crucial variable, for red and yellow differ little from blue, while blue and green differ markedly from each other. Nor can one positively assert, without an extensive sampling of the frequency continuum, that differences in dominant wave-length account for these results. On the other hand, however, the spectral interval between the two exaggerated (red and yellow) and two non-exaggerated colors (blue and green) being greater than that between the pairs of each category, wave-length as the principal determiner of differences may not be rejected.

The differences in size-estimation may be a reflection of the 'advancing' (red and yellow) and 'retreating' (blue and green) quality of the colors. Since under present testing conditions, ample cues are provided for accurate estimation of distance, the difference between 'advancing' and 'retreating' may be reflected as a difference in size. This possibility was evaluated through a re-analysis of the data. By combining the records of all Ss, a composite S who had been presented each stimulus-card in every position was created. A triple classification analysis was then carried out to assess the rôle of positions and the interaction of positions and color as sources of variance. Should the hypothesis just suggested be valid, the color-by-positions interaction should be significant; positions as a main source, insignificant. Analysis revealed this to be the case ( $F = 1.93$ ,  $df. = 45/135$ ,  $P < .01$ ;  $F = 1.07$ ,  $df. = 15/135$ ,  $P > .05$ ). The latter  $F$  indicates, as Gibson has already shown,<sup>4</sup> size-constancy at distances greater than those usually employed in laboratory demonstrations of this phenomenon.

Katz attributes the difference between 'advancing' and 'retreating' colors to *insistence* (Eindringlichkeit) which he suggests is a separate attribute from hue, brightness, or saturation.<sup>5</sup> The present writers favor the possibility that *insistence* is a higher-order variable, resulting from the interactions of the three basic variables. One area of promising research would seem to be the more adequate definition, in terms of stimulus-attributes, of insistence and other phenomenological variables described by Katz.

*Discussion.* Examination of Table III shows that all except the next to largest size were reliably different from 1.00 (perfect accuracy), the largest size being underestimated, the smaller ones overestimated, and that accuracy for each size was, furthermore, significantly different from that for every other size.

The meaning of these results is not entirely clear. The tendency to overestimate the small and to underestimate the large cards may simply be a

<sup>4</sup> J. J. Gibson, *The Perception of the Visual World*, 1950, 183-186.

<sup>5</sup> David Katz, *The World of Color*, 1935, 69-70; 278-286.



function of the framework into which the responses were channeled. When smaller cards were the stimulus-objects the reference series contained more cards larger than they, and vice versa for the larger stimulus-cards. This means that responses had more freedom to vary at the low end for the large cards, at the high end for the small. Consequently, the probability for error of overestimation is greater for the smaller, and the probability of underestimation greater for the larger cards.

These results are reminiscent of certain data from Hollingworth.<sup>6</sup> His findings are explained in terms of 'the law of central tendency,' a principle

TABLE III

MEAN RATIOS FOR VARIOUS SIZED CARDS WITH  $t$ -VALUES FOR DIFFERENCES BETWEEN EACH AND PERFECT ACCURACY (1.00) AND FOR MEAN DIFFERENCES BETWEEN SIZES  
(df. = 15 in all cases)

Size (cm.)	Mean ratio	$t_{M-1.00}$	P	$t$ -values above diagonal; their P-values below diagonal			
				(1)	(2)	(3)	(4)
(1) 11×22	1.33	4.03	<.01	—	4.33	5.12	5.45
(2) 14×28	1.14	2.75	<.02	<.01	—	3.85	4.76
(3) 17×34	1.01	.33	>.10	<.01	<.01	—	3.33
(4) 20×40	.94	3.13	<.01	<.01	<.01	<.01	—

which he maintains describes a general characteristic of serial judgments, independent of particular experimental procedure or sensory physiology. Indifference points are seen to appear as standards around which all judgments are ordered and toward which they tend (cf. the more recent adaptation-level of Helson).<sup>7</sup> Certain factors, however, may displace this indifference point from the center of the stimulus-series. Such displacement is found in the present data and is conceivably associated with a further finding of Hollingworth; namely, a constant error of over-estimation of square magnitudes. (Geometrically, of course, the stimulus-objects used here are not squares, but popularly and phenomenologically rectangles are sometimes 'squares'.)

The present writers are inclined to regard the law of central tendency itself in the probabilistic terms discussed above. One may speculate that in all perceptual judgments, whether the range of possible responses is rigidly prescribed by experimental technique, as in the case of the reference series here, or is established more informally in everyday experience,

<sup>6</sup> H. L. Hollingworth, The central tendency of judgment, *J. Philos.*, 7, 1910, 461-469.

<sup>7</sup> Helson, Adaptation-level as a basis for a quantitative theory of frames of reference, *Psychol. Rev.*, 55, 1948, 297-313.



probability operates in this manner. This speculation is in no way deemed to be contradictory to certain hypotheses concerning the rôle of functional determinants in perception, but rather is considered coördinate with them. Certainly, it warrants more systematic testing. The present observations should be extended by varying the length of the reference series as well as the relative position of the stimulus cards within this series.

#### SUMMARY AND CONCLUSIONS

Sixteen college students in a relatively 'natural' setting selected from a series of 14 neutral gray reference cards those judged equal in area to 16 test cards of various sizes and colors. Statistical evaluation of *ratios* of judged area to actual area reveals reliable errors of overestimation, comparable in magnitude, for the red and yellow cards, no error for blue or green. The influence of a particular stimulus-dimension on a specific 'non-parallel' (in the sense that physical extension is correlated with psychological extensity) aspect of the perceptual response is thus considered demonstrated under conditions approximating those of ordinary experience.

Subsidiary to this, errors of estimation were found to vary with size of stimulus-card. Interpretation was framed in terms of response range defined by the reference series and the position of the various stimulus-objects within this range.



## APPARATUS

### APPARATUS FOR MEASURING THE THRESHOLD FOR VISUAL DISCRIMINATION OF DIRECTION OF MOVEMENT

By J. E. CONKLIN, A. BALDWIN, and R. H. BROWN,  
Naval Research Laboratory

The apparatus described here was designed to measure the threshold for visual discrimination of the direction of movement as a function of exposure-time.<sup>1</sup> Although the apparatus was specifically constructed for the movement of a spot of light in complete darkness, it can be readily adapted to a situation where faint illumination is required.

The equipment incorporates four desirable features: (1) the presentation is noiseless; (2) the movement within a given trial is made at a constant rate; (3) the rate of movement may be accurately varied; and (4) it has an automatic timing-device for controlling the exposure. Fig. 1 shows a block diagram of the essential components of the apparatus.

#### APPARATUS

(1) *Spot presentation.* A spot of green light is projected on a screen by means of a cathode ray tube oscilloscope and lens system. The oscilloscope is a 5-in. Dumont Oscillograph Type 304H, with a DC horizontal axis amplification of 10 to 1. The lens system is a single lens unit (4 diopters) mounted on the face of the cathode ray tube by means of a conical shaped sleeve. This fixed focused position gives a clearly defined spot of light,  $\frac{3}{4}$  in. in diameter, and 0.026 ml. in brightness on a white screen located 105 in. from the lens. The spot of light falls on the screen at eye level, 72 in. from S who is below and in front of the oscilloscope.

(2) *Rate-control.* The rate-control circuit provides for a variation of the rate of spot movement from 0.0 to 0.60 in. per sec. In terms of the visual angle subtended at S's eye, the highest rate utilized is  $0.47^\circ$  per sec. The rate at which the spot travels is a function of the voltage impressed across the horizontal deflection plates of the oscilloscope. This is accomplished by means of a condenser-resistor charging circuit supplied by a 90 v. D.C. source, as shown in Fig. 2. Rate, therefore, is readily calibrated in terms of input voltages to the network, and read by means of an electronic voltmeter. The direction of spot movement is controlled by a right-left switch which changes the polarity of the applied voltage.

<sup>1</sup> The opinions or assertions contained here are the private ones of the writers and are not to be construed as official or reflecting the views of the Navy Department or the naval service at large.



(3) *Exposure-time.* Fig. 3 shows the electro-mechanical layout of the timing device. The equipment is designed to furnish exposure-intervals of  $\frac{1}{2}$ , 1, 2, 4, 8, and 16 sec. A complete cycle can be described as follows. The exposure-time is set with the selector switch and the 'start' button pressed. The screen-light goes off and the spot appears in the center of the screen, moving either to the right or to the left.

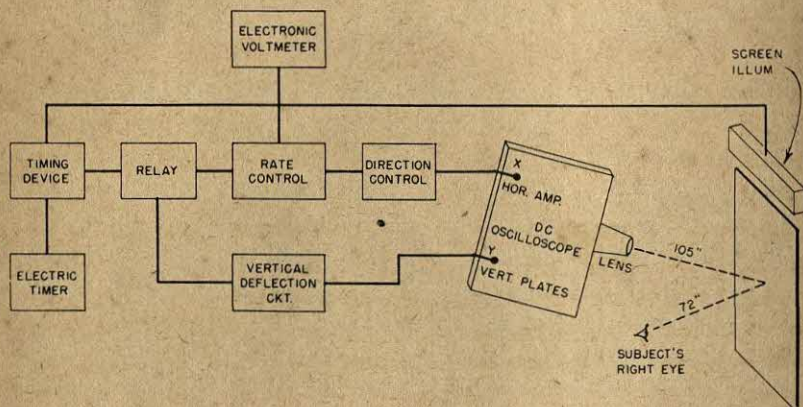


FIG. 1. BLOCK DIAGRAM OF APPARATUS

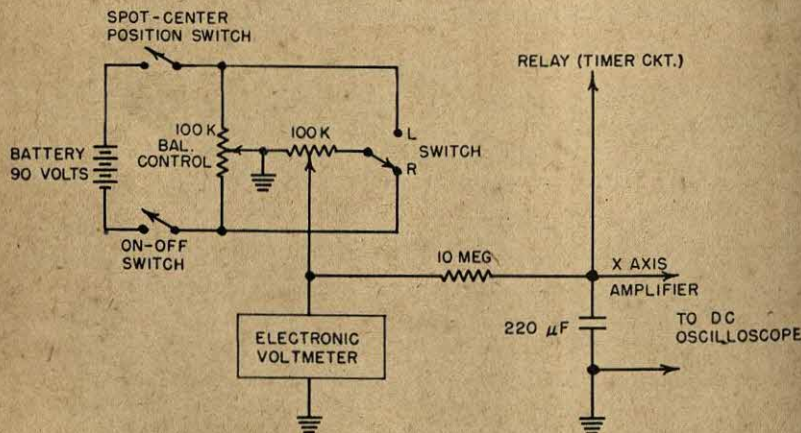


FIG. 2. RATE CIRCUIT DIAGRAM

After the appropriate time-interval, the spot disappears. The screen-light turns on again at the end of the cycle (19 sec.).

When the 'start' button is pressed, it energizes the  $\frac{1}{3}$  rpm., Bodine synchronous motor. The motor turns a cam shaft, revolving several cams and microswitch mountings. Six of the cam-microswitch circuits determine the exposure-intervals, pre-set



by the selector switch. The seventh cam-microswitch circuit keeps the motor running after the 'start' button is pressed, and shuts it off after one complete revolution. This circuit also turns the screen-light off while the motor is running, and turns it on again when the motor stops. The relay, shown in Fig. 3 serves two functions: (1) It applies the rate-voltage to the oscilloscope at the beginning of the cycle and removes it at the termination of the exposure-interval (returning the spot to the horizontal center). (2) It shorts the vertical deflection plates at the beginning of the cycle, and applies a voltage to the plates after the appropriate time-interval.

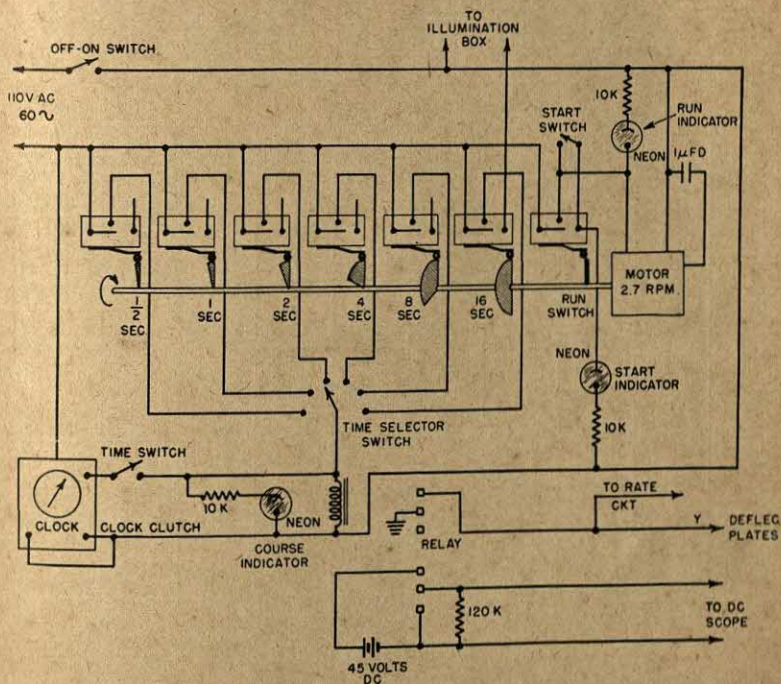


FIG. 3. TIME LAYOUT

(This, instantaneously, deflects the spot vertically causing it to disappear.) S sees the spot appear and move in a right or left direction, and disappear at the end of the exposure-interval. Actually, when the spot disappears, it is reflected upward first, and then returned to horizontal center at the end of each trial. To eliminate any possible cues, the face of the scope was masked above and below the horizontal axis with black tape. By this technique, the spot only moves a fraction of an inch in the vertical axis before disappearing.

#### CALIBRATION

(1) *Rate-control.* Table I shows the results of 144 calibration measures. The distance in inches the spot travels in 16 sec. was determined for each voltage reading.



TABLE I

DISTANCE AND RATE OF MOVEMENT AS A FUNCTION OF THE VOLTAGE  
INPUT TO THE RATE-CONTROL NETWORK  
(Time of exposure 16 sec.)

Volts	Distance		Rate		Volts	Distance		Rate	
	In.	Degrees visual angle	In./sec.	Deg./sec.		In.	Degrees visual angle	In./sec.	Deg./sec.
0	0.000	0.000	0.000	0.000	20	5.394	4.285	0.337	0.268
4	1.063	0.846	0.066	0.053	24	6.456	5.124	0.404	0.320
8	2.213	1.761	0.138	0.110	28	7.510	5.962	0.470	0.373
12	3.318	2.638	0.207	0.165	32	8.562	6.782	0.535	0.424
16	4.318	3.432	0.270	0.215	36	9.563	7.566	0.598	0.473

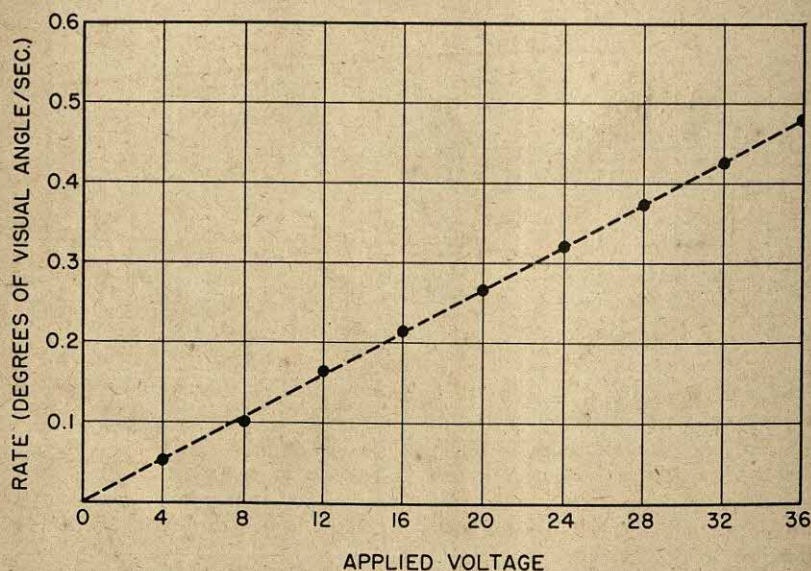


FIG. 4. CALIBRATION CHART

TABLE II

MEAN AND STANDARD DEVIATION OF EXPOSURE-TIMES IN SEC.

Exposure-time	Mean-time	SD	Exposure-time	Mean-time	SD
$\frac{1}{2}$	0.508	0.005	4	3.993	0.006
1	0.984	0.006	8	8.001	0.006
2	1.969	0.007	16	16.000	0.003



The standard deviation for any series of measurements did not exceed 0.1 in. The linear relationship between the applied voltage and rate of spot movement in degrees of visual angle per second is shown graphically in Fig. 4.

Calibrations made over a two-week period indicate that the rates were constant

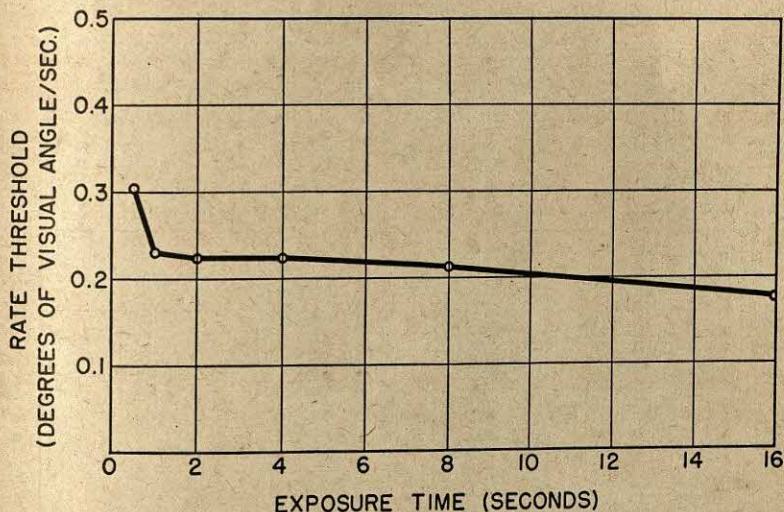


FIG. 5. THRESHOLD AS A FUNCTION OF EXPOSURE-TIME

from day to day with respect to voltage. The rates at which the spot moved were also constant for all exposure-intervals.

(2) *Exposure-time.* The exposure-times were calibrated with a Standard Electric Timer inserted in the time-circuit (see Fig. 3). Table II shows the results of 20 readings for each time-interval to the nearest hundredth of a second. The small variability in the measurements was due to the variability in the action of microswitch and in the Electric Timer itself.

### ILLUSTRATIVE EXPERIMENT

Utilization of this apparatus is illustrated in an experiment in which six members of this laboratory served as Ss.

*Procedure.* Every S, who served individually, was dark adapted in the experimental room for 2.5 min. During this period the following instructions were given him.

As soon as the screen-light comes on, you are to regard steadily the fixation-point on the screen, using the biting-board and chin-rest. After 6 sec. of fixation, the screen-light will go out and a green spot of light will appear. The spot of light will move either to the right or to the left and then disappear, leaving you in total darkness. Report the direction of the movement of the spot of light after it has disappeared. If you are unable to determine the correct direction, guess. Repeat this procedure when the screen-light is again turned on. Are there any questions?



The method of limits was used in the presentation of the various rates of movement. The ABBA order was used for the presentation of the exposure-intervals. Three Ss began with the shortest interval (0.5 sec.) and three with the longest (16 sec.). A total of eight series were presented for every time-interval. Observation was monocular and with S's better eye.

Fig. 5 shows the rate thresholds in degrees of visual angle per second as

TABLE III

## ANALYSIS OF VARIANCE

Source of variation	df	Mean square	F-ratio
Exposure-time (T)	5	0.020326	4.24*
Subjects (S)	5	0.037066	7.73*
Practice (P)	1	0.000159	—
T×S	25	0.005549	1.16†
T×P	5	0.008377	1.75†
S×P	5	0.009637	2.01†
Error (S×T×P)	25	0.004793	

\* Significant at 1% level; † not significant.

a function of exposure-time. The analysis of variance indicates that the only significant variables are individual differences among the Ss and exposure-times. There is no practice effect and no significant double interactions (see Table III).



## A CIRCUIT FOR THE CONTINUOUS MEASUREMENT OF PALMAR RESISTANCE

By NED A. FLANDERS, University of Minnesota

In spite of earlier disagreements concerning the measurement and interpretation of palmar resistance phenomena, more and more studies report data of this type. Contemporary workers use two measurements of resistance as the basis for psychological inferences: (1) gradual changes of resistance over relatively long periods of time, usually referred to as 'resistance-level'; and (2) sudden changes of resistance over short periods of time, usually referred to as 'responses,' 'reflexes,' or 'displacements,' which are associated with controlled stimuli.

Since the measurement of reflexes (Type 2) occur over such short periods of time, the problems associated with their measurement are reduced largely to the single problem of building an instrument which is sensitive enough to record them. For these measurements there is no need for an absolute scale of resistance and the design of such an instrument is relatively simple. Most research utilizes data of this type.

Measurement of resistance-level (Type 1), on the other hand, raises several major problems of instrumentation because the data to be compared are not only collected over relatively long periods of time, but also because it may be desirable to compare the resistance levels of a given *S* for two different experimental sessions. Such comparisons are based on at least the following requirements. First, conditions at the electrodes, where electrolytic reactions are in process, must be constant or nearly constant not only throughout the period of measurement but also from one experimental session to then next. Secondly, the measuring instrument should record on an absolute scale of resistance. Thirdly, from the standpoint of convenience, the record of resistance-level should be on a continuous roll of chart paper. This article is primarily concerned with a method of satisfying these requirements.

*Circuit design.* The first of these requirements is, by far, the most difficult to satisfy. The heart of the problem concerns the electrolytic reactions at the electrodes. No matter how the electrodes are constructed or what metals are used, the flow of current to and from the body involves chemical reactions which are subject to polarization phenomena and the build-up of by-products. These, in turn, influence the apparent or measured resistance-level. The formation of by-products and the presence of polarization phenomena depend on the magnitude of the current and therefore, variations in the flow of current will cause irregular artifacts in apparent



resistance-level. In short, it is desirable to reduce variation in the magnitude of current, a condition of constant current being the most desirable. A few simple calculations will serve to illustrate the problem.

If we arbitrarily assume, as a starting point, conditions of 40  $\mu$  amp. (I) flowing through an  $S$  whose resistance ( $R_s$ ) is 50,000 ohms, we can then calculate the voltage required by using Ohm's law.<sup>1</sup> The table in Fig. 1 illustrates the fact that changes of resistance ( $R_s$ ) by a factor of twenty to one will also produce an

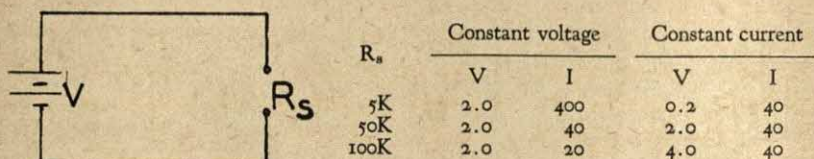


FIG. 1. SIMPLE CIRCUIT SHOWING VOLTAGE AND CURRENT CHANGES WITH CHANGES OF  $S$ 'S RESISTANCE

equivalent change in voltage or current when one or the other is held constant. This range of resistance-level may not normally occur with one  $S$  in one session but would be expected when several  $S$ s were placed in the apparatus or when one  $S$  served on several occasions. It follows, then, that a circuit designed to approach constant current conditions would best satisfy the first assumption.

A wide variety of circuits are used in commercially available equipment, including series and series-parallel designs. No one of these designs adequately approaches conditions of constant current. In Fig. 2 it is apparent that constant current con-

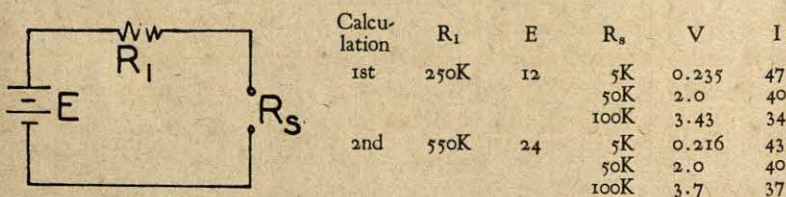


FIG. 2. USUAL COMMERCIAL CIRCUIT, SHOWING THAT CONSTANT CURRENT CONDITIONS ARE APPROACHED

ditions are approached when  $R_s$  is very small compared to the series resistance  $R_1$ . This requires, however, such a large voltage,  $E$ , that minor electrode movements, which normally produce only artifacts, will also produce the sensation of 'tickling' or even shock, which in turn, will influence  $S$ 's palmar skin-resistance.

<sup>1</sup>The arbitrary choice of 40  $\mu$  amp. follows C. W. Darrow's proposal for a standard current for GSR measurement. Currents as high as 200 and as low as 4  $\mu$  amp. have come to the attention of the author. In the former case, ranges of  $S$ 's resistance are rarely over 15,000 ohms; in the latter case, normal resistances are of the order of megohms. Apparently the magnitude of the current influences the absolute magnitude of the resistance measured.



Fig. 3 represents a design which overcomes this last objection and is commonly used in current apparatus. The price of solving the problem by this method includes decreased signal voltage per 1000 ohm change in resistance requiring excessive amplification to drive an inking galvanometer pen. Both the series and the series parallel circuits produce non-linear scales hence the problem of satisfying the second assumption is facilitated when conditions of constant-current are realized because the

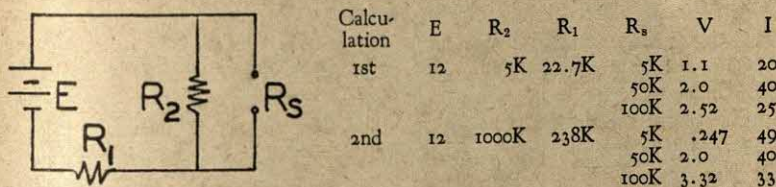


FIG. 3. CIRCUIT DESIGN TO ELIMINATE TICKLE AND SHOCK, AND CURRENT CONDITIONS OBTAINED WITH IT

variation of voltage bears a direct linear relationship with change in the resistance-level. This is not true if both voltage and current vary simultaneously. What is needed, then, is a constant current circuit. We now turn to the description of a circuit which satisfies the second and third requirements and, at the same time, best approximates the conditions necessary to satisfy the first requirement by providing automatic and continuous constant current.

*Constant current.* For purposes of description, suppose the proposed equipment necessary to measure resistance-level is divided into three parts, as in Fig. 4. Part 1

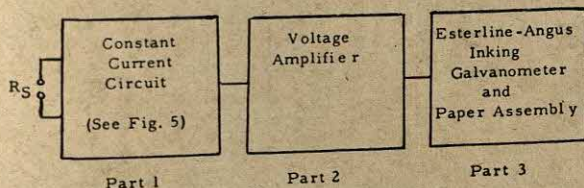


FIG. 4. BLOCK DIAGRAM OF THE APPARATUS

is a circuit which maintains a constant current through  $S$  at all times; Part 2 so amplifies the voltage signal from Part 1 that Part 3, the inking galvanometer, can be driven with a reasonable sensitivity.

A circuit for Part 1 is illustrated in Fig. 5. Inspection of the circuit diagram indicates that any current flowing through  $S$  must also flow through the triode  $V_1$ . If constant current is to be maintained then the function of  $V_2$  in the circuit will be to control the magnitude of the current and hold it constant regardless of any changes in resistance on the part of  $S$ . Such control is accomplished in the following manner. A minute change in the current flowing through  $R_1$ ,  $R_2$ , and  $R_3$  will raise or lower the voltage on the grid of triode  $V_1$ . At the same instant the voltage at the plate of triode  $V_1$  will change in an inverse direction, with a magnitude equal to the shift of the grid voltage multiplied by the amplification factor of the tube  $V_1$ . This



amplified voltage change is directly connected to the grid of the triode  $V_2$  which in turn tends to counteract the original change in the current flowing through triode  $V_1$ . In the circuit illustrated, it is possible to keep the current flowing through  $S$  constant within 0.5% for changes in  $S$ 's resistance ranging from 0 to 250,000 ohms because the amplification factor of both  $V_1$  and  $V_2$  are utilized in the control circuit. Where a 40- $\mu$  amp. current is used there is a 0.04-v. signal per 1000 ohms available from the plate of  $V_2$ . This signal is easily amplified by a two stage voltage amplifier to provide adequate sensitivity (e.g. 1000 ohms, full scale deflection).

This circuit has several advantages heretofore not available in GSR measurement. (1) Current is held constant continuously and automatically. There is no need for special adjustments for each resistance reading as has been true in the past when constant current measurement was achieved by the use of a Wheatstone Bridge circuit. (2) The inking-chart uses a linear scale which is easier to interpret and more accurate at nearly full deflection of the galvanometer. (3) Conditions at the electrodes remain relatively constant over time because current is held constant. Additional stability is achieved if both electrodes are 'fixed' before they are used;

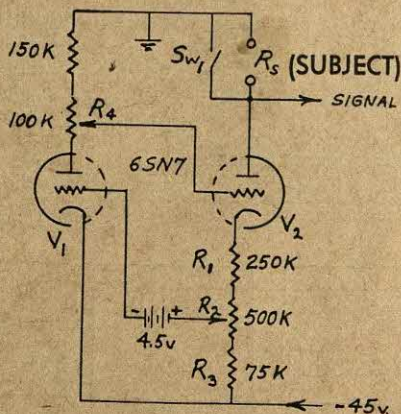


FIG. 5. CIRCUIT DIAGRAM FOR PART 1 OF THE APPARATUS

i.e. passing a fairly large electric current through them while they are immersed in their electrolytic solutions, thus building up large quantities of oxidation and reduction by-products. Once fixed, an electrode can operate for many hours at 40  $\mu$  amp. without perceptibly shifting the balance of the chemical reactions.

To conserve space, a complete circuit diagram of the voltage amplifier and pen-equipment are not included in this article. Such information, if needed, is available upon request from the author.

**Summary.** Three requirements must be satisfied if 'resistance level' is to be a useful measure of palmar skin-resistance. (1) Conditions at the electrodes must be constant or nearly constant not only over the period of resistance to be measured, but also from one experimental session to the next. (2)



The measuring instrument should record on an absolute scale of resistance. (3) Continuous recording must be possible, such as an inking galvanometer using a roll of paper. Specific suggestions for meeting these requirements have been proposed, based on equipment the author has successfully used over a period of several years.

## APPARATUS NOTES

### AN ELECTRICAL LATCH RELAY: A SUBSTITUTE FOR MECHANICAL LATCH RELAYS

In designing apparatus for use in an experimental laboratory it is frequently desirable to control the operation of an electrical circuit by means of electrical impulses. Thus, a sector disk shutter operated by a synchronous motor, may be activated by closing momentarily a pushbutton type of switch. The motor continues to operate until a second switch, controlled by a cam on the sector disk, is momentarily actuated. Mechanical latch relays are customarily used for this type of control.

The electrical latch relay, described here, is considered to be superior to the conventional mechanical latch relay for most applications because of its greater

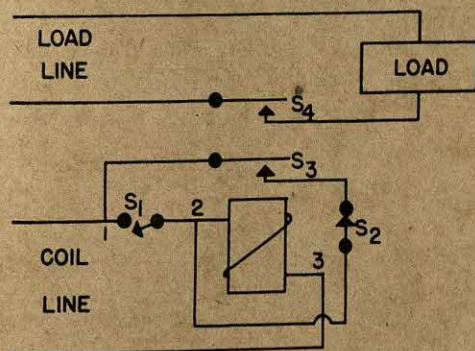


FIG. 1. WIRING DIAGRAM

simplicity, lower cost, and ready availability at any radio supply store. In addition, standard relay coils are available in a variety of voltage ratings. A large number of relay contact combinations may be constructed, precluding the necessity of designing the apparatus for a particular type of mechanical latch relay.

In principle, one pole of an ordinary double pole, single throw relay is used to bypass the switch which energizes the relay coil. Through it, the relay remains energized even though the coil switch is immediately opened. The relay is reset by opening a second switch in the bypass circuit. Fig. 1 shows the wiring diagram for



converting a general purpose double pole, single throw relay to an electrical latch relay. The sequence of operation is:

(1) When switch  $S_1$  is closed the relay coil is energized through circuit 1, 2, and 3, closing the relay contacts  $S_3$  and  $S_4$ .

(2) Closing contact  $S_3$  completes the circuit through 1,  $S_3$ ,  $S_2$ , and 2, bypassing switch  $S_1$ . Even though switch  $S_1$  is now opened the relay coil remains energized, holding relay contacts  $S_3$  and  $S_4$  closed.

(3) Opening switch  $S_2$  breaks the bypass circuit 1,  $S_3$ ,  $S_2$ , and 2, deenergizing the relay coil and opening relay contacts  $S_3$  and  $S_4$ . The device is then ready for the next cycle of operation.

A single pole, single throw contact assembly may be used in applications where the load voltage is the same as that required to operate the relay coil. In such instances  $S_3$  is used to control the load as well as to energize the relay coil. One side of the load is connected at 3 and the other is connected between  $S_2$  and  $S_3$ . Thus, if it is desired to use a 6-v. relay coil to control a 6-v. electric motor, a single pole, single throw contact assembly may be used. On the other hand, if a 6-v. relay coil is used in controlling a 110-v. motor a double pole, single throw contact assembly is required.

Several electrical latch relays of this design have been in almost continuous operation in this laboratory during the past three years in food delivery mechanisms, in the automatic delivery table of an automatic maze, in animal respirometers, and in sector disk shutters. During this time they have given no trouble and have had no breakdowns. As a precautionary measure, it has been the practice to clean the contacts with fine emery paper before operating them for protracted periods when a failure would be critical.

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## NOTES AND DISCUSSIONS

### A MOTOR HYPOTHESIS OF PERCEPTUAL DEVELOPMENT

Considerable attention has been focused recently upon the relationship between the various sensory modalities and visual perception. Gibson and Mowrer originally suggested that the visual vertical and horizontal are not determined primarily by visual cues, but by postural stimuli, and that posture is a primary capacity of the organism with the ability to see "up-down" and "right-left" being secondary.<sup>1</sup> Gibson, however, has deviated from this original point of view and has written recently in regard to it: "In making this suggestion, I admit having made what seems to be a mistake."<sup>2</sup> Gibson then asks why we should assume that motor ability is prior to visual perception since they are both closely interrelated. In normal adult visual perception, it appears that visual, other sensory, and motor cues are interrelated.<sup>3</sup> From the developmental point of view, however, some priority of emphasis is indicated. It seems, to the present writer, that evidence is accumulating which supports the following hypotheses: (1) an evolving percept succeeds through several developmental stages before the stage of normal adult visual dominance is reached; and (2) at each of these developmental stages, motor and other afferent sensory cues are utilized in perceptual actualization.

Years ago, Gelb and Goldstein found that a brain-injured patient was able to read only if he were allowed to rely upon some type of body movements.<sup>4</sup> This subject usually relied upon head movements, but if prevented from these, substituted hand and body movements. If all of these movements were prevented, the patient could read nothing whatever. Similarly, Gellerman found that a chimpanzee, in learning to discriminate an upright from an inverted triangle, traced the outlines with a finger.<sup>5</sup> Riesen reared chimpanzees from birth onwards in complete darkness.<sup>6</sup>

<sup>1</sup> J. J. Gibson and A. H. Mowrer, Determinants of the perceived vertical and horizontal, *Psychol. Rev.*, 45, 1938, 300-323.

<sup>2</sup> Gibson, The relation between visual and postural determinants of the phenomenal vertical, *ibid.*, 59, 1952, 371.

<sup>3</sup> H. H. Witkin, Perception of body position and of the position of the visual field, *Psychol. Monog.*, 63, 1949, (no. 302), 1-46; Heinz Werner and Seymour Wapner, Toward a general theory of perception, *Psychol. Rev.*, 59, 1952, 324-338.

<sup>4</sup> A. Gelb and Kurt Goldstein, Figural Blindness, in W. Ellis, *A Sourcebook of Gestalt Psychology*, 1939, 315-325.

<sup>5</sup> L. W. Gellerman, Form discrimination in chimpanzees and two-year-old children: I. Form (triangularity) *per se*, *J. Genet. Psychol.*, 42, 1933, 3-26.

<sup>6</sup> A. H. Riesen, The development of visual perception in man and the chimpanzee, *Science*, 106, 1947, 107-108.



When brought into the light as adults, the chimpanzees showed no signs of recognizing their feeding bottles visually but did recognize them tactually. Some children, when learning to count, touch objects consecutively while saying the numbers aloud and when learning to read mouth the words before reaching the so-called 'silent reading' stage. Do these reports of Gelb and Goldstein, Gellerman, and Riesen, as well as these everyday observations indicate an early primary perceptual involvement in terms of motor and somesthetic cues?

Siegel reared birds from birth to time of training without specific visual form-definition.<sup>7</sup> The birds were then trained monocularly to discriminate between a circle and a triangle. When tested for transfer from the eye used in training to the contralateral eye, neither 100% nor 0% transfer was shown. What was shown was a transfer of training ranging between 24-50% as contrasted with nearly 100% transfer for normally reared control birds. These data are in agreement with results obtained by Riesen who, in a similar experiment used cats which were reared without visual form-definition.<sup>8</sup> Is it possible that the visually deprived Ss of Siegel and of Riesen were reacting at a lower individual functional stage?

Some evidence indicating different stages of perceptual learning has been previously indicated by Deese, who trained two groups of rats in a U-maze.<sup>9</sup> After reaching criterion, one group (non-response extinction) was placed in the empty food box and then the correct response was extinguished by non-reinforcement. The correct response was extinguished by non-reinforcement in the second group, but this group experienced no time in the empty food box prior to the extinction trials (response extinction). Deese found that the 'non-response' group extinguished faster than the 'response' group. More important, the 'non-response' group showed no spontaneous recovery, while the 'response' group showed spontaneous recovery. Deese hypothesized that this may indicate that there are two types (stages?) of learning in this maze-situation.

If we assume, as Hebb implies,<sup>10</sup> that there are different stages of perceptual learning, we may also assume that the cues utilized at each of these stages are different. It is possible that visual dominance may characterize adult perceptual learning while cues at a tactual-motor-kinesthetic level may predominate in the early stages of perceptual learning. We postulate that there are stages in the development of visual domination. The stages shown in Fig. 1 are proposed.

Also shown in Fig. 1 is the stage at which the Ss of each of the experimenters mentioned above may have been reacting. For instance, the Ss of Gelb and Goldstein, who could read only when allowed movements of one

<sup>7</sup> A. I. Siegel, The effects of deprivation of visual form definition upon the development and transfer of simple form definition in the ring dove, Unpublished Ph. D. Thesis, New York University, 1952.

<sup>8</sup> Riesen, *op. cit.*, 107.

<sup>9</sup> James Deese, The extinction of a discrimination without performance of the choice response, *J. Comp. Psychol.*, 44, 1951, 362-366.

<sup>10</sup> D. O. Hebb, *Organization of Behavior: A Neuropsychological Theory*, 1949, 317, 321, 326.



type or another may have been fixated at the *motor*-somesthetic stage. The italics in Fig. 1 indicates the dominant aspect of each hypothetical stage.

With this schema, we may then speculate as to the basis of the difference in transfer found between the normally reared and visually deprived Ss of Siegel and of Riesen. The normally reared animals, due to their previous visual and motor experiences, may have reached the secondary or adult stage prior to the experimental situation. On the other hand, the experimental animals, lacking previous experience in the perception of form, may have been operating at a lower individual functional stage; *i.e.* relying upon different cues to actualize a percept. Assuming that the motor indications and components may have been established in the initial training sessions, we may attribute the lack of 100% transfer to the number of trials

PRIMARY STAGE →	INTERMEDIATE STAGE →	SECONDARY STAGE
<i>motor</i> -somesthetic	<i>motor</i> -visual	<i>visual</i> -motor
Gelb and Goldstein	Gellerman	visual function of
Riesen (chimpanzees)	Siegel	normal adult
	Riesen (cats)	

FIG. 1. DEVELOPMENTAL STAGES OF THE VISUAL FUNCTION OF NORMAL ADULTS

necessary to establish links between the already established motor indications and the 'inexperienced' optical pathways. Thus, the savings shown would be due to the transfer of the motor aspect of the situation.

Hebb, following the traditional lead, has placed an important, if not exclusive, emphasis on the rôle of eye movements in the development of a percept. We propose that this motor involvement be broadened to include other sensory and motor components. For instance, the rat who learns to perceive the positive from the negative stimulus, in a Lashley jumping stand makes head movements, eye movements, and body movements, and receives kinesthetic and proprioceptive cues. We propose that a differential emphasis is placed upon each of these types of cues as a percept evolves. At each stage of perceptual development different cues may be maximally utilized to actualize a percept.

Such a proposal would not be opposed to the point of view of Werner and Wapner, but would be a developmental complement to their point of view.

Our developmental motor hypothesis offers fruitful predictions. For instance, we predict that animals reared without visual form definition would show, when trained monocularly in a visual discriminatory task, no transfer whatsoever to the contralateral eye provided a different motor response was required when the afferent avenue of experience was shifted. We also



predict that Ss with congenital cataract, who are pre-operatively trained to recognize forms by tactual and kinesthetic cues, will not recognize these forms post-operatively if only visual cues are relied upon.

To state our thesis concretely, we believe: (1) that an evolving percept passes through differential experiential stages; (2) that, at each stage, different cues are intergrated with previously established cues to yield the normal adult perception; and (3) that the motor aspect of an evolving percept is basic when the motor component is expanded beyond mere eye movements.

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### A NOTE CONCERNING THE *VEG* SCALE OF APPARENT WEIGHT

Experimental investigators have long sought for relations between scales of physical and psychological magnitude. Important progress in this direction has been made by Harper and Stevens who have developed the *veg* scale of apparent weight.<sup>1</sup> They derived both graphical and mathematical procedures which may be used to translate data from it to a scale of physical values. In fact, the mathematical techniques have appeared to be of such general usefulness that they have been adopted in psychophysical studies of the taste mechanism.<sup>2</sup> In one study, however, in which the data met all explicit requirements, it was found that the formula could not be applied.<sup>3</sup> A consequent examination of the derivation of the formula of the *veg* scale revealed an apparent mathematical error which acts to limit its general applicability. The purpose of this note is to point out the difficulty and to suggest a possible solution.

Derivation of the *veg* formula assumes that the experimental data meet the requirements expressed by the relation:

$$(\log I - \log I_h) / (\log \Psi - \log \Psi_h) = (\Delta \log I) / (\Delta \log \Psi) = a + b \log I \dots \dots \dots [1]$$

where *I* is the intensity or weight of the stimulus in physical units; *I<sub>h</sub>* is an

<sup>1</sup> R. S. Harper and S. S. Stevens, A psychological scale of weight and a formula for its derivation, this JOURNAL, 61, 1948, 343-351.

<sup>2</sup> D. R. Lewis, Psychological scales of taste, *J. Psychol.*, 26, 1948, 437-466; J. G. Beebe-Center and D. Waddell, A general psychological scale of taste, *ibid.*, 26, 1948, 517-524.

<sup>3</sup> S. MacLeod, Personal Communication, 1951.



intensity judged to be half as strong as  $I$ ;  $\Psi$  is a psychological magnitude corresponding to stimulation with  $I$ ;  $\Psi_h$  is the psychological magnitude corresponding to stimulation with  $I_h$  and is numerically equal to  $1/2 \Psi$ ;  $a$  is a constant; and  $b$  is a constant.

To obtain their formula, Harper and Stevens simply substitute  $(d \log I) / (d \log \Psi)$  for  $(\Delta \log I) / (\Delta \log \Psi)$  and solve the differential equation,

$$d \log \Psi = (d \log I) / (a + b \log I) \dots \dots \dots [2]$$

The point to be made is that this substitution ignores the fact that  $\Delta \log \Psi$  is actually a constant equal to  $\log 2$ .  $\Delta \log \Psi$  cannot be treated as a differential, and accordingly, Equation [2] does not follow from Equation [1]. It may be shown that as a result the *veg* formula provides an adequate description of data only when a plot of  $\log I_h$  versus  $\log I$  has a slope of  $+1$ ; this condition is approximated by the data of the original *veg* scale.

An attempt has been made to derive an expression which has more general usefulness. To accomplish this the assumed relation

$$(\log I - \log I_h) / (\log 2) = a + b \log I \dots \dots \dots [3]$$

is expressed in the form

$$I_h = m I^n \dots \dots \dots [4]$$

where  $m$  is the constant  $2^{-a}$  and  $n$  is  $1 = b \log 2$ . Using this simpler notation, we may make a table (Table I) of values of  $I$  and  $\Psi$  for a series of

TABLE I  
CORRESPONDING SUBJECTIVE AND PHYSICAL SCALE-VALUES FOR A  
SERIES OF SUCCESSIVE JUDGMENTS

Judgment	Values of $\Psi$	Values of $I$
reference values	$\Psi_0$	$I_0$
first	$(1/2)\Psi_0$	$mI_0^n$
second	$(1/2) \cdot (1/2)\Psi_0$	$m(mI_0^n)^n$
third	$(1/2)^3\Psi_0$	$m^{1+n+n^2}I_0^{n^3}$
...	...	...
$r$ -th	$(1/2)^r\Psi_0$	$m^{1+n+n^2+\dots+n^{r-2}+n^{r-1}}I_0^{n^r}$

consecutive judgments where each judgment is made with a stimulus which is subjectively half that used for the previous judgment.

These are progressions which describe the assumed psychological and physical scales. Now, if we remember that

$$1 + n + n^2 + \dots + n^{r-2} + n^{r-1} = (1 - n^r) / (1 - n), \dots \dots [5]$$

we may write the  $r$ -th value of physical magnitude,  $I_r$ , as

$$I_r = m^{(1-n^r)/(1-n)} I_0^{n^r} \dots \dots \dots [6]$$

The corresponding value of  $\Psi$  is given by

$$\Psi_r = (1/2)^r \Psi_0 \dots \dots \dots [7]$$



Equations [6] and [7] are now solved simultaneously to remove  $r$ . In this way we obtain the desired expression relating  $\log \Psi$  and  $\log I$  for any pair of consecutive judgments.

$$\log \Psi_r = [\log 2 / \log n] [\log (\log I_0^{1-n} - \log m) - \log (\log I_r^{1-n} - \log m)] + \log \Psi_0 \dots \dots \dots [8]$$

Although this formula may appear formidable, the constants are easily evaluated. For example, suppose we find from a plot of hypothetical data that 16 physical units appear to be half as great as 64 and that 18 units seem to be half of 81 units. These quantities may be substituted in Equation [4], and the resulting equations may then be solved to yield values of  $m$  and  $n$ . Thus,

$$16 = m 64^n, 18 = m 81^n, \text{ and } m = 2, n = 1/2.$$

Now let us further decide to equate 64 physical units to 32 psychological units; then  $I_0 = 64$  and  $\Psi_0 = 32$ . If all of these values are substituted in Equations [8] we obtain

$$\log \Psi_r = - [\log (\log 64^{1/2} - \log 2) - \log (\log I_r^{1/2} - \log 2)] + \log 32.$$

This may be reduced to

$$\Psi_r = [\log (I_r^{1/2} / 2)] [32 / \log 4] = 26.58 \log I_r - 16.$$

The result is a simple expression relating our physical and psychological scales.

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## THE TRADITIONAL FORMULAS FOR PITCHES CREATED IN THE COCHLEA

Ever since the middle of the eighteenth century, when Romieu, Sorge, and Tartini revealed that the ear adds self-created pitches to notes presented simultaneously, attempts have been made to bring these 'secondary' pitches *under a formula* so simple that a third-grade pupil could foretell which tones are physiologically added. During the next three-fourths of a century Tartini's notion prevailed in the musico-psychological literature that, after the frequency *ratio* had been reduced to very small numbers, the pitch '1' was added by the ear. For example, if the primary notes were  $c'$  ( $= 3$ ) and  $a'$  ( $= 5$ ), the secondary, sometimes called the 'subjective' note, would be  $F$  ( $= 1$ ); or for the primaries  $e'$  ( $= 5$ ) and  $c''$  ( $= 8$ ) the secondary would be  $C$  ( $= 1$ ); and that would be all. In accordance with the speculative tendency of his time, Tartini must have recalled the ancient Pythagorean 'harmony of the spheres' which also represent the numbers



from '1' up the series. Tartini actually tried to build a theory of musical harmony upon the secondary pitches of the ear but his formula, which I shall refer to as Formula I, found no permanent favor among musicians.

Formula II, which replaced Tartini's formula, was reported by Hallström in a dissertation in 1818 and republished for a wider public in 1832.<sup>1</sup> It was popular during the second three-quarters of a century. Hallström introduced his pitch-concept of "differences of the first order, the second order, the third order, etc." For example, if one starts with the ratio 5:7 (these tones being the primaries), the secondary pitch of the first order would be '2' because  $7 - 5 = 2$ . The pitch of the second order would be '3' because  $5 - 2 = 3$ . The pitches of the third order would be '4' because  $7 - 3 = 4$ , and '1' because  $3 - 2 = 1$ . No mechanical explanation of these partly observed and partly alleged occurrences was ever offered to supplement this Formula.

Formula III has appeared in the textbooks of the last half-century.<sup>2</sup> This in spite of the fact that it is nothing but a *mistranslation* from Helmholtz. The great scientist makes the remark as 'a mere guess' (*Vermuthung*) that difference and summation tones *would have to result if the anatomy of the ear could be shown* to act like a double siren attached to and under pressure from a single air reservoir.<sup>3</sup> No anatomist has ever given support to that 'guess' by showing that the cochlea could act mechanically as a two-tone siren does. That the middle ear might act in this manner has also been discredited.<sup>4</sup> Helmholtz's cautious and conditional 'guess,' has, nevertheless, entered the textbooks dogmatically, unsupported by a listener's testimony, as *an authoritative endorsement by him of pitches created by the cochlea*. Formula III, then, asserts that the cochlea creates summation and difference tones.

Tests of these formulas may be made as follows.

(1) My first experiment was made in the following manner. An audio-generator acting on a loud-speaker (not a telephone because that would be

<sup>1</sup> Poggendorff's *Annalen der Physik und Chemie*, 24, 1832, 438.

<sup>2</sup> S. S. Stevens and Hallowell Davis, *Hearing*, 1928, 184: "When two loud tones are sounded together, we hear . . . the sums and differences."

<sup>3</sup> H. Helmholtz, *Tonempfindungen*, 5 ed., 1896, last paragraph, p. 259, describes the double siren, the easily heard combination tones of which are not ascribed to anatomy; and in Annex XII, p. 625, last six lines, he "guesses" that, though the other parts of the sense organ hardly seem qualified, the tympanum might do it. But such a rôle of the tympanum has since then also been discredited.

<sup>4</sup> E. G. Wever, *Theory of Hearing*, 1949, 383: "Even in the presence of undue stresses, the middle ear apparatus carries out its duties of sound transmission with great faithfulness."



an unusual way of listening to tones) was tuned to 1200 ~, about  $d'''$  in musical terms. A second generator (with its own loud-speaker) was tuned at 400 ~ to make the exact interval of a downward duodecima, *i.e.* about  $g'$  in musical terms. A third generator also with its own loud-speaker, was tuned at 640 ~, a Minor Sixth, with the aid of frequency 400. An experienced acoustician can do all this if endowed with enough patience. The tones of 640 ~ and 1200 ~ gave us the ratio 8:15. The tone 400 ~ being a mere auxiliary, was silenced.

With the ratio 8:15 thus available in all absolute and relative volumes neither the pitch 1 of Formula I nor the pitches 7 ( $15 - 8$ ), nor 1 ( $8 - 7$ ),  $14 = (15 - 1)$ ,  $6 = (14 - 8)$  nor 9 ( $15 - 6$ ), nor any other differential pitch of Hallström's "higher order" was audible. Thus both Formula I and II are discredited. Formula III is also thoroughly discredited, since no trace of pitch 23 ( $8 + 15$ ) was audible.

(2) The next experiment was made as follows. An audio-generator was tuned to 830 ~, about  $g'' \sharp$  in musical terms. A second generator was tuned exactly one octave lower, at 415 ~. A third generator was tuned exactly at 332 ~, *i.e.* a Major Third below 415 ~. The generator tuned at 415 ~ was now retuned at 581 ~ to give the so-called Natural Seventh over 332 ~. We now had  $581:830 = 7:10$ . Then 581 ~ and 830 ~ were sounded together and all audible pitches carefully observed.

Only two secondary pitches could be heard, 3 and 1, the latter considerably more conspicuous than the former. According to Hallström's formula one should also hear 4 ( $7 - 3$ ), 9 ( $10 - 1$ ) and 6 ( $7 - 1$ ), and even some differential pitches of the 'third' order. None of them were audible. Thus is Formula II again discredited. Furthermore, pitch 17 ( $7 + 10$ ) was not audible, which discredits Formula III. The inadequacy of Formula I lay in the fact that pitch 3 was heard although it cannot be derived from it.

(3) A third experiment was made. A generator was tuned to 1107 ~, about  $c''' \sharp$  in musical terms. A second generator was tuned to 738 ~, a Fifth below. A third generator was tuned to 492 ~, a Fifth below 738 ~. We now had  $492:1107 = 4:9$ . These were sounded together. We could not hear any secondary pitches whatsoever. This discredits all three formulas, in particular Formula III, since no trace of a pitch 13 ( $4 + 9$ ) could be discerned.

(4) In the next experiment a generator was tuned to 600 ~, a second generator to 720 ~, a Minor Third higher, and a third generator to 1080 ~, a Fifth above 720 ~. We now had  $600:1080 = 5:9$ . When these two



tones were sounded together, only two secondary pitches were heard: 1 and 4. The former was weak but clear. Pitch 4 was heard but was very faint. This discredits Hallström's formula because it seems unreasonable that a differential pitch of *lower* order should be much *weaker* than one of higher (*i.e.* derived) order. The pitch 14 ( $5 + 9$ ) predicted by Formula III could not be heard at all.

All three formulas are useless arithmetically, without even mentioning that none of them offers an idea *how the cochlea functions mechanically*. If a reader suggests that I offer a better *formula*, I answer, "I have none," but I add this advice: "apply the hydraulic theory of cochlear mechanics and see what you get."<sup>5</sup> That requires more than mere subtraction of numbers; reading trigonometric tables is requisite, and it can not be done in a few seconds but takes many hours or even days. I, nevertheless, recommend the procedure. It gives at least the satisfaction that it never predicts a summation tone. A summation tone produced by the ear has never been heard in an experiment conducted by me or by anybody reporting in the auditory literature.

Miami, Florida

MAX F. MEYER

### A SIMPLIFIED METHOD OF MEASURING KINESTHETIC REACTION-TIMES

Chernikoff and Taylor have recently reported two methods of measuring kinesthetic reaction-times: an 'arm-stop' method and a 'key-release' method.<sup>1</sup> Both of these methods require elaborate equipment and a goodly amount of time on the part of both the *S* and the *E*. This paper reports a simpler method which is rapid, does not require much equipment, and yields a satisfactory reliability. It is based upon a modification of the 'key-release' method.

The only elaborate apparatus required is a chronoscope, an electromagnet, and a source of direct current. In the experiment to be reported, a 67 v. B-battery is employed. The only other equipment is a two-way switch, a telegraph key, a mount for an electromagnet, and a wrist-band. To the wrist-band is riveted a piece of soft iron which is placed in contact with the magnet. The source of DC current is fed to the magnet through one half of the two-way switch. The other half of the switch

<sup>5</sup> M. F. Meyer, "The cochlea does more than analyze. . . . In addition to analyzing, the cochlea creates," *How We Hear*, 1950, 79.

<sup>1</sup> Rube Chernikoff and F. V. Taylor. Reaction-time to kinesthetic stimulation resulting from sudden arm displacement, *J. Exper. Psychol.*, 1952, 43, 1-8.



is connected with the chronoscope. The chronoscope is started when *S* has the telegraph-key depressed and the switch is in the proper position. The wrist-band is fastened to *S*'s right wrist and brought into contact with the magnet. With his left hand he depresses the telegraph key. When the magnet circuit is on, the chronoscope circuit is off. When *E* throws the switch, this cuts off the magnetic circuit, the arm starts to fall, and the chronoscope is started. *S* is instructed to release his left hand from the key as soon as he is aware that his right hand is falling. His reaction-time is then read directly from the chronoscope.

Twenty-three men were used as *Ss* whose ages varied from 17 to 23 yr. The procedure was as follows. *S* was seated at the apparatus with wrist-band on his arm. He was then given two practice trials and the purpose and procedure were explained to him. He was permitted to see the apparatus during these trials but during the eight experimental trials, which followed immediately, he was blindfolded. Before every trial he was given a 'ready' signal, then the switch was thrown and his arm fell. He was told, however, that a variable time-interval would lapse between the signal and the throwing of the switch, that he was not to try to outguess *E*, and that his key should be released only when he became aware of the fall of his arm. He was not instructed to let his arm fall freely, as Chernikoff and Taylor did with their *Ss*, hence every *S* halted its fall in course.

The data were treated by the procedures used in the analysis of variance and the reliability of their internal consistency was determined. Table I summarizes the results.

TABLE I  
ANALYSIS OF VARIANCE OF KINESTHETIC REACTION-TIMES

Source	DF	MS	F
Trials	7	907.186	1.709
<i>Ss</i>	22	4166.446	7.840*
Error	154	531.450	
Total	183		

\* Significant beyond the 1-% level.

As can be seen, there is no significant practice-effect. The *F*-ratio indicates that the data have significant reliability. Using the conventional formula for internal consistency, the reliability coefficient is found to be 0.872 for all eight trials.

An analysis was also made comparing these data with those reported by Chernikoff and Taylor. Though these authors did not report their raw data, they did list their means and variances for their two groups of *Ss*. We consequently worked backwards to find the necessary constants and then recombined the data to obtain one mean and variance for both groups. These results are compared with ours in Table II. There are no significant differences between either the means or variances, thus the hypothesis of sampling from the same population is retained.



Since the method described here does not require elaborate apparatus, is rapid (taking about 10 min. per *S*) and reliable, and yields the same norms

TABLE II  
COMPARISON OF THE CHERNIKOFF-TAYLOR RESULTS WITH THE PRESENT RESULTS

	Present results	C.-T. results	Significance
Mean	158.34	151.49	$t=0.97$
Variance	520.81	277.56	$F=1.876$
SD	22.82	16.66	
<i>N</i>	23	14	

as Chernikoff and Taylor's more complex procedure, it is thought that it is a profitable one to use in measuring kinesthetic reaction-times.

Human Resources Research

VICTOR H. DENENBERG

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## STIMULUS CONTROL OF OPERANT RESPONDING IN THE PIGEON

In a recent experiment on bringing the operant responding of pigeons under the control of a visual stimulus, observations were made which indicate that the rate of operant responses is a function of the magnitude of the stimulus.

The birds used in this study were deprived of food until they reached 80% ad libitum weight. They were placed on a 2-min. variable interval reinforcement schedule.<sup>1</sup> Imposed upon this schedule for the first two birds was the requirement that with one discriminative stimulus the bird must have emitted a rapid burst of responses immediately before reinforcement, and with a second stimulus it should delay for 3 sec. between responses as a requirement for reinforcement. It was thought that by this means the two discriminative stimuli might be brought to control responding at highly divergent rates. A spot of light was projected on the key which the bird was to peck. This represented the discriminative stimulus. Two spots were used initially, a smaller one 1/16 in. in diameter, and a larger 5/16 in. in diameter. These were alternated with their appropriate reinforcing contingencies every 4 min.

With the first bird, rapid responding was appropriate to the large spot, and a low rate of response to the small. This organism developed the expected differentiation in rates within the first 8-hr. experimental period. When, however, rapid responding was paired with the small spot of light and slow responding with the large spot, as was the case with the second bird, there was only slight indication that the change was occurring in the expected direction after 120 experimental hours.

<sup>1</sup>R. S. Harper and C. R. Oldroyd, An inexpensive differential color-mixer, this JOURNAL, 65, 1952, 614-616.



Individual differences would probably not account for disparities of this magnitude; therefore three more birds were run on the 2-min. schedule with no additional contingencies. A third spot 3/16 in. in diameter was introduced in order to determine a possible gradient, and the three stimulus-spots were alternated every 4 min. as before. After these birds had attained stable rates, they were given a 4-hr. extinction-run. The mean number of responses for these three birds was 303, 674, and 1010 responses to the small, medium, and large spots, respectively. There were no reversals in response-rates for any of the birds during this run. The only difference between birds was in the total number of responses emitted.

To isolate the relevant variables, filters were used to equate the three spots of light for total light flux. The first five birds were then reconditioned on the simple 2-min. schedule and were given another 4-hr. extinction-run. The mean number of responses for these animals to the small, medium, and large stimulus spots was 407, 393, and 262 responses respectively. No test of significance was made on these data, but it is clear from examination of individual cumulative response-curves that a reversal from the former condition was obtained by this means.

As stated before, reinforcing differential rates of response did not overcome the above described effect of illumination, nor did the simple administering of a greater number of reinforcements to responses made to the small spot. Before the runs with filters were made, two naïve birds were conditioned and run on the 2-min. schedule with only the small spot of light projected on the key. On the subsequent 4-hr. extinction-period they emitted to the small, medium, and large spots a mean of 519, 612, and 1379 responses respectively.

It seems, therefore, that light cannot be regarded as a neutral stimulus-condition. The use of light as a discriminative stimulus is subject then to the objection that its function for the organism is not uniquely determined by the experimental contingencies. It appears further that its effect upon the operant behavior of this particular species is a joint function of size of lighted area, and the brightness of unit area. The question of the origin of this effect, whether it arises from some primitive physiological mechanism, or whether it develops early in the life of the organism in relation to some activity such as food selection, remains an experimental one.

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EDWARD J. GREEN

### DIFFERENTIAL COLOR-MIXERS

In a recent number of this JOURNAL, an "inexpensive differential color-mixer" is described by Harper and Oldroyd.<sup>1</sup> The apparatus consists of a plane mirror mounted at an angle, somewhat less than 90°, to the shaft of a motor. The mirror is rotated in front of a stationary disk and the re-

<sup>1</sup> A description of this reinforcement schedule is given by B. F. Skinner, Are theories of learning necessary?, *Psychol. Rev.*, 57, 1950, 193-216.



sulting mixture is viewed by the observer through an opening in the center of the disk.

The illustration they give shows triple-sectored stationary disks that can be adjusted while the motor is rotating. In my paper, to which Harper and Oldroyd referred, double-sectored disks were described and recommended.<sup>2</sup> I found by actual trial and by geometric analysis that the usual single-sectored disks are unsatisfactory with the rotating mirror and only when *double-sectored* disks are used is the color field uniform. If, therefore, anyone contemplates duplicating this apparatus, *double-sectored* disks must be employed rather than the type of disk pictured by Harper and Oldroyd. In my paper I gave instructions for the preparation of two kinds of double-sectored disks.

Harper and Oldroyd overlooked completely the fact that I described two types of differential color-mixer and that the second (rotating mirror) type is identical in principle with the mixer they have described. My second type of mixer utilizes a plane mirror (3 in. in diameter) mounted at an angle of about  $86^\circ$  to the axis of rotation, with a set of stationary double-sectored color disks in front of the mirror. A photograph of two plane-mirror mixers was published (Plate IV). The color-field is observed through an opening at the center of the disks exactly as in the apparatus described by Harper and Oldroyd.

There are only minor differences between the construction of my mirror-mixer and the one described by Harper and Oldroyd. With my apparatus the angle of the mirror to the axis of rotation can be varied but with the apparatus of Harper and Oldroyd the angle of the mirror is fixed and constant. This adjustable feature gives a definite advantage in that the angle of the mirror can be changed for different sizes of disks and for different distances between disks, mixer, and observer.

When I first saw this note I was surprised to read: "Recently, when we needed a differential mixer, we took Young's principle, simplified the design, and considerably reduced the cost." Incidentally, the rotating-mirror color-mixer does work nicely both for continuous variation of color quality and for presenting a fixed series of disks—but only when double-sectored disks are employed.

University of Illinois

PAUL THOMAS YOUNG

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<sup>2</sup> P. T. Young, A differential color-mixer with stationary disks, *J. Exper. Psychol.*, 6, 1923, 323-343.



## THE COMPARISON OF TWO CORRELATED SAMPLE VARIANCES

Current textbooks on statistical methods in psychology usually discuss adequately an exact solution to the problem of testing the hypothesis that the variances from two *independent* normal populations are equal.<sup>1</sup> The method employs the variance ratio  $F = s_1^2/s_2^2$ , where  $s_1^2$  and  $s_2^2$  are unbiased estimates of variance,  $s_i^2 = \Sigma(x_i - \bar{x})^2/(n_i - 1)$ ,  $i = 1, 2$ , with degrees of freedom  $n_1 - 1$  and  $n_2 - 1$ .

No information is presented, however, as how to test the significance of this difference when the samples involved are *correlated*. The ordinary variance ratio is *not* correct in the correlated case and considerable error can be introduced in using it inappropriately.

Fortunately, an exact test for the case of correlated samples has been developed and in fact this test-criterion has the familiar Student *t*-distribution.<sup>2</sup> The following example is taken from a genetic-study of the Army Alpha by W. A. Owens where, among other things, it was of interest to investigate the effect of age increments on individual differences.<sup>3</sup> Any study such as this, *i.e.* longitudinal in character, will involve correlated measurements.

In general, consider a sample of  $n$  Ss measured on some psychological trait yielding scores  $x_1$  and sample variance  $s_1^2$ . At a later date the Ss are measured again on the same trait with the same instrument, yielding scores  $x_2$  in this case, with sample variance  $s_2^2$ . Since the same Ss are used, the measurements  $x_1$  and  $x_2$  are correlated. Then, in order to test the hypothesis  $H: \sigma_1^2 = \sigma_2^2$ , *i.e.* the hypothesis of equal population variability, assume the observed  $x_1$  and  $x_2$  form a random sample from a normal bivariate population of correlation  $\rho_{12}$  and variances  $\sigma_1^2$  and  $\sigma_2^2$ . Under this model,  $H$  may be appropriately tested using the criterion,

$$t = [(s_1^2 - s_2^2) / \{4(1 - r_{12}^2) s_1^2 s_2^2\}^{1/2}] \cdot [(n - 2)^{1/2}],$$

which has the Student's *t*-distribution for  $n - 2$  degrees of freedom, and where  $r_{12}$  is the usual zero-order correlation coefficient estimate of  $\rho_{12}$ .

In 1919, a sample of 127 Iowa State College students obtained a variance

<sup>1</sup> J. P. Guilford, *Fundamental Statistics in Psychology and Education*, 2nd ed., 1950, 232-233; Quinn McNemar, *Psychological Statistics*, 1949, 228-231.

<sup>2</sup> E. G. Pitman, A note on normal correlation, *Biometrika*, 31, 1939, 9-12; W. A. Morgan, A test for the significance of the difference between the two variances in a sample from a normal bivariate population, *Biometrika*, 31, 1939, 13-19.

<sup>3</sup> W. A. Owens, Age and mental abilities—a longitudinal study, *Genet. Psychol. Monog.*, In Press.



of 0.81 on subtest 2, Arithmetic Reasoning, of the Army Alpha Examination.<sup>4</sup> In 1950 these same Ss were retested, a variance of 0.92 resulting. The correlation coefficient between the initial and retest scores was 0.69. For the college population of which this group is a sample, it is desired to test at the 1-% level of significance the hypothesis that the true variability on subtest 2 in 1950 is the same as it was in 1919. Here the test statistic becomes  $t = [(.92 - .81) (127 - 2)^{1/2}] / [4 (1 - .69^2) (.92) (.81)]^{1/2} = 1.03$ . For 125 degrees of freedom,  $t$  at the 1-% level is about 2.58. Hence using a two tailed critical region, the evidence does not suggest rejection of the hypothesis at the 1-% level of significance.

Iowa State College

RICHARD B. MCHUGH

### THE 1952 MEETING OF SECTION I, AAAS

The annual meeting of Section I (Psychology) of the American Association for the Advancement of Science was held in St. Louis, Missouri, on December 29 and 30. The five sessions of submitted papers were held, with two sessions devoted to Clinical and a session each to Training, Theoretical, and Experimental.

In addition Section I co-sponsored a symposium on "Men and Machines," with the sections on Medicine, Engineering, and Industrial Science. The program was arranged by Philip H. DuBois. The morning session was chaired by William A. Hunt and the afternoon by Paul M. Fitts.

The Vice-Presidential address was delivered by Harold Schlosberg on the topic "The Intensive Dimension of Emotion."

The following officers were elected for the coming year: Vice-President, Frank Beach; Secretary, Dewey Neff; and Committee-member, Clarence Graham.

Ohio State University

DELOS D. WICKENS

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<sup>4</sup>Based upon normalized standard scores; cf. reference given in footnote 3 for further details.



## BOOK REVIEWS

Edited by M. E. BITTERMAN, University of Texas

*Psychology: The Fundamentals of Human Adjustment.* By NORMAN L. MUNN. Second edition. New York, Houghton Mifflin, 1951. Pp. xvi, 624.

*An Introduction to Psychology.* By GARDNER MURPHY, with the assistance of HERBERT SPOHN. New York, Harper and Bros. 1951. Pp. xvii, 582.

Munn's well known text needs no introduction and its general organization does not depart substantially from that of the first edition. The main changes are stated by the author to be (1) a more precise definition of the scope of the subject; (2) the use of a larger proportion of material derived from researches on man with a corresponding reduction of the space devoted to animal studies; (3) a more extended treatment of the higher mental processes; and (4) a complete revision of the chapter dealing with human motivation. As in the earlier edition, the selection of topics is judicious, the illustrations lively and instructive, and the presentation clear throughout. The author deserves the admiration and gratitude of all concerned with the teaching of psychology at a University level.

Despite its many and varied excellencies, Munn's book offers a certain difficulty to a British reviewer, largely on account of the very real differences that exist between the scope and intention of psychological teaching in Britain and America. In Great Britain, at least in the older Universities, it is customary to encourage critical thinking on a limited range of topics rather than detailed acquaintance with the whole *corpus* of psychological knowledge. A book such as this might be thought to lull the unwary student into the belief that the author's conception of the scope and methods of psychology is final rather than provisional, that the basic factors which govern behaviour are clearly understood rather than dimly surmised, and that the techniques in current use represent the epitome of scientific progress in the psychological field. Hence he may be led to underestimate the vast extent of our ignorance in matters psychological and to overestimate the little that is securely known. Although psychology has achieved much in its brief career, it is important for the serious student to acquire a 'sense of problems' as well as mere knowledge of those which have already been solved. Only in this way can he hope to develop new and rewarding approaches to its intractable subject-matter. Can a book of this kind help him to do so?

In his appreciative introduction, Professor Leonard Carmichael lays stress on the value of Munn's presentation of psychology not only for the specialized student but also as a part of general education. This, again, causes the present reviewer some hesitation. While sharing to the full Carmichael's high opinion of the author's wide knowledge and gift for communicating it, he finds it hard to trace anything in the approach which might serve to forge a broader link with the Humanities. If by "general education" is meant a sense of scientific humanism this book signally fails to contribute to it. Carmichael perhaps seeks something which it was not the author's primary intension to provide; more likely, the present reviewer is influenced by a peculiarly Oxonian conception of "general education" which has little relevance



to the American academic scene. Be this as it may, no fault attaches to Munn, whose conception of psychology is both broader and brighter than that to which we are accustomed in many British Universities today.

The accuracy with which the author reports and assembles materials drawn from a very wide field of psychological investigation is altogether outstanding. None the less, specialists in the various fields covered will probably wish to offer a number of minor criticisms and corrections. The present reviewer, whose main interests lie in the borderlands of neurology, feels impelled to question the accuracy of three statements. First, there is the statement that aphasia in left-handed individuals is usually associated with injuries of the right cerebral hemisphere (p. 67). Recent work strongly suggests that, even in left-handed patients, it is more usually associated with lesions of the left hemisphere. Second, there is the statement that "all psychological processes are responses of organisms to stimuli" (p. 71). In view of recent work on the embryology of behaviour this position requires some qualification. Third, the assertion that "Pavlov . . . believed that all learning is reducible to conditioned responses" (p. 130) is surely an over-simplification, as every reader of Pavlov's "Reply of a Physiologist to Psychologists" (*Psychol. Rev.* 39, 1932, 91) will know. Certainly Pavlov made such a statement, but in view of the fact that he made equally dogmatic contradictory statements it is difficult, if not impossible, to know what he really did believe. These, however, are minor objections which in no way detract from Professor Munn's lucid, forceful, and authoritative presentation of physiological issues in psychology.

The book is well produced and has an adequate index. References are (from the student's point of view) admirably chosen and mercifully few. Some minor errors (e.g. in the placing of the colour plate illustrating colour defects) will doubtless be corrected in the next printing. An English edition of this book would be especially welcome.

Professor Murphy is unashamed in his belief that psychology should deal with people. His aim is to present a systematic introductory text in which the emphasis is throughout upon the individual human being, considered first from the biological and secondly from the social aspect. Although he owes much to Stern and Allport, Murphy differs from both in placing greater weight upon the biological basis of individuality and in showing himself more receptive to psychoanalytical ideas. In consequence, his book is likely to make a wide appeal to students who begin psychology with the not unreasonable expectation that the proper study of mankind is man.

Given this most promising frame of reference, it is perhaps a little disappointing to find that Murphy has not attempted a very much more radical presentation of psychology than in fact he has. The general plan of his book follows closely upon that of innumerable contemporary texts and must be regarded as in some sense a compromise between idealism and expediency—particularly notable in the earlier parts of the book, in which the presentation differs little from that which is conventional at the present time. Thus the chapters on heredity and environment, the special senses, innate reaction patterns, and emotions show little, if any, influence of the avowedly 'personalistic' standpoint from which the book is written. If psychology is to be defined as the study of individuals, does not its biological foundation require a very much more comprehensive account of the organism in its bodily aspects than



is given us here? Modern psychology, it is true, has endowed the mind with a body, but perusal of our present-day texts might lead the reader to suppose that this body consists of little more than the sensory receptors, the voluntary muscles, the central and autonomic nervous systems, and perhaps the endocrine glands. If 'personalist' psychology is to be a discipline rather than a faith, it is surely essential that it should be founded on a basis of human anatomy and physiology at least as thorough as that instilled into the ordinary medical student. Only thus can the concept of 'person' acquire full biological reality.

If the biological groundwork of this book is somewhat scanty, its author's account of general and social issues in psychology is altogether more rewarding. The central chapters on remembering, imagining, thinking and creative skills in general are marked by that rare combination of shrewd comment and down-to-earth fact displayed in outstanding form by William James. Although Murphy insists throughout that firm theory must be based upon controlled experiment, he never allows us to forget that "... each activity of a person can best be introduced not by describing the abstract process (e.g. perception, learning, thinking), but by describing a person carrying out such an activity." It follows that although broad, general forms of mental activity may be distinguished, empirical study must concern itself with their concrete manifestations in particular individuals. For example, thinking can be regarded as a broad term applicable to a wide range of mental activities, habits and skills; these may be elucidated by the experimental study of specific individuals in a variety of problem-situations. There is no universal 'thought process' and experiments based upon the premise that such a process exists (e.g. those of the Würzburg school) are doomed to end in disaster. Although such views are not entirely novel—Stern and Bartlett, among others, have taken up a very similar position—Murphy is perhaps the first to work them out in the form of a systematic introductory text. It only for this reason, all teachers of introductory classes in psychology will stand in his debt.

The last section of the book deals with personality in a more explicit way. Here the approach is entirely psychological and draws heavily (although not uncritically) upon simplified psychoanalytical conceptions. Unfortunately, perhaps, there is no suggestion of a more basic biological standpoint and, in consequence, no evidence from clinical neurology or neuropsychiatry is adduced. Some attention, however, is given to such subjects as attitude-measurement, projective techniques, and the work of Sheldon. In the last connection, the author has some shrewd and timely remarks about typologies which deserve careful pondering by those of statistical bent. Finally, the place of the individual in society and kindred broad social problems are briefly considered. Here, Murphy draws heavily upon the work of recent social anthropologists, such as Mead and Kardiner, who have given special attention to the interplay between personality and culture. It is a pity, perhaps, that no mention is made of more solid, if less exciting, social factors such as kinship-structure and linguistic systems, which evidently play so large a part in the organization of human societies. Nevertheless, these chapters are entirely adequate as a first introduction to social psychology and will do very much more good than harm.

Murphy's *Introduction to Psychology* will be widely read and generally admired. In the reviewer's opinion, its principal value lies first, in the endeavour to integrate biological, clinical, and social approaches to the study of behavior; secondly, in its



author's ungrudging recognition of the higher, more complex, and less easily accessible aspects of human personality; and thirdly, in the very real feeling for people which animates almost every page of the book. In an age of 'human engineering,' it is not a little encouraging to find a psychology of, and for, that 'common man' whose century this is supposed to be.

University of Oxford

O. L. ZANGWILL

*Rotgrünblindheit als Erlebnis: Ein Führer durch die Farbwelt für Rot-Grün Blinde.* By HEINZ AHLENTIEL. Göttingen, "Meisterschmidt," Wissenschaftlicher Verlag, 1951. Pp. 47.

This monograph, though brief, is of unique interest, offering as it does the first detailed and intelligible account of the color world of the dichromat since John Dalton broke ground in 1796, and Dr. William Pole followed in 1859-96. Prepared by a Mainz physician under advisement with various colleagues, the book is designed as a guide for the red-green blind, and addressed to an estimated million of German speaking color-deviates by one of their own number.

Choice is made of the Ostwald color system (not dissimilar in plan to the Munsell), with its 24-hue circle, its cross-sectional diagrams of the color solid (a double cone), its numerical and literal symbols, as best fitted to coördinate and elucidate the dissimilar worlds of the normal and the deviate-visioned. The author eschews theory, but like many workers in this field, he finds Hering's stress on complementary opposites more useful than the Helmholtzian elaboration of the color-mixing triangle.

Bound into the volume is a plate showing the 24 Ostwald hues on half-inch highly saturated matt-paper swatches arranged in a circle and numbered from lemon yellow, (1)—close to, Munsell 5Y,—through the reds (6, 7 and 8), the purples (9 to 11) to U-Blau (indigo) at 14, opposite the yellow. The critical greenish blues and blue-greens run from 19 to 22, across from the equally significant reds and purples.

A double-armed, pivoted, movable gray pointer permits the color blind reader, by setting it on his twin neutral points (red 7 and greenish blue 19 or 20, or red-purple 8 and blue-green 21 or 22 to determine his own type—'red-blind,' with darkening of reds and shift of brightness from the yellow region into the greens; or 'green-blind,' with normal brightness distribution throughout the spectrum or a slight darkening in the green.

Ahlenstiel identifies himself with the first named type agreeing with Dalton and Pole as to the entire absence of the qualities of red and green, with retention of blue and yellow only. The dichromat's preception of color, he believes, is limited to shades, tints, and saturations of the two latter hues, a fact illustrated graphically on the inner sectors of his Ostwald color-circle. The highly technical and theoretical terms *protanopia* and *deutanopia* of Von Kries are carefully avoided. The older 'red-blindness' and 'green-blindness' are used merely to indicate lowered brightness sensitivity and a shift of brightness in the spectrum. Discussion of red-green weakness is excluded, this topic having been treated earlier in *Natur und Volk* (1948-49). Blue-yellow anomalies also are ignored.

In 24 short sections he then proceeds to organize the perceptual world of his color-restricted readers, using a single vertical cross-section (through blue and yellow) of the 24-leaved double-cone color-solid of Ostwald. A double-triangle, hinged on a



vertical gray axis, with saturated blue and yellow at the right and left apices, the figure is crosshatched into 54 two-lettered compartments to represent the intermediate tints, shades, and saturations of the two hues. Heavier outlines enclose certain of the 27 'boxes' seen as yellow by the dichromat, but sometimes reported by the normal-visioned as 'greens' or 'browns' to the bewilderment of the former. Confusion stems also from the fact that some of the color-deviate's best *blues* (indicated through use of the two-letter symbols of the Ostwald diagram) are declared by the normal to be *violet*, a related but quite different quality.

An auxiliary diagram (after Wolfel) depicts graphically the proportion of white and black which must be added to blue or yellow, in order to match each of the 56 small divisions of the plane to which the color solid of the normal is reduced for the blue-yellow visioned. Each of these divisions carries its identifying 2-letter symbol, *pa*, *pc*, and so forth, for use in the discussion. To aid the dichromat further in translating the hues of the Ostwald circle into terms of his own experience, a table of color names derived from familiar fruits, flowers, and natural objects is correlated with the numerals 1 to 24.

After location of a pair of neutral points for each of the two common types of deficiency, and noting the swing clockwise around the circle in passing from one type (the author's) to the other (a swing paralleled, he states, when the first type passes from daylight to artificial illumination), the discussion turns to typical color confusions, such as red or purple with gray, red with yellow and the like, and the part these confusions play, along with the absence of red and green, in everyday experience. A special section is devoted to the handling of pigments in art by the dichromat, about which Ahlenstiel is none too optimistic, since no filter can really restore his vision to normal.

On the whole, the observations reported in the text are not out of harmony with laboratory observations covering a wide range of color deviation made by the reviewer and others—this in spite of the fact that the author's case is presumably one of extreme shortening of the spectrum in the long wave end, to judge by his location of his red neutral point on Ostwald's No. 7 or 'raspberry,' which on the color plate is tomato red (Munsell 5R). In ordinary daylight No 7 carries no trace of the blue tone, needed apparently to neutralize the 'yellow valence' of red rays for the average scoterythrous or 'red-blind' and to a lesser degree for the 'green-blind.' Pointing to the same conclusion is his choice for his second neutral point of a complementary across the color circle in the blues, showing no tinge of green. His shift of brightness in the solar spectrum from yellow into the greens, he states, is such that in twilight hardly any of the Purkinje change occurs. With respect both to neutral points and brightness-shift, however, he maintains that in passing from daylight to artificial illumination his type changes from 'red-blind' to 'green-blind,' which may enable him to speak for both types. It is possible, of course, that the reproduction of his Ostwald may be misleading. The two sizable color swatches on his book's jacket, it may be noted, are nearer the red-purple and bluish-green neutral points of the average 'green-blind' than those he selects on his color plate for this type. His color plate, although limited to 20 hue gradients, is, however, remarkable for its accuracy and high chroma.

The common conviction of the color-blind that they possess a unique quality in the red, distinct from yellow, Ahlenstiel believes illusory, turning in part on the vivid-



ness (or *Reinheit*) of certain orange-red tones in the spectrum—a reference apparently to the strong yellow 'valence' of many long-wave stimuli. As a matter of fact, the possibility that the most saturated yellows for the deviate actually lie in the orange-red region where the extreme brightness component that thins out the yellow hues from 570 to 590 mμ is lacking (along with the red-response)—conditions favoring a high-chroma yellow—has long appealed to this reviewer. Similarly, the most saturated blues may extend into the violets, due to the elimination there of the dark gray component in the scoterythrous.

A second ground for the illusion of a unique red, we are told, lies in the uneven pigmentation of colored surfaces, resulting in a heterogeneous perception of a mottled yellow, gray, and blue (possibly an obscure contrast phenomenon). The red illusion disappears, Ahlenstiel maintains, with spectral or direct light-sources.

In conclusion, there is no question but that the author's unique development of his topic is of both practical and theoretical significance. The book should not only smooth the path of the dichromat, but afford the average reader a glimpse into a No-Man's Land of simplified and primitive perceptions, and enlighten physician, educator, and vocational counsellor as to the peculiar handicaps of a sizable fraction of the community. In addition, it highlights a number of points which physical, physiological, and psychological theory would do well to note. Some of the author's findings suggest the view that variant color vision may involve not merely reduction of the number of kinds of receptor, but the sliding along the physical spectrum of the apices of all the color sensitivity curves with or without attendant shift in brightness acuity. Whether variable pigmentation would account for all of this or not is doubtful. In any case, the author's observations and conclusions point up the complexity of the task confronting the would-be designer of a single all-purpose screening test for eliminating 'dangerously' anomalous cases.

Even the run-of-the-mill psychologist will find in the book a mine of material bearing on the perceptual problems of "the 13th or 20th man out of every hundred" (according to Ahlenstiel), who must base his judgements of size, distance, shape, and substance on a fraction of the clues available to the normal visioned whose color-discriminations range into hundreds of thousands instead of merely hundreds. The usefulness of a translation suggests itself. In view, however, of the misconceptions launched by careless rendering of even such authorities as Helmholtz, a better plan might be to induce an English-speaking dichromat, with or without shortening of the spectrum in the red, to undertake a similar detailed account of his color world, with as apt concrete illustrations. Better still, a group representing various degrees of deviation might collaborate, pool their findings, and publish a like systematic account of their perceptions, along with their test-ratings in standard measures. In any case, credit is due to Ahlenstiel for pointing the way and for turning up new problems for both theorist and experimenter.

Cornell University

ELSIE MURRAY

*Behavior Pathology*. By NORMAN CAMERON and ANN MAGARET. Boston, Houghton Mifflin Company, 1951. Pp. xvi, 645.

As Dr. Leonard Carmichael says in the Editor's Introduction, the present volume is a supplementation of Dr. Cameron's previous work *The Psychology of Behavior Disorders* (cf. 61, 1948, 598). In the earlier book "principal emphasis was given to



the modern understanding of the major varieties of mental illness. In the present book the basic emphasis is upon an understanding of the patient as an individual who has a given hereditary make-up and a unique history of fortunate and unfortunate social learning" (p. vi). Similarly, the authors in their preface point out that their book will begin with "the conception of the human being as a biological organism, with hereditary and constitutional background operating as one component of a dynamic social environment," and that the development of behavior pathology will be presented as "essentially a process of maturation and learning in such a setting" (p. xv).

In pursuance of these aims the authors take up in detail such topics as need, stress, frustration, learning, symbolizing, role-taking, emotional reactions, maturation and fixation, regression, conflict, anxiety, repression, pseudocommunity and delusion, autistic community and hallucinations, disorganization, desocialization and deterioration. In addition there are chapters dealing with developmental retardation, social deviation (psychopathic personality), the several neuroses and functional psychoses, certain of the organic psychoses, and various types of therapy. The conventional syndromes are not, however, presented for their own sake, but rather to exemplify the authors' behavioral concepts. Thus, depression is discussed under the heading "Anxiety in behavior pathology," and schizophrenic disorders under "Desocialization."

As those familiar with his work will be aware, Cameron's position in psychology and psychiatry has several outstanding characteristics. One of these is a consistently behavioristic viewpoint, in which virtually every activity of a human being is subsumed under the category of reaction. Thus anxiety is defined as "the predominantly covert skeletal and visceral reaction which constitutes the unconsummated preliminary phase of emotional excitement" (p. 278), and insight is said to be "the occurrence of a reaction or a system of reactions, which precipitates a sudden and comparatively stable behavioral organization" (p. 84). A motive is any factor "of special significance in instigating and sustaining a given need-satisfaction sequence," and moreover is only "a product of behavior analysis . . . a construct of our own making" (p. 34). Hallucination is classified as "a response" (p. 426), and the word "perception" does not appear in the index. Carmichael says that the book "avoids in any form the dualistic approach to mental illness" (p. v), and that the authors never pretend to explain anything as resulting from hypothetical forces. These statements are entirely correct; the authors' preoccupation with R in the conventional S-O-R formula leads to the omission of almost all reference to so-called central processes. Throughout the book exposition is kept so far as possible at the level of what people are doing, and intervening variables of the psychoanalytic or Gestalt types—such as instinct, ego, superego, vector, demand, insight, expectancy, hypotheses—remain unutilized.

Another major characteristic of Cameron's viewpoint is his extremely environmentalist position and his constant stress on what are commonly termed functional rather than organic factors as causative of behavior disorder. In spite of the implication in the introduction and the preface that hereditary make-up and constitutional background are regarded as contributing significantly to the understanding of behavioral disturbances, the reader soon discovers that virtually all of the factors to which the authors ascribe much significance are environmental and functional. Thus,



Kallmann's conclusions concerning the importance of genetic factors in the production of schizophrenia are rejected as unproved, and Pastore's well-known criticisms of them are cited with approval. Studies of blood-chemistry and blood-type are dismissed as yielding no clearly interpretable results, and Sheldon's body-type hypotheses are said to leave unanswered the crucial question as to the "interplay between constitution and social learning" (p. 28). Cameron's views (detailed in his earlier volume) as to the relation of actual brain injuries to behavioral disorders are reiterated, and the behavioral changes following cortical damage are attributed largely to the reactions of the individual himself to his diminished status. The authors regard virtually all investigations of constitutional and physiological factors as having yielded results which are either negative or ambiguous. In fact they even express surprise—in view of the meager positive findings so far obtained—that so much research in behavior disorder has concentrated upon hereditary, prenatal, and physiological factors, to the comparative neglect of "the decades of social learning" (p. 31) which all adults have experienced.

A third outstanding characteristic of the book is the authors' emphasis upon learning, more specifically upon learning in social situations. To them, behavioral disorder "is the end-result of progressively inappropriate and unrewarding social learning" (p. 10). Physiological and constitutional factors, *e.g.* those apparently responsible for differences in the responsiveness of infants to stimulation, seem to be important only because they may influence learning. One gets the impression that whether an individual is normal or abnormal depends almost wholly upon the kind of techniques he has acquired to overcome anxiety, resolve conflicts, and the like. The authors devote a section of 35 pages to a description of how this learning takes place. With respect to learning theories, they seem to favor a two-factor theory similar to that of Mowrer, but without attempting to distinguish between the acquisition of reactions mediated by the autonomic and the cerebro-spinal nervous systems. Human reactions acquire new excitants because of "temporal concomitance" (p. 69), a conception which, the authors remark, appears in learning theories as the law of contiguity, associative shifting, or reintegration. Human beings also are said to learn in accordance with their needs, *i.e.* they show selective learning. In this connection, reactions which eventuate in tension-reduction are characterized as especially likely to be learned, and—in agreement with contemporary emphases—anxiety is stipulated as one of the most important types or sources of tension. 'Insightful' learning also is described, but defined in the fashion already noted.

These three general characteristics of the position presented by Cameron and Magaret indicate fairly clearly which psychologists will like the book and which ones may not. The non-behaviorist, whether he is an adherent of psychoanalytic principles or of Gestalt and related phenomenalist interpretations, will regard the position as constituting a grossly over-simplified account of human behavior; the specialist who is genetically minded will scarcely approve of the almost exclusively environmentalist orientation; and belligerent advocates of certain contemporary learning theories will not relish the authors' eclecticism in this area, especially their unparsimonious preference for two factors rather than one.

The reader of this book is inevitably reminded of the other two major attempts to describe the nature and development of mental disorders and the processes of psychotherapy in terms of systematic psychological theory: namely, Guthrie's



*Psychology of Human Conflict* and Dollard and Miller's *Personality and Psychotherapy*. It is unnecessary to venture any comparative evaluation of these three works. Each provides an application of S-R theory to the understanding of mental disorders by a psychologist of the first rank; each abounds in original and felicitous ideas and turns of thought; each deserves a place in the library of every psychologist. It may not be amiss, however, to report that the reviewer has noticed this year the spontaneous development of an interest in Cameron's position among some of the graduate students enrolled in a course dealing with theories of abnormal psychology. These students, most of whom hope to become clinical psychologists, are interested to learn about approaches other than the psychoanalytic because they are surfeited with freudian concepts through their reading and their contacts with psychiatrists and others in the clinical field. They are, however, tiring of the continuing polemics in the area of learning theory, and becoming increasingly sceptical of the experimental testability of the rival hypotheses. They therefore turn to Cameron with some sense of relief. Granting his behavioristic bias, they feel that at least he has written a book in which considerations of direct importance to the clinician have consistent priority over those which concern the application of a particular learning theory. Nevertheless, many of these students regret the absence in the literature of a systematic account of abnormal behavior in functional terms. It remains to be seen whether there is any theorist of this persuasion who will undertake the task of writing a psychology of the abnormal which is neither psychoanalytic on the one hand nor ultra-behavioristic on the other.

New York University

LELAND W. CRAFTS

*Field Theory in Social Science*. By KURT LEWIN. Edited by DORWIN CARTWRIGHT. New York, Harper Bros., 1951. Pp. xx, 346.

This book brings together the more important theoretical writings of Kurt Lewin between 1939 and 1947, the year of his death. The editor has done an excellent job of selecting articles and excerpts and putting them together into a reasonably well integrated series with a minimum of editorial changes. After three initial chapters dealing with the place of theory and the nature of constructs in psychology, and with the field-theoretical approach, there follow six chapters concerned with particular problems of individual and social psychology. Lewin's article on "Behavior and development as a function of the total situation" (published originally in Carmichael's *Manual of Child Psychology*) rounds out the volume. An appendix illustrates Lewin's mathematical analysis of concepts central to his thinking.

In his Foreword, Cartwright describes as the theme of this volume "a thorough and careful answer to the question: What is field theory in social science?" The reader may have difficulty in finding this answer. According to Lewin, "*Field theory is probably best characterized as a method: namely, a method of analyzing causal relations and of building scientific constructs*" (P. 45) Granting that field theory is a method and not a systematic body of constructs and hypotheses pertaining to a certain class of phenomena, it is still reasonable to ask what differentiates this method from others. What, in Lewin's own terms, are its "elements of construction"? In various places Lewin summarized or alluded to presumably distinctive properties of the field-theoretical approach. Whether these characteristics are actually distinctive of field theory the reader may decide. Certainly most psychologists would agree on the



desirability of using constructs or abstractly defined dimensions in psychological theory, and on the requirement that these deal with the underlying forces of behavior. No one would question, when terms are adequately defined, that the factors influencing behavior are factors existing at the present moment. It seems to be fairly generally recognized that behavior is determined by a multitude of influences, and that such influences are commonly interdependent (in a sense which is perhaps defined more precisely by the statistical concept of interaction than by diagrams depicting the "life space"). There would be little argument that factors affecting one person in a situation do not necessarily affect everyone else, or in other words that the particular "life space" of the specific individual or group under study should enter into the analysis of behavior. What might be questioned by most psychologists is whether the objectively describable features of situations (or stimuli) can be ignored in psychology. Actually Lewin did not go so far as to claim that objective stimulus conditions *should* be ignored; he regarded the "life space" as determined both by the individual's history and by the non-psychological—physical and social characteristics of the situation. (These latter were described as "boundary conditions," however, and were effectually ignored, along with the individual history.) Finally, the use of mathematical models should command general support. Questions may arise again, however, as to whether geometric, or more specifically, topological models are the only possible ones, and as to whether these were at all productive as Lewin used them. It would almost seem that Lewin had developed toward these constructions the attitude of the loving father whose favorite son has not quite realized his early promise.

What this reviewer takes to be widespread acceptance among psychologists of the allegedly central features of field theory may only constitute a measure of the influence which Lewin has had. As he himself pointed out, frequently the early critics of a theory turn out later to be among those who knew it all the time. This may, however, all too easily be an argument of the variety *post hoc ergo propter hoc*. A more likely conclusion is that field theory refers to the various methods by which Lewin chose to approach the problems which interested him.

Whatever the status of field theory, this volume provides an eloquent reminder that Kurt Lewin did approach a variety of problems in a stimulating and productive manner. One major focus of his contributions obviously had to do with problems of motivation and the cognitive processes relating to goal-achievement. Particularly noteworthy here were his theoretical analyses and empirical studies of such phenomena as recognition, satiation, level of aspiration, time-perspective, and the so-called Zeigarnik effect. Even the sharpest critic of particular conceptions or experiments must concede that Lewin brought to these problems a fresh and provocative point of view, together with a readiness to tackle even the most complex of phenomena by experimental means. His later concentration upon problems of group behavior was in one sense only a logical extension of his enduring interest in the dynamics of behavior, but for the social sciences in general it is these later activities which will probably be remembered longest. Although laboratory studies of group behavior did not start with Lewin, he was largely responsible for the beginning of an all-out effort to develop a group psychology, utilizing wherever appropriate the techniques of the experimental laboratory. It would seem, however, that here, as in the more specifically individual studies, Lewin's most fruitful contributions were the



concepts and methods which he introduced to deal with particular problems. In very few cases do these ideas show more than superficial earmarks of descent from a fundamental conceptual structure which might be regarded as a general theory.

For the average psychologist trained in the American tradition, Lewin's rather casual attitude toward the problems of learning is inevitably disturbing. His article on the subject in the present volume might almost be regarded as an "Aristotelian" classification of different kinds of behavioral change, showing little real concern for the conditions under which persisting changes in an individual's potentialities take place. Avoidance of the problem is also evident—and equally incongruous—in his final chapter on development. His assertion that the conditions of behavior and of development are essentially the same does not really solve this problem, and merely leads, where he deals with questions of long term development, to a purely descriptive account which seems quite uncharacteristic of Lewin. This curious blind spot evidently derives in part from the Gestalt reaction to the excesses of early associationism. In part also it seems to follow from a lack of concern for the objective features of stimulus-situations which necessarily figure in an adequate theory of learning. Perhaps more fundamentally it expresses Lewin's evident conviction that social change for the better should always be, and hence always is, possible. Accordingly, firm habits which may obstruct desirable change should not be, and hence are not, worthy of serious consideration. It is, however, gratuitous to cavil at blind spots where other contributions are so rich. For that matter, it is possible that Lewin's emphasis on the changeability of human beings has called attention to the very real importance in human behavior of many situational factors which would undoubtedly have escaped psychologists preoccupied with problems of learning and personality.

The present volume will prove of real assistance to students of psychological and social theory. Together with the *Dynamic Theory of Personality*, *Principles of Topological Psychology*, and the recent compilation of Lewin's more popular writings, *Resolving Social Conflicts*, it will virtually complete the picture of Lewin's development as a psychologist.

Combat Crew Training Research Laboratory

ROBERT L. FRENCH

*The Prediction of Performance in Clinical Psychology.* By E. LOWELL KELLY and DONALD W. FISKE. Ann Arbor, Michigan, University of Michigan Press, 1951. Pp. viii, 311.

The most useful thing a reviewer can do in connection with this volume is to give some advice on how to read it. For reasons which will be suggested later, the going is hard, and the reader needs every aid he can get if he is not to give up before he has absorbed the full implication and impact of the contents.

The advice, then, is this. Read Chapter E-I, "Summary and discussion," first. There are few psychologists who will not find in these ten pages some useful and provocative facts and ideas. Next, leaf through the book, reading the chapter summaries which the authors have thoughtfully prepared and placed at the beginning of each chapter. Then single out specific chapters according to specific interests. Finally, consider the 106 pages of Appendix which contain more or less relevant details.



From earlier reports, most readers will know something about the project which this volume describes. In the authors' words: "The primary purpose of this five-year research was the evaluation of a variety of procedures as predictors of later success in graduate training and professional functioning in clinical psychology. The overall design of the project was as follows: in 1947 and 1948, several hundred college graduates seeking admission to or just entering the four-year [Veterans Administration] training program in clinical psychology in some 40 universities were evaluated by a wide variety of techniques, and predictions were made concerning their probable success in training and their future professional competence. . . . The second half of the project was devoted to (a) the development of criterion measures of the several functions which clinical psychologists are expected to perform in VA installations, (b) the administration of these measures to trainees near the end of the four-year training program, and (c) the analysis of the interrelationships among the predictor and criterion measures (p. 193)."

It would be a mistake, however, to suppose that this project was conceived or carried out from a narrow 'applied' point of view. As the authors say, quite justifiably, in the Preface: "It is believed . . . that both the methods of investigation and the resulting findings will be of interest to those concerned with the selection of other groups of professional students. And, although the primary object of the project was that of evaluating selection procedures, we believe that many of the findings will also be of interest to psychologists not immediately concerned with the problem of selection (p. v)."

Somewhat paradoxically, this study acquires its greatest importance from the same circumstances that make it difficult and, on the whole, uninteresting reading: the findings are essentially negative. For example, there is a table on p. 70 giving "Correlations between objective test-scores and final ratings on overall suitability." In this table there are 67 correlations with a range from  $+.36$  to  $-.23$  and a central tendency near zero. Now correlations of this order do not generate enthusiasm, and the incentive for reading about them is not very great. These findings have, nonetheless, a sobering significance: they raise, acutely, the question—Just how meaningful and valid are the tests and test-procedures commonly employed for purposes of personality assessment and prediction? Especially sobering is the finding that the widely popular projective techniques seem to be virtually worthless in this connection. It can be and has been said, of course, that the projective tests are not designed for precisely the uses to which they were put in this investigation, but growing evidence from other sources shows generally negative results on the score of validation. Certain other testing procedures stand up somewhat better in the present study; but the overall picture is not a pretty or reassuring one.

Nor are we comforted by the findings of this investigation on the score of validating criteria. The authors say: "The potential validity of all predictors is apparently limited by inadequacies in available criterion measures. All criterion measures used and considered are more or less fallible because of one or more . . . characteristics (p. 195)."

The fiasco is thus a double one: In plain terms, not only can we not predict who will *become* the best clinical psychologist; we cannot even decide very confidently who *is* good and who not-so-good in this profession. But how could it be otherwise? If clinical psychologists, at least insofar as their functioning involves person-



ality diagnosis and recommendation, are using instruments of questionable value, how can we say who is using them 'well' and who 'poorly'?

There are, however, two counts on which psychologists can take some satisfaction:

(1) The present volume well represents the tradition and reputation which psychologists have for being self-searching and critical. The investigation which is here described may involve errors of detail or perhaps even false assumptions or inferences, but it is honest to the point of bluntness. Perhaps one of the best arguments for keeping the more strictly professional and scientific aspects of psychology together as one discipline and one scientific-professional organization is protection thus afforded against the continued application of unvalidated psychological procedures just because there is a social need or market for such procedures.

(2) A consideration which, in the reviewer's opinion, the authors do not sufficiently stress (see pp. 130-135) is this: The whole notion of 'diagnosis and prediction' is predicated on the assumption that human personality is essentially fixed; whereas we know that personality is always to some extent a function of the 'situation,' and psychologists, above all others, ought also to be impressed by the human potentialities for change which are made possible through learning. Kelly and Fiske remark that roughly one third of their subjects had some form of personal therapy during their graduate careers. Either we do not believe in the possibility of radical personal change through therapy *or*, if we do, we should not expect 'predictions' regarding a population receiving therapy to be highly reliable.

Many other features of this volume invite comment, but enough has been said to indicate that it must be read and pondered by every psychologist who purports to be responsible and informed in the clinical and personnel area. One possible misapprehension should be avoided. The reviewer's assertion that the book is indisputably dull should not be interpreted as implying that it is poorly written. Quite the contrary: the book is a model of clear, compact scientific exposition. The 'typesetting' (typing) is impeccable, and the lithoprinting and book-making are excellent.

University of Illinois

O. H. MOWRER

*World Tensions: The Psychopathology of International Relations.* Edited by GEORGE W. KISKER. New York, Prentice-Hall, 1951. Pp. x, 324.

Reviewing a volume prepared by 22 authors is necessarily confusing, since the quality of contributions inevitably varies so widely. There is a unique justification, however, for the format in this instance; each author presents the problem of world tensions as seen from the viewpoint of his particular nation (except that Otto Klineberg speaks—much too briefly—for the United Nations.) This is a very sound idea, since a major essential of a psychological approach to this problem is recognition of the dependence of facts upon the observer's attitude. The point is only too well demonstrated in naive expressions of national pride and prejudice from Australia, Ireland, and Spain. Unfortunately, only two authors attempt to deal constructively with the problem. Hesnard (France) and G. Murphy (USA) take up the question: How do the facts look to leaders of the USA and to leaders of the USSR?

The recurring theme of the volume is the rôle of aggressive nationalism as the major factor in world tension, which is variously ascribed to collective egotism (Austregesilo, Conrad), to the "death instinct" (Jones) and to mass psychosis more



or less explicitly defined. Many of the authors are extraordinarily fuzzy about the relation of individual to group, and use psychopathological concepts as if they could be applied unmodified to social phenomena. The shadow of Gustave LeBon is dark across the pages; one wonders if most psychiatrists (who use group-mind concepts in this volume deplorably often) are completely ignorant of twentieth-century developments in social psychology.

The importance of leadership is coming to be more explicitly recognized. Stevenson, Perrotti, Conrad, Forel, and Murphy have pointed remarks to make on the problem, and it is mentioned in other chapters. The rôle of the group in selecting emotionally suitable leaders, and of the leader in voicing repressed or suppressed desires of followers, must be understood. "The demagogue then becomes the loud-speaker of the collective unconscious" (p. 259). (I would object to the adjective "collective" in this context and many similar passages. Nowhere in the book is the concept of a shared frame of reference mentioned, and very little attention is given to group-uniformity based on common training).

Far too much attention is paid to the Freudian theory of the father-image, and too little to the adult problem of the symbolism by which the presumptive father-image gets related to specific governmental institutions. Bostock and Notcutt have some intelligent comments on this point; Notcutt in particular does an able job of relating individual psychology to social psychology, and the "implicit hypotheses of the culture" to early or repressed memories. Perrotti offers a few striking cases to illustrate the thesis that political activity finds its dynamic basis in personal (unconscious) strivings. Perrotti, incidentally, is the only one of the authors (according to the biographical data given) to have been elected to national office. This, I think, reflects badly on our profession. Do we really expect the political leaders to come begging us for assistance, or shall we take our convictions out of the ivory tower into the real world of politics? If we shirk this latter course, I think we are as guilty of irresponsibility as the common man who is flayed so often in this volume for his lack of effort to improve international relations.

On the positive side not many solid suggestions were offered. Perhaps we could demand that political leaders have a certificate as to their emotional maturity (Stevenson), but who would enforce the demand? The same problem arises with the suggestion that top statesmen operate with a board of psychological advisers (Zeylmans van Emmichoven). Various authors noted that anything aiding mental health is desirable, since emotionally stable citizens will be less prone to choose neurotically aggressive leaders, or to project infantile aggressions onto foreign countries. Almost all authors who cleanly stated the problem concluded that the only hope of reducing world tensions below the danger point lay in the establishment of an international organization to which some "ego" (national sovereignty) can be ceded—this at a time when patriotic groups are demanding withdrawal of the USA from the UN! Bostock gives an interesting analysis of this problem and cites the Australian aborigines as evidence that it can be solved. Conrad likewise concludes that the individual can reduce tension by "ceding" some of his ego, *i.e.* compromising with his opponent; for some obscure reason, he holds that a group cannot achieve the same solution. It seems clear that groups can, although often they do not.

Gardner Murphy, with his usual interest in research, suggests that if we can "stall for time" in the cold war, social research may help us better to understand



the Russians, and help them to understand us. With our State Department and the Soviet government both actively engaged in sealing the frontiers, this will be difficult, albeit not impossible. Likewise Murphy notes the desirability of making intensive studies of our own political, economic, and military leaders; here, too, cooperation may be something less than psychologists will wish. The problem is so desperate however, that every meager clue should be pursued with painstaking care.

It is deplorable that Kisker's concluding chapter does not point up these and other positive suggestions for research and policy formation. His contribution as editor seems to have been to quote liberally both from the essays in the volume and from a large number of other experts, on all sides of all the questions he raises, thus confusing even more the already somewhat bewildered reader. Despite these critical remarks, I hope this volume gets wide attention, and that it stimulates more psychologists and psychiatrists toward research and writing in this vital area. It was impressive to see how much agreement existed with the conclusion of the SPSSI Committee on the Psychology of War and Peace, which worked from 1937 to 1941, and which should be set up anew in some form.

I left the volume with one thought nagging at me. Almost every chapter emphasized the irrationality of the crowd—the loss of individual critical faculties, the polarized nature of crowd judgments, inability to solve problems, and so on. Now it happens that experiments show conclusively that groups can solve problems—usually, more efficiently than individuals—but where, in western civilization or in the world, do people in groups practice dealing with rational problems? In our public schools, colleges, universities, do we give training in cooperative problem-solving? In factories, in the army, men cooperate under orders but they do not work out rational solutions in groups. Must crowds be irrational? If we started with children, and gave rewards for intellectual cooperation within the group, would groups show a problem-centered approach to adult crisis situations? Bostock says of American policy toward Russia, "The countermeasure is not grotesque super-rockets or incredible radar, but mass psychology." I submit that the countermeasure may be the development of group skills in thinking, not just in playing and in fighting.

University of Illinois

ROSS STAGNER

*La Strutturazione Psicologica Del Linguaggio Studiata Mediante L'Analisi Elettroacustica.* By AGOSTINO GEMELLI. Vatican City, *Pontificias Academiae Scientiarum Scripta Varia*, 1950. Pp. 1-53.

The author, an experimental psychologist well known for his researches in phonetics and linguistics, examines what he calls the "psychological structuralization of language"—organization of forms essentially acoustic that must be embodied in a spoken language if it is to be understood. He assumes that these unities and organizations, the play of which constitutes language, correspond to similar unities and organizations within the person who perceives the language. They are independent of individual peculiarities and variations of utterance, and their absence results in a breakdown of communication. It is the task of the psychologist to identify and to define these unities.

A considerable portion of the monograph is devoted to the demonstration of variations and similarities in samples of recorded speech obtained from a group of speakers. Chapter headings indicate the trend of the author's explorations: the



variations of vowels in spoken language; individual differences in language and the limits of these variations; the variations of tonal accent; the variations of intensity; the significance of expressive qualities; the structuralization of words and phrases. Gemelli's method is to record speech samples simultaneously by means of the cathode ray oscillograph with high film speed and by electromagnetic (wire) recorder. The acoustic records are replayed and analyzed by a specially designed spectrograph (tonometer) and by a recording voltmeter. The oscillographic records are studied to determine the acoustic structure of the different phonemes. Thus, the investigator is able to determine the harmonic structure of speech, the distribution of the zones of resonance, and the variations of frequency and intensity. Speech samples obtained from a group of individuals enable him to study individual similarities and differences.

These quantitative measurements constitute the basic data of the study. They require the conclusion that speech is a stream of continuously varying sounds with few or no clearly defined divisions between words or even between phonemes. Variations among different samples are, however, clearly evident. They are due to such factors as (a) the influence which the preceding and following sounds have upon vowels, (b) the accentuation and frequency, and the intensity with which the sample is uttered, (c) the contextual meaning which the words convey aside from their lexicographic meaning, as in interrogatory or exclamatory phrases, and (d) variations induced by emotional states of the speaker. Gemelli holds that it is the task of the psychologist to determine the meaning of these variations and their bearing upon the structure of language. The psychologist, he says, is concerned with determining the correlations existing between variations in linguistic forms and the psychic states of which they are the expression. The physicist, the linguist, the phonetician, and the phonologist will study the same variations from other points of view.

Confining his analyses to electro-acoustic records, Gemelli is struck by the fact that speech on an oscillogram appears as a continuous sequence of vibrations; phonemes, syllables, and words, are lost in this "sonorous current." The only obvious units that appear on the records are the individual cycles. Yet he is sure that larger unities are present because they can be heard in the acoustic recordings. The transitions from one to another are obscured in the visual record. Gemelli fails to take into account the fact that the single-line oscillogram, which transforms speech into visual terms, presents to the eye a totally different set of sensory data from that presented to the ear by the acoustic form of speech. The physical transducers which the author employs with such skill in making his recordings are not matched by corresponding perceptual, or psychological, transducers when he comes to study the records. Visual perception can never achieve the functions of temporal fusions, pitch and loudness changes, and so forth, which are commonplace in auditory perception. In other words it is impossible to transform an acoustic pattern into graphic form in such a manner that the resultant visual experience will have the same psychological dimensions as the original auditory experience. The skilled lip reader perceives speech visually, but there is little likelihood that the rapid movements of speech are perceived in acoustic terms. The exception may be an adult who has been losing his hearing gradually over a prolonged period, in which case the eye gradually assumes the function of the ear.

Speech has two essential aspects, acoustic and motor, *product* and *process*. It



must ordinarily be perceived by the listener in terms of both, otherwise it is difficult to see how anyone learns to speak by ear. In the early stages of learning the ear receives a series of arbitrary acoustic signals which gradually become symbols for the motor processes. In trial-and-error fashion the hearer learns to imitate by virtue of certain patterns of movement. This is as true of the small child learning his first words as it is of the adult learning to speak a foreign language. These considerations suggest that an adequate analysis of speech must include both its acoustic and its motor aspects. The electro-acoustical method of analysis, taken alone, can hardly present a complete picture of spoken language.

Clarke School for the Deaf

C. V. HUDGINS

*Annual Review of Psychology, Volume 3.* Edited by CALVIN P. STONE and DONALD W. TAYLOR. Stanford, Annual Reviews, 1952. Pp. 462.

The foremost impression left with this reader of the *Annual Review* is one of bewilderment at the tremendous range of problems actively pursued by psychologists today. The studies summarized run the gamut from painstaking measurements of sensory processes and muscle movements to broad analyses of culture and personality. Although we are all well aware of the ever-widening spread and proliferation of psychological problems and methods, it is a sobering experience to be confronted with a representative sample between the covers of one book. The experience is sobering in two ways. First, it underscores the fact that it is no longer possible to be a good, all-around psychologist; the multiplicity of specialties forces specialization on one and all. Second, this reviewer, at least, was reinforced in his uneasiness about psychology's spreading itself dangerously thin. Our methods and skills lag far behind our interests, curiosities and aspirations.

The *Annual Review* performs a remarkable service in bringing order and organization to a vast variety of material. Many of the reviewers do not limit themselves to summarizing the studies in their specific area but also point out relations and connections with other areas of psychology and neighboring disciplines. Thus, it becomes possible to focus on some concepts, methods, and interests which cut across the specialties. Several general trends become apparent. (1) In many of the special fields there is concern about theoretical continuity with general psychology. We note, for example, the repeated references to learning theory and perceptual theory in many chapters, e.g. in the summaries of child psychology, social psychology, personality, psychodiagnostics, and psychotherapy. (2) Most of the reviewers tend to de-emphasize the distinction between pure and applied research. In the appraisals of applied research, the implications of the findings for general principles of behavior and methodology are often stressed. (3) Investigators in many fields, particularly in the non-experimental areas, are devoting much effort to the development and refinement of statistical techniques. Interest in correlational methods and factor analysis continues to be high.

In spite of concern with such common problems, the contributors vary considerably in their approach to the task of writing a critical summary of the year's work in a given area. The chapters can almost be ordered along a continuum of systematic selectivity. At one end of this continuum we would place Mowrer's chapter on motivation which is built around the author's theoretical position with citations which are not necessarily limited to the year covered by the *Review*. The other end of the



continuum is probably represented by Elmgren's summary of educational psychology which consists largely of a string of abstracts following each other in a none too systematic order, and certainly without any evidence of selection by the author. Most of the other contributions strike a balance between some selective stress on particular problems, concepts, or methods and extensive documentation. By and large, such variability of treatment adds to the readability of the book. There is a place for both systematic critiques and bibliographical summaries. Wherever it is necessary to choose between the two, however, this reader would vote for more critique and less bibliography. Certainly the non-specialist reader is in danger of being snowed under by the sheer weight of facts unless the reviewer helps him by sifting, selecting, and judiciously criticizing.

The *Annual Review* makes an important contribution to the organization and evaluation of psychological data. The high level of scholarship shown in these pages augurs well for the continuing success of this work.

University of California.

LEO POSTMAN

*The Psychology of Adolescence.* By ALEXANDER A. SCHNEIDERS. Milwaukee, Bruce Publishing Company, 1951. Pp. xii, 550.

For most modern American psychologists Schneiders' *Psychology of Adolescence* would undoubtedly make truly remarkable reading. The comment that "Practically, there is no such thing as a determinist or determinism, unless the term is reserved for those unfortunate individuals who, through depravity or mental disorder, can no longer control their impulses and conduct" (p. 47, note) was the first one to raise this reviewer's eyebrows. Throughout the entire book Schneiders makes constant reference to "morality," "authority," the "total mind-body entity," and in time the reader realizes that Schneiders has written a book that attempts not only to give the facts and theories concerning adolescence, but also to present them in such a way as to direct adolescent readers in a very particular way—the way of the Roman Catholic Church. Perhaps it is better to say that the text misses no opportunity to mention the position of the Church in relation to any empirical finding likely to have philosophical or moral implications. The author quotes with approval Kahn's opinion that "Woman's chief biologic function is the bearing and raising of children, while man is destined by nature for intellectual activities" and while agreeing that masturbation is not statistically abnormal he mentions in a footnote that it is "certainly abnormal in the sense that it is perversion, and leads to maladjustment." (p. 204).

This reviewer happens to disagree with the general philosophical position taken by the Church as well as with many specific corollaries of the position, but he sees no point in debating these issues here. It is only fair to mention that, given the premises and the task he sets out to accomplish, Schneiders has done an excellent job. He wrestles with the mind-body problem, the question of free will, Freudianism, sexuality in adolescence, moral and religious development, instinct doctrine, and the uses of punishment. No issue of major importance to adolescents or to adolescent psychology is missed, and there is a certain amount of discussion of all of them—not merely pronouncements from on high.

Schneiders' book includes a vast amount of empirical material. It is a true psychological textbook, and not merely a religious tract or apology. The results of



basic research are well reported (though Kinsey is conspicuously missing), and, in addition, a great deal of work with which this reviewer is not familiar also is described. Much of the latter is drawn from Catholic sources—theses from Catholic colleges and universities, articles in the *Journal of Religious Instruction* and the *Catholic Educational Review*, and so on. Unlike many writers of textbooks in this area, Schneiders manages to pull all this material together in a way that it is easy to read, easy to remember, and consistently related to a single overall point of view. Students in Catholic colleges will undoubtedly like this text, and will learn much from it. Perhaps the completeness of the integration will prevent their being challenged to ask further questions, but doubtless this will not disturb Professor Schneiders, for he remarks in his chapter on the "Development of intellectual functions" that "fundamental beliefs of a religious, moral and social nature should be protected against immature thinking and inadequate knowledge; the adolescent should be given a chance to mature fully in the intellectual sense before he undertakes a critical analysis of these basic concepts" (p. 499).

University of California

JOHN P. MCKEE

*Russian Purge and the Extraction of Confession.* By F. BECK and W. GODIN. Trans. from German, New York, Viking Press, 1951, Pp. 288.

The authors (announced by pseudonyms) were "a distinguished historian and a German professor of science," fellow prisoners of the Soviets. The book should have an enlightening effect in a land where many of the inhabitants neither intelligently know nor take serious pains to know the character, history, statecraft, and hostile intent of an encroaching enemy, which well uses drastic means to deceive both its own subjects and those individuals, peoples, and nations who oppose its announced plans of domination of the world. On our own side, indifference, prejudice, and stupidity have led many to a state of anarchial contention which encourages at once misjudgment concerning active and dangerous enemies and an objective discrimination of allies and enemies within our national borders. The chief immediate alarm from this confused state appears to be its blind furies and antagonisms which are carried over by political partisans to the responsibilities of government in its affairs with other nations; by partisans who openly repudiate and denounce in the presence of the enemy, policies and actions, sometimes to the point of crying 'treason' and threatening impeachment. Thus do party contention and ambition divide the country upon matters where responsible governmental agencies are required to act, and to act for the nation and not for influential partisans in or out of The Congress. They threaten to do more than divide: they destroy standards of judgment and justice, cripple defenses, aid the enemy, disturb the confidence of powerful friends abroad, and obscure international issues of immediate concern. No single book will restore equilibrium and unification within and a clear view of dangers and duties without; but the little book in my hand (with its delineation of the ghastly Yeshov Purge of 1936-39, the barbaric tools of persecution and torture, the treatment of alleged 'criminals,' and the many reasons alleged for this and other purges)—all of this calm relation of fact and infamy, preferably laid against the broad background of Russian history—may well make American readers reflective upon the exact requirements and responsibilities, as well as upon the benefits, of a free and democratic unit among the nations.

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## MILITARY PSYCHOLOGY: SCIENCE OR TECHNOLOGY?

By FRANK A. GELDARD, University of Virginia

Modern man, viewing the sharp ideational cleavage between Eastern and Western civilizations, witnessing with each passing day fresh outrages against cherished principles of humanity and freedom, and contemplating the unrelenting threat of new and awesome methods of waging total war, finds his prospect an anxious one. At best, in most deliberative mood, his outlook is one of grave concern for the future. Faced with a real world the most ominous in recorded history what is he to do with that fancied one, the world of plans and projects? Should his theme be 'business as usual' or does the brave new world bring with it novel guides to human action? Are the patterns of life planning more apparent in the atomic age than they were in the old molecular one?

It would be a brash person who would attempt an answer of any degree of generality; but for some particular human concerns answers are at hand. The physicist, advised to plan his professional career as though nuclear fission and fusion could occur only in the realm of imagination, might be pardoned if he expressed some doubt about the sanity of his adviser. The communications engineer, adjusting his life to a future devoid of transistors and beamed energy, could reasonably be judged to be on the schizoid side. And so for the organic chemist, the epidemiologist, or the oil geologist were they similarly to fly in the face of modern technical advances. So, perhaps, at least in some life relations, for the butcher, the baker, the candlestick maker. The world has been transformed. However nostalgic we may be for the relatively uncomplicated and serene pre-war days, Los Alamos signalled not alone the advent of a terrifying tool of destruction but a changed way of life for great segments of the world's population. The American scientific public is one of those segments.

What of scientific psychology? What is the impact of the atomic age

\* An address delivered at the dedication of Mezes Hall at The University of Texas on April 1, 1953, under the title "Scientific psychology in a troubled world." Professor Geldard represented The Southern Society for Philosophy and Psychology which held its 1953 meetings, as part of the dedication, at The University of Texas.



on it? What implications for the future of psychological knowledge and research are there in the current East-West impasse with its mounting tensions?

The first thing we must recognize is that scientific psychology, with only four-score years of history behind it, has much to contribute to the Nation in its current predicament. In some quarters there is clear expression of this fact. One need only point to the heartening post-war development of psychological research and services in government laboratories and hospitals, in field stations of the Armed Forces—more than a fair share of them in public-spirited Texas—and to the widespread initiation of federally sponsored psychological research in universities, colleges, and non-academic institutions. Research contracts in force during 1952 between such agencies and the federal government represented an annual rate of expenditure of somewhat over eleven million dollars. More than four-fifths of this consisted of contracts between the Department of Defense and extra-governmental research agencies. Even shrinking such a sum back to the rounder and harder dollars of 1939, in terms of which it always seems so much more congenial to think, this represents an annual financial investment in psychological research which a pre-war generation would have put down as fantastic.

Government-supported research on a large scale, born of World War II, appears to have survived successfully the post-war reactionary period. All the sciences—the social less than the physical, but all of them—have profited by this belated arousal of public approbation of research. One could wish that such approval were being tendered science for science sake and the optimistic view, of course, is that the public's attitude may eventually be transformed into that. For the present, one suspects that Los Alamos, Hiroshima, and the alleged moving of mountains are in the motivational picture and that relatively generous public support of research and development is inspired less by Helmholtz's Law of Sufficient Reason than by widespread thinking at the lesser ethical plane of 'scientific research is the best policy.'

If there are clear signs that psychology is receiving the support it deserves, that its rôle in national defense is being recognized, there are other indications, from different quarters, that such is not the case. Division 18 (Psychologists in Public Service) and Division 19 (Military Psychology) of the American Psychological Association constitute the natural media for the expression of professional psychological interest in governmental affairs. The total membership of the two Divisions combined represents



less than 4% of the roster of the American Psychological Association. (Division 18 has considerably less than 2% of the APA membership and Division 19 has only a little more than that amount.)

I do not conclude that the American Psychological Association membership is either subversive or lacking in patriotism. I do not suggest an investigation. Indeed, I suggest we not have one. My contacts with my colleagues lead me to believe that patriotism is probably distributed reasonably normally among them. I am led, however, to raise the question as to why, in a period of clear national peril, when every scientific effort should be bent towards giving our country the advantage in the grim game it is caught up in, when we should be doing our most perspicacious planning against the day of possible disaster, there is so little interest in governmental and military psychology or, at least, so little institutional expression of it.

There are doubtless many causes, including a widespread popular belief that warfare is changing in a fundamental fashion from hot to cold and the suspicion that conflict is less imminent in a world composed of two sufficiently large and well armed camps. These I shall not discuss. Among all possible other reasons I should stress (a) an erroneous conception, shared by an indeterminate but significantly large number of professional psychologists, concerning the true nature of military psychology. This branch of psychology is regarded not uncommonly to be an esoteric pursuit, requiring by way of preparation active service in one or another branch of the military and involving the use of techniques known only to and closely held by the initiated. Another brand of misconception identifies (b) the whole of military psychology with one of its specialized branches—the effort that has come to be called ‘psychological warfare’—and so views military psychology as a kind of cosmic game judged to be beyond the ken of ordinary mortals and somehow vaguely tied up with something called ‘national policy.’ Still a third misunderstanding, a particularly insidious one because it derives support from some ancient and most respectable sources, leads (c) to the shunning of military psychology on the ground that it is essentially an art or a skill, like warfare itself, and is therefore fundamentally non-scientific. We shall return to this last belief with the intent of holding it up to closer inspection and possibly of exorcising a demon or, at least, a noisy *Poltergeist*. Meanwhile, let us consider the somewhat more serious charge that military psychology has an esoteric content.

The identification of the stuff of which military psychology consists may



be best accomplished by positive measures; we need to look in broad sweep at what those in the field are doing, what problems they encounter, what methods they must bring to bear on solutions. One convenient way of providing a framework for the contents of military psychology is to consider the military career in its chronology and to examine cross sections of it at various points. There are some things to be seen in long section also but we are more interested here in sampling a variety of phenomena than in disclosing laws of development and change.

From the moment we view the military man at the point of initial induction to one of the armed services we see a veritable plethora of scientific as well as practical problems. Not all the problems are psychological ones. Some are anthropological, others sociological. Some belong to physiology, others to economics, and some even to political science. It is for this reason that the Research and Development Board of the Department of Defense, carrying out its broad assignment of improving military effectiveness through scientific advance, has generated the concept of Human Resources Research, which it places on a level coördinate with such concepts as Aeronautics, Electronics, Atomic Energy, and Guided Missiles. It is the aim and responsibility of those engaged in Human Resources Research to raise the effectiveness of human processes wherever they occur in the military structure, whether in mobilization or assignment, in the course of training, or in military field operations themselves. Obviously, the full range of human sciences, biological and social, has a stake in Human Resources Research.

Not all problems, then, into which the human factor enters belong to military psychology. To the extent that our scientific neighbors have come to recognize certain problems of the military man to fall within their domains some delimitation of military psychology's province is possible. A tremendous field remains. Its boundaries are not altogether distinct at some points and the filling in of certain regions will have to await future developments. Its main contents, however, are clear. The degree to which they are divided up for consideration involves, of course, some arbitrary decisions, as all classificatory efforts do. I am familiar with a pigeon-holing device, designed within a defense agency to classify research projects in military psychology and cognate fields, which finds 129 categories not to be too many. They are reasonably non-overlapping and yield quite convenient 'packages' for unitary consideration in research planning. Clearly, by grouping and regrouping and ascending to ever higher orders of abstraction one may progressively reduce the number of



rubrics needed. I find, after a fairly extensive adventure in classification, that the contents of military psychology can be subsumed under six major categories. They are:

(1) *Manpower resources.* Here we raise the question as to where our military man comes from. What are the population characteristics? What are the demands of the military on those population traits and qualities?

(2) *Personnel selection and classification.* Who shall be chosen for military service? What abilities, aptitudes, and skills are needed? How are they best put to work in the military structure?

(3) *Human engineering.* Are military tasks properly divided between men and machines? Are equipment and weapons optimally designed for human use?

(4) *Military training.* To what extent and in what manner must behavior be changed to meet military needs?

(5) *Proficiency measurement.* How do we estimate the effectiveness of our military man, once trained and on the job?

(6) *Human relations.* What are the social problems of the military group?

Perhaps I should add a 'seventh age' of military man, a "last scene of all—mere oblivion—sans teeth, sans eyes, sans taste, sans everything." There is, after all, a gerontic problem in the military despite recent statements concerning the fate of the old soldier. Moreover, as everyone knows, old soldiers do not fade away; they retire to San Antonio where they continue, in perpetuity, to lead riotous existences. But these six large, perhaps over-sized baskets seem adequate to accommodate all the proper concerns of military psychology. Let us take a closer look at their contents. I shall not attempt a full inventory, merely a ticking off of the more important items and an accenting of the more urgent problems, taking care not to avoid any cabalistic matters that would tend to relegate military psychology to the realm of the occult or the esoteric.

(1) *Manpower resources.* This category is far from exclusively psychological in composition. From a strictly psychological point of view, indeed, it is the least extensive of the six areas. So much of what we need to know about the origins of our military selectee is sociological, economic, medical, anthropological, ethnic, and demographic, that it cannot be said that the study of manpower resources and requirements calls predominantly for the methods and procedures of the psychologist.

We know a good deal about our people at the 'counting' level, the sort of information provided by repeated census-taking—occupations, racial components, nativity, sex ratios, family composition, etc.—but we have much to learn about the simple, allegedly immutable physical characteristics of our population. These are being altered more rapidly than might be thought. The average American soldier in World War II was ten pounds heavier and nearly an inch taller than his 1917 father. In the same period of time the age distribution, as is somewhat more generally known, at least by Social Security registrants, has changed from a pyramidal to a barrel-shaped



one, so radically has normal life expectation been increased and so sharply has infant mortality been reduced.

We know with even less certainty about our manpower resources psychiatrically considered. Rejections for 'mental and emotional' reasons during the war, especially the early stages of it, were alarmingly high. Nearly a third of those initially classified by Selective Service as 1-A were rejected for mental and physical deficiencies in medical screening at draft-board level and, such was the reliability of the process, that another 16% of presumably qualified men was turned down at Army induction stations. The procedure of screening out not only those who were manifestly psychotic but also those who were suspected of being emotionally unstable or who were judged to be capable of developing psychotic or neurotic symptoms under the stress of military service proved to be a costly one. It is reassuring to note the current trend towards 'screening in' rather than 'out,' with emphasis on preventive psychiatry. Whereas the state of the art is not such as to lead to the belief that all problems of maladjustment in the military will be solved with unerring dispatch, the demand on scientific fact created by this change is a welcome one. It should bring with it a challenge in a research field which has a tendency to lag badly, whether because of ineptness of methods or complexity of investigative situations.

Much of the field of manpower resources, then, lies outside the purview of the psychologist. What part is the province of military psychology? It does not stand alone—there are other legitimate psychological interests here—but the one area of great promise of utility is that of determining the distribution of normal psychological traits in the general population. I am not talking of the testing of college sophomores. Indeed, there would be fewer misconceptions about it if we had tested fewer college students in the belief that we were 'sampling' the general population. We need a truly national aptitude census and we also need a national survey of skills, including their convertibility, and their obsolescence. These are important and urgent. We have an adversary who, by reason of the fact that he controls a vast segment of humanity, can afford to be profligate with personnel. We may not. Knowledge of our own psychological capacities is an advantage we simply cannot afford to forego. This part of military psychology could supply the desired answers. Thus far only the meagerest of beginnings has been made.

(2) *Personnel selection and classification.* This area should not require description in great detail. Insofar as there are any 'classical' problems in military psychology the majority of them fall here. It is an area of great theoretical and practical accomplishment in two wars. We are, moreover, all familiar with the imposing development of testing techniques in education and industry, and the process of isolating and measuring behavior samples differs in no important way from one of these situations to another.

The regions of the military personnel selection area currently showing considerable investigative activity are those of: (1) trait identification, (2) assessment procedures, (3) performance criteria, (4) job analysis and work modification, and (5) underlying statistical and mathematical theory. With respect to the identification and isolation of personality traits, the hope is that the less cognitive elements—such traits as persistence, decisiveness, and carefulness—may yield themselves up to measurement with some such reliability as attends the assessment of the more perceptual and intellectual components of personality. Clinical impressions are badly in need of being



supplanted by psychometric procedures where these pervasive behavioral characteristics, standing at the border between intellect and temperament, are concerned. We shall have made important strides forward when we find ourselves able to test loyalty, sagacity, and enthusiasm with the same competence we now bring to bear on word fluency, finger dexterity, and clerical aptitude.

The great problem in connection with assessment for classification purposes remains what it has been from the beginning, that of differential diagnosis of aptitudes and special skills. The job of classification into a host of military specialties, a task perpetually confronted by all the Services, points up the need for measures highly correlated with the validating criterion but having low intercorrelations among themselves. One suspects that we do not do better in differential classification because the behavior sampled is derived from too narrow a range of response-patterns, that the traits under measurement should be more broadly representative of total personality than are those created by the accidents and prejudices of the testing movement which, it will be recalled, was born and nurtured through the difficult years in an atmosphere of sensationism and intellectualism. At the same time, a question of classification that has to be kept constantly in the forefront is, What are we classifying for—specialized careers built around a limited range of abilities or a duty plan which rotates the individual through a jack-of-all-trades routine? In the military the question seems well on its way to being answered for the enlisted man with some well considered decisions concerning 'career ladders.' At the commissioned level, however, the quarrel between proponents of the generalist and specialist conceptions of officership persists and seems certain to be projected well into the future. The problem, of course, is nearly identical with that encountered in the industrial world. 'Captains of industry' can come up from the ranks or be commissioned directly from civilian life, so to speak, by way of graduate schools of business administration. Much of the future of classification-testing, then, depends on the way the world wags in this respect. Is leadership at the top of the ladder or does one reach it by way of the flying trapeze?

(3) *Human engineering.* The third general area, what has variously been called engineering psychology, psychotechnology, biotechnology, biomechanics, and 'man-machine systems research,' as well as human engineering, is a relative newcomer to the psychological family. Indeed, as an organized effort it is only a few years of age, having come out of problems concerned with the design of military equipment in World War II. It scarcely seems necessary to make an attempt at marking off the boundaries. Fitts and some other recent writers have given definition to the field.<sup>1</sup> Moreover, the boundaries naturally are not too rigidly fixed in an area only recently differentiated from other contents.

It is important that the fighting man vent his spleen on the enemy, not on his own equipment and weapons. A tank gunsight that one cannot get his eye close to, launching gear that requires three hands to operate, or radio headsets that numb the ears cannot be counted as distinct boons. We are a great manufacturing nation and our ability rapidly and in quantity to turn out machines of the highest quality is acknowledged by the whole world. But we often forget that 'machines cannot fight

<sup>1</sup> P. M. Fitts, Engineering psychology and equipment design, in *Handbook of Experimental Psychology*, ed. by S. S. Stevens, 1951, 1287-1340.



alone,' that human operators are necessary and that human limitations, both sensory and motor, have to be taken into account in the design of equipment. It is encouraging that there has recently arisen, even though somewhat belatedly, a recognition on the part of designers of defense materiel that the user is an important element in the situation and many a development-contract now carries the provision that the equipment shall be 'human engineered' at all appropriate stages. The savings, in time and materials otherwise wasted, can be prodigious and it is simple prudence to insure against weapons systems that cannot possibly square with human capacities and proclivities.

Problems belonging in the area of human engineering range from the design of dials of aircraft instruments, which will minimize errors in reading them, to the arrangement and coördination of whole equipment-systems such as are used in naval combat information centers and air-defense networks. It is concerned with the optimal design of methods of working and thus, through time and motion studies, perpetuates the line of endeavor initiated by the parents of the cheap, red-headed dozen. It is interested in the reduction of discomfort occasioned by clothing and personal equipment in extreme climates and under other adverse environmental influences, in the avoidance of work operations that might induce motion sickness, fatigue, or other incapacitating state of the organism, in the development of sensory aids and prosthetic appliances to improve the observational process, in the adoption of equipment and training procedures which will promote safety on the job, in design standardization which will permit interchangeability of items of equipment or obviate the necessity of retraining as weapons and equipment are improved. The human engineer, working in the military setting, is concerned with perceptual discriminations involved in responses to signals and warning devices, those made in reconnaissance operations, those concerned with communications systems of ever-increasing levels of complexity, and those relied upon in contending with the hazards of night combat.

Perhaps, as we tick off the contents of the human engineering area, it becomes apparent why Chapanis, Garner, and Morgan could label their book on human engineering "Applied Experimental Psychology." This is surely no contribution to esoterica. This is a set of problems with which we have had some familiarity for a long time and involves the very stuff of which classical experimental psychology is constituted.

(4) *Military training.* If sensory-perceptual capacities and limitations loom large in the picture of the human engineering area the other great content of experimental psychology, learning, occupies the center of the canvas in the military training area. This, until quite recently, was a scandalously neglected topic. To be sure, the psychology of learning is by now a field of respectable age and psychologists know a great deal about the learning process. Educators have, moreover, come a certain distance in engineering that process. It is quite unsafe, however, to assume that military training is just the schoolroom transferred to the military base. It is even more unsound to assume that soldiers, sailors, and airmen learn their jobs in the same way that white rats learn to run mazes.

As with the other areas considered we can only pause to get the flavor, not attempt a systematizing of training problems. From the first day of military indoctrination in a basic training center to the final one of instruction in the most advanced of the staff and command schools and war colleges all the Services may be viewed, in one



important aspect, as educational institutions. A military commander, recently attempting to characterize succinctly the Army's current predicament, had this to say, "We fight a war, simultaneously mobilize and demobilize, and give everybody at least a fourth grade education." It is not alone the lack of minimal competence in the three R's with which the Army has to struggle. Methods of warfare and weapon systems, becoming increasingly automatic and electronic, require skillful technicians to service them and skillful technicians have to be made that way through a long, expensive, and painstaking course of training. Ask your television serviceman where he acquired his technical knowledge. The chances are that you, Mr. Taxpayer, provided the funds to train him as a military radio, radar, or sonar technician. Radar maintenance men can hardly wait to get out of military service these days to set up their own television shops.

The opportunities for study of the learning process in the military setting are myriad. There is no industry, business, or profession which attempts to develop as many specific skills at so many levels of competence as do the military services. The Army alone, which among them has the greatest complexity of organization, had in World War II over 700 different *types* of authorized units for which it wrote so-called Tables of Organization. These units contained in the neighborhood of 1300 different military occupational specialties for which training had to be provided. There is scarcely a training or educational problem that is not of some interest to military psychology, whether it be about method or motivation, classroom construction or curricula, instructor selection or individual differences in final level of attainment. The classical problems of training management, what has commonly been called in our educational psychology textbooks 'engineering the learning process,' are all represented in the interests of the military: distribution of effort, whole versus part, transfer of training, learning with knowledge of results, etc. The list would run the full distance; and the situations on which each of these would bear would range from small arms instruction in the basic training center to field drill for mountain infantry, from the use of "Why we fight" films to the training of submariners in escape procedures, from the acquisition of minimal proficiency in the use of radio or blinker code to special training in survival techniques for use in prisoner of war camps. Were completeness of coverage the aim we should have to make the list a long one. The important problems which would recur time and again would be those of (1) transfer of training, a central interest because it holds the key to convertibility of skills from civilian to military life and from one form of military duty to another; (2) skill obsolescence, especially significant in relation to reserve policy; and (3) the place of training aids, simulators, and devices in the total training picture, a problem having no single, simple answer but economically as well as psychologically important in that training device development represents an annual expenditure in the armed forces these days of more than ten million dollars.

(5) *Proficiency measurement.* In all walks of life there arises with some frequency the necessity of 'taking a reading,' so to speak, on the behavior of others. More often than not it assumes the form of an unreflective, ill-considered estimate. Less commonly it consists of a grade or rating in which there is at least cursory attention to scaling considerations. Quite infrequently it is formalized into the careful examination of a more or less representative behavior sample and becomes an achievement or proficiency test. In arriving at practical assessments of behavioral effectiveness the



proficiency test, when available and feasible of application, is obviously to be preferred to less objective, less relevant, and less reliable indicators. The military commander, like the educator and the industrial personnel manager, would ordinarily prefer such data as proficiency tests yield in passing judgment on success or failure in military assignments, in guiding decisions on promotions, in developing strong cadres in expanding operations, or in diagnosing weaknesses in his organization. He is certainly familiar by now with the defects of so-called 'efficiency' or 'fitness' reports with their halo effects and skewed distributions.

For purely practical and administrative reasons, then, proficiency measurement is to be encouraged in the military setting. At the present stage of development of military psychology the proficiency test has, however, an even greater importance in relation to the ever-present problem of criterion. Criteria susceptible of quantitative treatment, showing a high degree of relevance and possessing reasonably good reliability, are obviously necessary in validation, not only of selection tests, but of training techniques and assignment procedures as well. On the educational scene the shortcomings of school grades and teachers' ratings are well known. In industry we witness the unsatisfactoriness of performance checks and job-sample ratings. In general, the quantifiable data of performance are far from constituting what has been called the 'ultimate criterion.' In the military setting, as in more peaceful pursuits, school grades, instructor's ratings, flights checks, etc., leave much to be desired. We might do well to set our sights on the development of *composite criteria*, i.e., indices made up of all the relevant and reliably measurable facets of the behavior it is desired to predict, each element weighted in accordance with its logical and statistical worth. To be sure, the empirical determination of the 'right' weights would presumably never be possible, but how can 'ultimate' criteria ever be determined empirically? Besides, there is more than a little precedent for what have euphemistically been called 'rational weights.' The point is that if you are trying to predict an elaborate pattern of behavior you do well to use a similarly elaborate (and thereby more realistic) criterion. In the military situation this is a most important matter, for wartime and peacetime demands on behavior are likely to differ. The requirements of combat are not entirely met by the skills of the technical specialist despite the heralding of push-button warfare.

On the other hand, it is too ready a conclusion that the ultimate criterion must necessarily consist of objective measures of combat behavior. Whereas it would seem, on the surface, that all selection, classification, and training efforts, whether in peace or war, should be directed toward the end that soldiers, sailors, and airmen shall acquit themselves well under fire, it is the fact that armies, navies, and air forces do a great many things beside shoot bazookas, launch torpedoes, and fly Sabre jets. The 1300 military occupational specialties of the Army would hardly be necessary otherwise. We need to seek out the dimensions of behavior at whatever level of elaborateness it may appear and however proximal to the front line it may occur but, for criterion purposes, it must be of the highest obtainable degree of relevance.

(6) *Human relations.* If there is uncertainty as to the exact boundaries of some of the areas of military psychology already surveyed there is much more when we come to consider this last one. How much of the vast domain of interpersonal relations, group behavior, morale, military management and kindred topics one wishes to assign to military psychology and how much to its scientific neighbors is, of course, a problem that can only be solved after some other, related questions have



been answered. How much of psychology is social science, how much natural science? History has not yet said. Meanwhile, proclamations on the subject seem not to carry much conviction. If one sets up the criterion that to partake of the character of scientific psychology a content must be factually determined through the rigorous application of the experimental method, with all its implications for control and careful manipulation of variables, the conclusion will have to be that the 'human relations' area in military psychology is a very small, eminently respectable, probably promising one. If one adopts the view that physical science has not laid down the methodological pattern for investigation of social phenomena, that human relations can never be fully explored by experimental procedures, he then sees this area as a huge and, as yet, practically uncharted territory.

Fortunately, we need not usurp history's function; we shall not issue yet another proclamation. There is no gainsaying the fact that the problems of this area are real enough and important to the military, as to all other social aggregates on earth. There are the common problems of incentives, leader-follower relationships, intra- and inter-group communication, levels of aspiration, group and individual attitudes, coherence and homogeneity of groups, assimilation phenomena, social maladjustment—indeed, as with the training and education area, it would be difficult to specify a problem of social behavior that does not carry some implications for military life. There are the analogues of all industrial management problems together with some new ones which come out of the particular formal properties of military communities.

One class of problems receiving special attention, which is certainly its due, is that of crew formation. In all Services the problem arises of putting individuals together to work as an effective unit—in the Navy, practically all operations at sea, with emphasis on the special demands of submarine life; in the Air Force, the crewing of the big bombers; in the Army, tank teams, to mention only some of the more stressful situations. This problem does not have a glorious history, as is well known to those who, late in World War II in the Army Air Forces, expended a fair measure of sweat and tears, if not blood, in pursuit of the needed answers. Whether the crewing problem can have a glorious future depends, one suspects, upon ingenuity in controlling within the framework of experimental design, a complex set of interpersonal factors which are of the very essence of the small-group situation. This is a good place to get a grasp on the problems of military social psychology because the components of the crew's task can be specified with some precision, the stressful environment is a vivid one, and there is at least some restriction of behavior as contrasted with a completely free-field situation.

Perhaps enough has now been said to carry the point originally made even though it has been possible only to draw attention to some of the local accents and colorings on the spacious canvas of military psychology. This branch of psychology is by no means in the hands of the recondite or abstruse. Indeed it would be difficult to find a topic in general psychology which, in some clear relation or other, does not have a bearing on military psychology.<sup>2</sup>

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<sup>2</sup> Having written that sentence I have put it to test by examining, item by item, the



Now to return to our *Poltergeist*. Many for whom the defining operation I have been engaged in would have been quite unnecessary, who would have freely admitted from the beginning that there is a distinct field of military psychology, would still object to characterizing it as a *scientific* field. Military psychology, they say, is a technological effort and that, by definition, is non-scientific.

Let us consider a hypothetical research project coming out of a practical question, one such as might be posed by the Quartermaster General of the Army. In the course of planning for the production of rations to be used in the tropics, say, he asks what amount of sweetening should be added to certain foods to make them maximally palatable and acceptable to the user. This is not an unreasonable question for the Quartermaster to raise; confectioners have long had the practice of making candies for tropical consumption less sweet lest they taste insipid. In this case the needed detailed data are not available from field-testing nor is there at hand information concerning sufficiently similar situations to serve as a reliable guide. An investigation of the matter is indicated.

A research project having been initiated to provide the desired answers, the investigator, after preliminary familiarization with the many variables involved, designs an experiment which may measure food preferences or perhaps degrees of pleasantness-unpleasantness. One of these, or one of several other alternatives, constitutes the dependent variable in the experiment. The independent variable presumably is concentration of the sweetening agent, since it is the effect of variations in this factor that is of chief interest. Following the accepted rules of scientific investigation, a number of other possible variables—such matters as composition and quality of the basic food product, color and texture of the food, method of presenting the food for tasting, size of the samples, to mention only a few—are held rigidly constant throughout the course of the experiment. Only concentration of the sweetening material is systematically varied in successive trials and with different subjects.

Up to this point any qualified researcher would have taken very much the same steps in planning his work. The pattern is an entirely familiar one. It is just at this point, however, that a decision concerning the manipulation of a particular variable,

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unusually well prepared and detailed subject index of the 1953 volume (Vol. 4) of the *Annual Review of Psychology*, a publication which compensates for not recognizing the field of military psychology as such by including on its editorial committee several good military psychologists. I have sought, with what I hope is not too jaundiced an eye, to find items that could be excluded from consideration. Perhaps my apperceptive mass has its own peculiar rules of organization but I find no topic of major proportions without a significant bearing on military psychology. Oh, there is a little trouble with "Time sense of songbirds," "Group therapy, unconscious transference," and "Character structure of weight-lifters," but these items connect with general psychology a little tenuously anyway. Then there is "Psychoanalysis, dilution of," which seems improbable on the face of it, and "Children, feral," which is a bit bothersome until one recalls that it was a band of soldiers who originally picked up the wild boy of Aveyron, thus making feral children an infrequent but nonetheless real military hazard. At any rate, this little exercise is reassuring; the content of military psychology seems to be of one piece with general scientific psychology.



among all possible operant ones, marks the difference between an *applied* and a *pure* research effort. The problem was stated to be that of ascertaining the effect of various amounts of sweetening on palatability under *tropical* conditions of use. Assuming continuously high temperature to be the significant feature of the tropical environment for this purpose, which may or may not be true, it is clear that a practical, useful set of results can come out of the work only if continuously high temperature is maintained throughout the experiment. The mark of an applied problem is always that certain fixed conditions are given; they are parts of the practical situation and must remain invariant. In the 'pure' or basic research experiment things are different. The investigator is under no compunction to hold constant any particular variable, except, of course, as an ordinary matter of experimental control. Thus, in the hypothetical case cited, he might be tempted to vary temperature over a wide range to test the effect on taste preferences. If he does so he may find out some interesting things and he may be in a position at the end of his research to advise the Quartermaster on rations for the arctic as well as the tropics. The chances are, however, that he will really only be in a position to suggest what variables might be worth manipulating in a new set of experiments which also hold invariant the particular conditions peculiar to the arctic. Basic research findings naturally differ widely in degree of applicability to specific practical situation. Only by the greatest good fortune may they be brought to bear directly and conclusively, without qualification.

I have sought in vain for differentia of basic and applied research. I do not find one more scientific than the other. I do not find one characterized by the use of the experimental method, the other by trial and error procedures. I do not find one in the hands of selfless, dispassionate fellows, the other controlled by self-seeking, avaricious people. The sole difference seems to be that the 'pure' researcher is at liberty to use any and all manipulable variables as independent variables. The 'applied' researcher is restricted to those variables which, when used as independent ones, do not change appreciably the situation from which they were extracted.

In an applied problem on the effect of varying the shape of the sighting mechanism on the accuracy of fire of 50-calibre machine guns one does not vary, systematically or otherwise, the amount of recoil transmitted to the experimental gunner. One keeps it at a uniform level—as a matter of fact, at a level just subliminal for yanking the arms from the sockets—for that is the amount of recoil encountered in 50-calibre free gunnery.

Similarly, in a study designed to test the effect of altitude on ability to detect colored patterns on the ground the technological researcher will not attempt to vary altitude below a practical lower limit set by military aeronautical practice. If he makes such a study he will find, as Hecht and Wallace did at Eglin Field during the War, that from any real height there are no colors at all to be discriminated, whether by normal trichromats or by dichromats. The whole world is a blue-green haze with some bright-



ness discriminations left in it. This, incidentally, is why we cannot make capital of color blindness to cut through camouflage, an idea resubmitted to the military in every mail.

Military psychology obviously needs much research of the applied variety. Less obviously, but just as certainly, it needs basic research. Indeed, the state of the art is such, the reserve fund of fact and theory is so meager, that there is little doubt that long-run progress will be greater in proportion to the early investment of effort in research of the more fundamental variety. A balanced program in military psychology demands both in some measure at all times.

The moral of all that has been said must be obvious. The field of military psychology is of such scope, its boundaries enclose such a miscellany of contents, that many a psychologist who has never regarded himself to be identified with it is yet in a position to make a contribution. Military psychology is not an esoteric art; it is an exoteric science. It demands of its adherents neither that they be steeped in the writings of Clausewitz nor serve an apprenticeship in uniform. The chief requirement for fruitful participation is precisely what it is elsewhere in science—enough knowledge of the potential variables to insure intelligent handling of them.



## PROBABILITY-PREFERENCES IN GAMBLING

By WARD EDWARDS, The Johns Hopkins University

People in gambling situations do not make choices in such a way as to maximize their expected winnings or minimize their expected losses, although there is reason to assume that these are their goals. Several theories have been offered to explain these facts, and experiments have been done to test the theories. In a sense, any study of reinforcement may be considered a study of gambling, but only a very restricted class of experiments, those most similar to real-life human gambling, are here considered.

In 1948, Preston and Baratta reported an experiment in which they presented groups of Ss with bets (described as having a certain probability of winning a given amount of money) and required them to bid competitively (with play money) for the privilege of taking each bet.<sup>1</sup> The players consistently overvalued long shots (low probabilities of high winnings) and undervalued short shots (high probabilities of low winnings), with an indifference point somewhere near a 0.20-probability of winning. No systematic relationship between size of winning bid and amount of payoff in the bet was found, and knowledge of probability theory (*i.e.* which could be assumed in mathematicians and other sophisticated Ss) did not make much difference. Preston and Baratta interpreted their results in terms of a scale of subjective probability different from the scale of objective probability. Griffith has obtained results similar to those of Preston and Baratta in an analysis of actual betting on horse-races.<sup>2</sup>

*Objective model.* The notion of a scale of subjective probability depends on a very simple mathematical model for gambling. Mathematicians have a concept which they call the expected value (*EV*) of a bet. It represents the average payoff per play on a large number of plays, and it is calculated by multiplying the amount of money (*r*) which an outcome will yield the gambler (with losses treated as negative winnings) by the probability (*p*) of that outcome. This product is summed over all *n* possible outcomes:  $EV = p_1r_1 + p_2r_2 + \dots + p_nr_n$ . In any simple gambling situation, it is useful to assume (though the assumption may

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<sup>1</sup> M. G. Preston and Philip Baratta, An experimental study of the auction-value of an uncertain outcome, this JOURNAL, 61, 1948, 183-193.

<sup>2</sup> R. M. Griffith, Odds adjustments by American horse-race bettors, this JOURNAL, 62, 1949, 290-294.





be oversimplified) that the goal of any player is to win as much money as possible or lose as little money as possible. If this assumption is made, then (whenever long-range strategic considerations can be ruled out) the best strategy for the player is easily stated: He should always choose the bet with the larger positive *EV* or (if he is required to accept bets with negative *EV*s) the bet with the smaller negative *EV*. Let us call this mathematical model the *objective model*, since it states the objective conditions for maximizing monetary returns.

Neither in the experiment of Preston and Baratta nor in real life do people follow the objective model. Why not? Several possibilities exist: It may be wrong to assume that people try to maximize monetary returns (or anything else, for that matter) when they gamble. Or, it may be that people try to maximize returns but don't know how. They may misinterpret the probabilities, or the amounts of money involved, or both. Or, they may not know how to combine probabilities and amounts of money to determine the best bet.

The model used by Preston and Baratta to account for their results (a slight modification of the objective model) assumes that people do try to maximize monetary returns, that they do combine probabilities and values according to a simple multiplicative formula, and that the probabilities which enter into this formula differ from the objective probabilities. The subjective probabilities may be calculated from the equation given above. There is no reason, however, why the probabilities should be singled out as the parts of the situation in which objective and subjective values differ. It would be possible instead to assume that the subjective value of a given amount of money differs from its objective value, a notion that has great importance and a long history in economics. Recently von Neumann and Morgenstern proposed the use of the *EV-equation* given above and an experimental procedure similar to that of Preston and Baratta to measure this subjective value (which economists call the *utility* of money).<sup>3</sup>

Mosteller and Nogee translated the proposal by von Neumann and Morgenstern into experimental detail.<sup>4</sup> They presented Ss (Harvard undergraduates and National Guardsmen) with bets, described as rolls at poker-dice, which the Ss could accept or refuse. Bets with the same probability of winning but different amounts of payoff were used to determine for each probability and for each S (with a few exceptions) the bet which S would accept just 50% of the time. These bets to which S was 'indifferent' were then inserted into the *EV-equation* which was modified to include the scale-determining assumption that a loss of five cents had a utility of  $-1$  *utile*. The result was a curve for each S relating subjective to objective values of money. These curves, which differed from person to person, could be used with some degree of success to predict behavior on more complex bets. Each S thus showed some consistency from one situation to another in the way in which he made his decisions.

A series of experiments on rats and human beings have shown that Ss who are

<sup>3</sup> J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior*, 1944.

<sup>4</sup> F. Mosteller, and P. Nogee, An experimental measurement of utility, *J. Polit. Econ.*, 59, 1951, 371-404. The proposal had meanwhile been elaborated by M. Friedman and L. J. Savage (The utility analysis of choices involving risk, *J. Polit. Econ.*, 56, 1948, 279-304).



required to make a long series of choices between two alternatives usually prefer each alternative approximately in proportion to its probability of being correct. This behavior appears in spite of the fact that, whenever one choice is correct more often than the other, the best strategy for maximizing correct choices is always to choose the more frequently correct one.<sup>5</sup> Recently Jarrett has shown that this 'irrational' kind of behavior occurs more frequently when *S* believes that there is some problem present which he can solve, while the strategically best behavior occurs when *S* does not believe there is a solution which will enable him to respond correctly at the 100% level.<sup>6</sup> This finding is reminiscent of Heibreder's distinction between spectator- and participant-behavior in problem-solving situations.<sup>7</sup>

The value of experiments like that of Preston and Baratta or Mosteller and Noguee for determining why people do not make decisions consistent with the objective model is very limited. Neither of these experiments was actually intended to examine this question. Each was based on the assumption that the objective model was correct except that one of its variables needed to be adjusted for the difference between objective and subjective magnitudes. Neither experiment asked the question which logically ought to come first—how do people actually go about making decisions in gambling situations? Jarrett's experiment clarifies this question for a limited set of gambling situations, but much more exploratory experimentation is necessary before it will be reasonable to try to formulate any general theory about gambling. The experiment here reported was designed to explore the general question of why *Ss* fail to make decisions consistent with the objective model. The most direct way of attacking this problem is to require *S* to make decisions under circumstances in which the objective model is inapplicable. Determinants of gambling decisions which can thus be identified may also operate in situations in which the objective model is applicable. The only class of gambling decisions to which the objective model is inapplicable are those in which all choices have equal *EVs*. This experiment, therefore, deals exclusively with such choices. Experiments in which this condition was relaxed will be reported in another paper.

#### APPARATUS AND PROCEDURE

*Apparatus.* The gambling in this experiment was done on the pinball machine illustrated in Fig. 1. There are eight cells at the bottom of this machine, into any one of which the ball may roll. The *Ss* were assured that the ball was just as likely to roll into any one cell as into any other. This statement was correct,

<sup>5</sup> For a review of this literature, see W. O. Jenkins and J. C. Stanley, Jr., Partial reinforcement: A review and critique, *Psychol. Bull.*, 47, 1950, 193-234.

<sup>6</sup> Jacqueline M. Jarrett, *Strategies in Risk-Taking Situations*. Unpublished Ph.D. Thesis, Harvard University, 1951. Available from the Radcliffe College Library.

<sup>7</sup> Edna Heibreder, An experimental study of thinking, *Arch. Psychol.*, N.Y., 1924, No. 73.



though not quite in the way the *Ss* thought. The machine is 'fixed.' Underneath its surface are 18 electromagnets, one on each side of each pin from which the ball must rebound, which are controlled by a tape-reader concealed in another room. In the experiments to be reported here, the endless tape used contained 96 numbers from 1 through 8 selected from a table of random numbers, with the

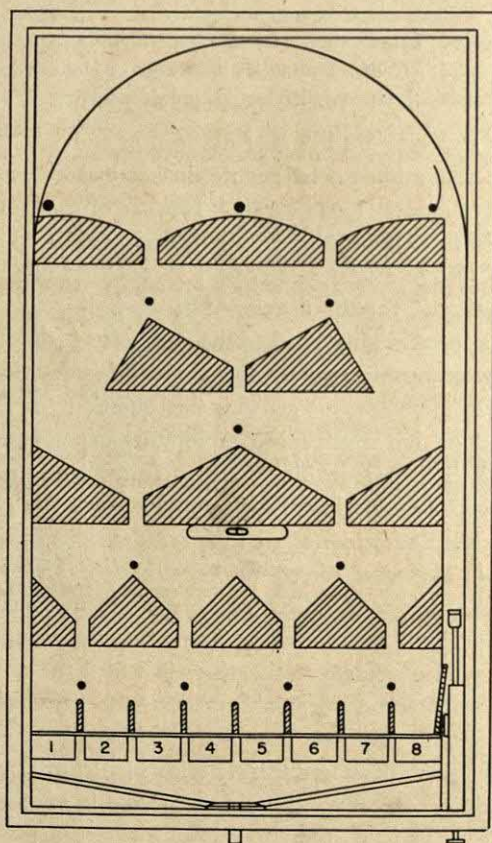


FIG. 1. THE PINBALL MACHINE

restriction that no number should come up more than twice in succession or more often than any other number. Thus, it was literally true that each cell was exactly as likely to pay off as every other cell. During the experiment no *S* expressed doubts about the "honesty" of the machine.<sup>8</sup>

*Bets.* All bets were stated in terms of rolls on this machine. Three sets of

<sup>8</sup> The machine was designed for another purpose by Dr. F. C. Frick and Dr. G. A. Miller; it was built by Mr. Ralph Gerbrands and Mr. Arthur Larsen of the Psychological Laboratories at Harvard University.



eight bets were used (Table I). Each bet in the first group of eight has a positive expected value or *PEV* (+\$.52½). Each of the next group of eight bets has a negative expected value or *NEV* (—\$.52½). The last group consists of eight bets, each of which has a zero expected value (*ZEV*). Each bet at a given *EV* level was paired with all others at the same level according to Ross's optimal order for pairs in the method of paired comparisons.<sup>9</sup> This procedure produced 28 pairs at

TABLE I  
BETS USED IN THE EXPERIMENT

*Positive expected value*

- (1) If you roll a 4, you win \$4.20. If you roll anything else, you win nothing.
- (2) If you roll a 1 or a 7, you win \$2.10. If you roll anything else, you win nothing.
- (3) If you roll a 2, a 4, or a 6, you win \$1.40. If you roll anything else, you win nothing.
- (4) If you roll a 2, a 4, a 7, or an 8, you win \$1.05. If you roll anything else, you win nothing.
- (5) If you roll a 2, a 3, a 5, a 7, or an 8, you win \$0.84. If you roll anything else, you win nothing.
- (6) If you roll anything but a 3 or a 6, you win \$0.70. If you roll a 3 or a 6, you win nothing.
- (7) If you roll anything but a 5, you win \$0.60. If you roll a 5, you win nothing.
- (8) Regardless of what you roll, you win \$0.53.

*Negative expected value*

The eight bets with negative expected value are identical with those with positive expected value listed above except that the verb in each bet is "lose" instead of "win."

*Zero expected value*

- (1) If you roll a 4, you win \$4.20. If you roll anything else, you lose \$0.60.
- (2) If you roll a 1 or a 7, you win \$2.10. If you roll anything else, you lose \$0.70.
- (3) If you roll a 2, a 4, or a 6, you win \$1.40. If you roll anything else, you lose \$0.84.
- (4) If you roll a 2, a 4, a 7, or an 8, you win \$1.05. If you roll anything else, you lose \$1.05.
- (5) If you roll a 2, a 3, a 5, a 7, or an 8, you win \$0.84. If you roll anything else, you lose \$1.40.
- (6) If you roll anything but a 3 or a 6, you win \$0.70. If you roll a 3 or a 6, you lose \$2.10.
- (7) If you roll anything but a 5, you win \$0.60. If you roll a 5, you lose \$4.20.
- (8) Regardless of what you roll, you neither win nor lose.

each *EV* level. Each pair of bets was typed on a 3 × 5 card, making a total of 84 cards, and the deck was shuffled. This deck of cards, each with a pair of bets typed on it; the pinball machine and its associated programmer and tape; and plenty of poker chips constituted the experimental apparatus.

*Subjects.* The Ss were 12 Harvard undergraduates, randomly chosen from a directory of students with the aid of a table of random numbers. Fourteen invitations were sent out by letter, and after varying amounts of persuasion 12 of the 14 accepted. Various data on socioeconomic level, financial status, and personal history were collected from each S; no relations between any of this information and behavior in the experiment could subsequently be found. The sample studied was quite representative of the Harvard undergraduate population. Since the Harvard undergraduate population is, of course, far from representative either of college students in general or of the U. S. population, generalizations of the present findings must be made cautiously.

*Instructions.* The Ss were shown the pinball machine, told the nature of the

<sup>9</sup>R. T. Ross, Optimum orders for the presentation of pairs in the method of paired comparisons, *J. Educ. Psychol.*, 25, 1934, 375-382.



experiment, and informed that they might well lose some of their own money. They were instructed that once they started the experiment they would be required to finish it, regardless of whether they won or lost. They were also told that the experiment would be biased in their favor to the extent of \$1.00 per hour. This was accomplished by paying them \$1.00 an hour for the sessions during which no real gambling took place, and by giving them \$1.00 more in chips at the beginning of each real gambling session than they were required to return at the end of the session. The money earned during the early sessions in which no real gambling took place was retained by *E* until the end of the experiment, as a bond against *S*'s failure to continue the experiment or to pay his losses. The *S*s were run individually and never met one another.

*Just-imagining sessions.* The first four experimental sessions, which will be called the 'just-imagining' sessions, took place in *E*'s office. Each *S* looked at each card and reported which bet he would prefer if he were really gambling, but the pinball machine was not operated and no chips or money changed hands.

*Worthless-chip sessions.* The next six experimental sessions, which will be called the 'worthless-chip' sessions, took place in the experimental room in which the pinball machine was located. Each *S* was given \$21.00 in poker chips at the beginning of each session. He was required to keep these chips in a box sufficiently small so that it was not easy for him to estimate during the course of the experiment how many chips he had. The *S*s were discouraged from counting their chips after verifying that the original count was correct, but if one insisted, as occasionally happened, he was permitted to do so. After *S* announced his preference for one of the bets typed on a card, he operated the pinball machine. If he won, he was immediately given the amount of his winning in chips; if he lost, he immediately paid in chips. After the choice and its outcome were recorded, *S* turned that card face down and went on to the next. Each *S* was required to choose one of each pair of bets, including the *NEV* bets; he did not have the alternative of refusing to gamble on a particular card. At the end of the experimental hour, the chips in *S*'s box were counted. If the sum exceeded \$20.00, \$20.00 worth of chips were taken away and the rest considered *S*'s winnings. If the sum was less than \$20.00, the difference was considered *S*'s losses.

*Real-gambling sessions.* The final four experimental sessions, the 'real-gambling' sessions, were exactly like the worthless-chip sessions except that at the end of each session *E* paid *S* the amount of his winnings or collected from him the amount of his losses in cash.

*Post-experimental sessions.* After all experimental sessions (including some which follow those reported on here) were concluded, extra sessions were conducted to ensure that no *S* lost money in the experiment as a whole. The net effect of these extra sessions was that each *S* recovered the amount of his losses, if any, plus enough more to bring him up to \$1.00 per hour. He was, of course, also given the money earned during the non-gambling sessions and held as bond. This devious method of paying the *S*s prevented the potential *S*s of future experiments from finding out that no one can lose in the experiments on gambling.

## RESULTS

There are many ways of treating data on paired comparisons, and this experiment provided a great deal of data for treatment. In addition to the



choices themselves, there was also information about the outcome of previous choices within a given experimental session, the outcome of the same choice on previous experimental sessions, the immediate or long-range financial status of *S* (whether he was winning or losing), the numbers which came up on the immediately preceding rolls, the comments made by *Ss* during the course of each session and in the questioning period after each session, and many other personal and situational variables. The effects

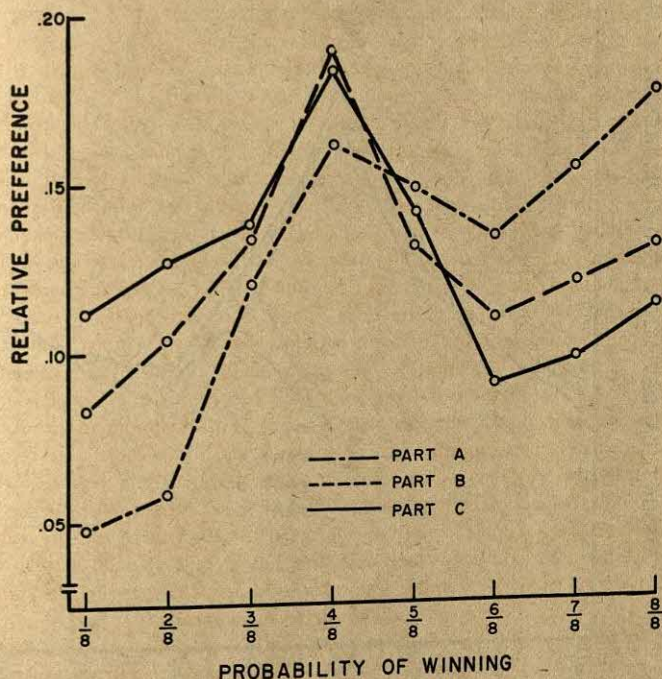


FIG. 2. VOTE-COUNT RESULTS FOR THE PEV-BETS  
(Part A, just-imagining; Part B, worthless-chip; Part C, real-gambling.)

of most of these variables were considered, and for many of them specific analyses of the data were made. The analyses to be reported here are only those which seem most important for understanding what variables systematically influenced decisions. Experiments specifically designed to exhibit the effects of some of the subsidiary variables will be reported in another paper.

*Vote-counts.* The simplest way to treat data on paired comparisons is to count the choices of each item over all the other items with which it was compared. Such a count will here be called a 'vote-count.' Vote counts can be made for one subject-hour, or can be combined to represent several repe-



titions of the experiment by one *S*, or can be combined for all *Ss*. Only grouped results are reported here, but individual vote-counts were performed, and every conclusion drawn for the group depends on properties of the results which also appear in all or most of the individual vote-counts. The standard method of treating data on paired comparisons in-

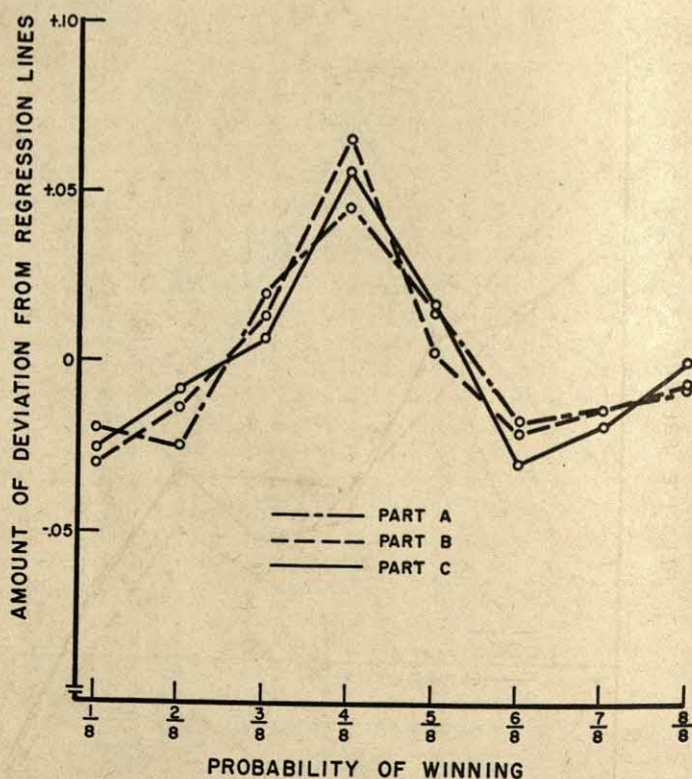


FIG. 3. DEVIATIONS FROM REGRESSION LINES FOR THE *PEV*-BETS  
(Part A, just-imagining; Part B, worthless-chip; Part C, real-gambling.)

volves making vote-counts and then transforming the results by means of assumptions about the theoretical distribution of variability of the choices, usually using Case Five of Thurstone's Law of Comparative Judgment. Such transformations were tried on some of the data from this experiment. They added no new information and concealed some of the properties of the untransformed data, so they are not reported here. The Law of Comparative Judgment is useful when the results of paired com-



parisons are used to generate a ratio-scale; since no such purpose underlay this experiment, it is difficult to see how a scale of the stimuli used would be either reasonable or helpful.

Fig. 2 presents the results of the *PEV*-bets. The measure of relative preference is the number of times each bet was preferred to the seven others with which it was compared, divided by the total number of com-

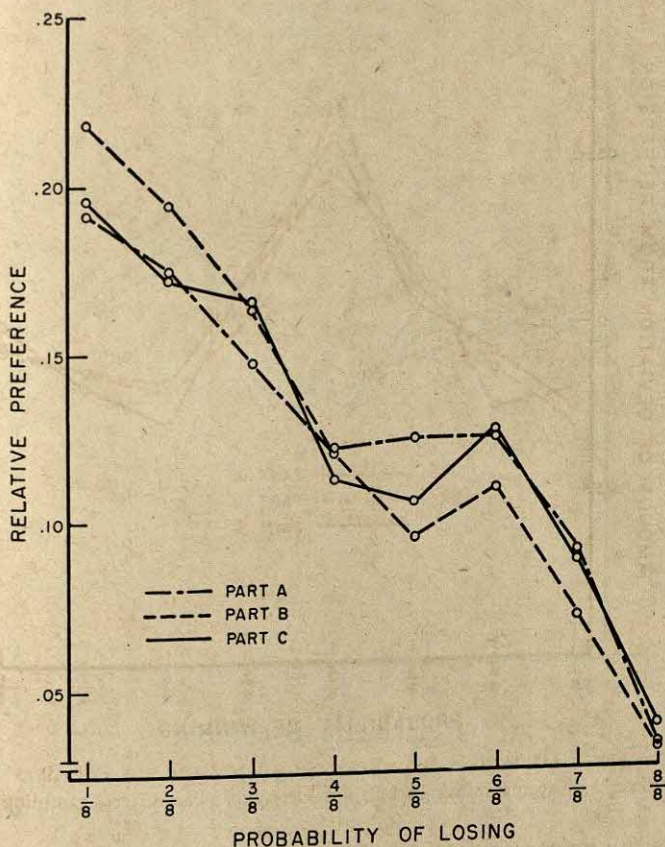


FIG. 4. VOTE-COUNT RESULTS FOR THE *NEV*-BETS  
(Part A, just-imagining; Part B, worthless-chip; Part C, real-gambling.)

parisons made on bets of that *EV* (*i.e.* 28 per subject-hour). The significant features of this graph are the peak at 4/8, the valley at 6/8, and the change in slope from just-imagining to worthless-chip to real-gambling sessions. The first two phenomena probably represent a specific preference



and a specific aversion for the  $4/8$  and the  $6/8$  probabilities, respectively. The last probably represents an increase in general willingness to take chances (long shots rather than short shots) which results from the attractiveness of winning many worthless chips on a long shot and the even greater attractiveness of winning many valuable chips. Experiments to be

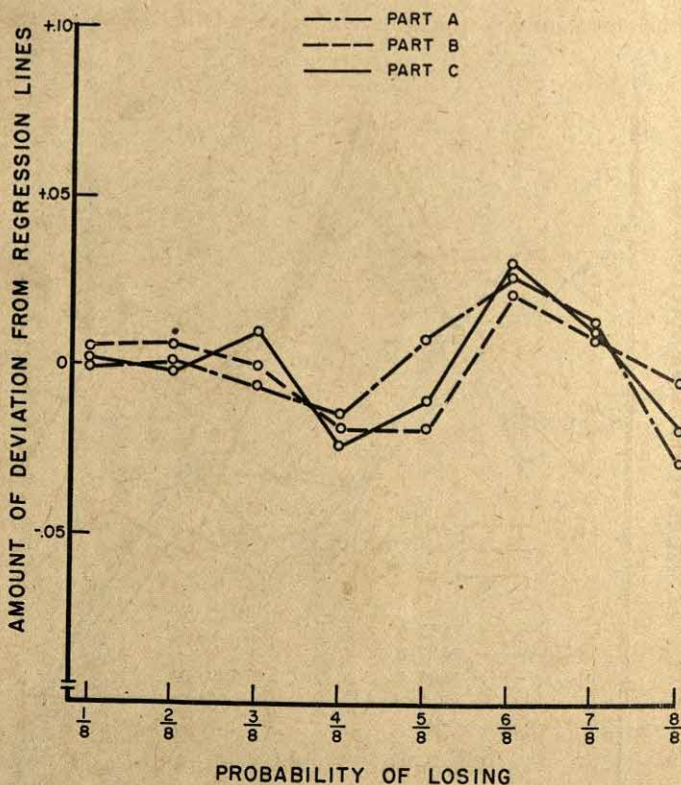


FIG. 5. DEVIATIONS FROM REGRESSION LINES FOR THE *NEV*-BETS (Part A, just-imagining; Part B, worthless-chip; Part C, real-gambling.)

reported in another paper give reason to believe this is not a result of learning.

The general change in willingness to take chances can be removed in a rough way by the graphical device of passing a regression line through each set of points and plotting the deviations from it. Fig. 3 shows the deviation of each point from its regression line. Once the difference in slope due to the different experimental conditions is taken out, the agreement among



the residual curves which represents the specific pattern of preferences for some probabilities over others is much greater than before.

Results for the *NEV*-bets are presented in Fig. 4. The dominant fact about *NEV* bets is that *Ss* don't like to lose—they rather consistently prefer the alternative which had the lower probabilities of losing (and, of course, the higher amounts of loss). This trend is so strong in Fig. 4 that it obscures other relationships which may be present. Other relationships may,

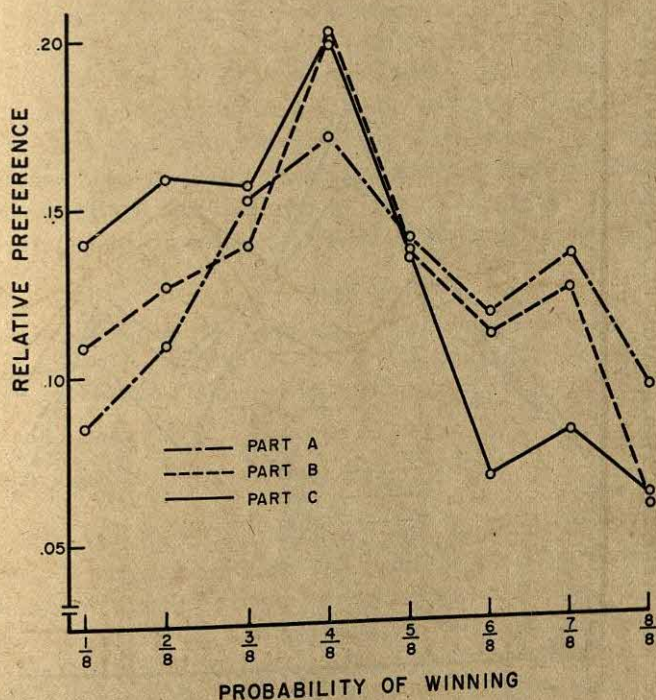


FIG. 6. VOTE-COUNT RESULTS FOR THE ZEV-BETS  
(Part A, just-imagining; Part B, worthless-chip; Part C, real-gambling.)

however, be uncovered by taking out the regression lines. The deviations from the regression lines, plotted in Fig. 5, show a small valley at 4/8 and a small peak at 6/8. In other words, the same specific preferences which operated with the *PEV*-bets also operated to some extent with the *NEV*-bets, but in reversed form. Bets which *Ss* especially like when they contain the verb 'win' are especially disliked when that verb changes to 'lose.'



Results for the *ZEV*-bets are presented in Fig. 6, which looks very much like the corresponding graph for the *PEV*-bets. Fig. 7 shows the deviations from regression lines for the *ZEV*-bets, and once again the similarity to the corresponding figures for *PEV*-bets is marked. The most significant difference between Figs. 2 and 6 is the clockwise rotation of all three curves in Fig. 6—there is a greater preference for a high gain and a low probability of winning on the *ZEV*-bets than on the *PEV*-bets. This result is

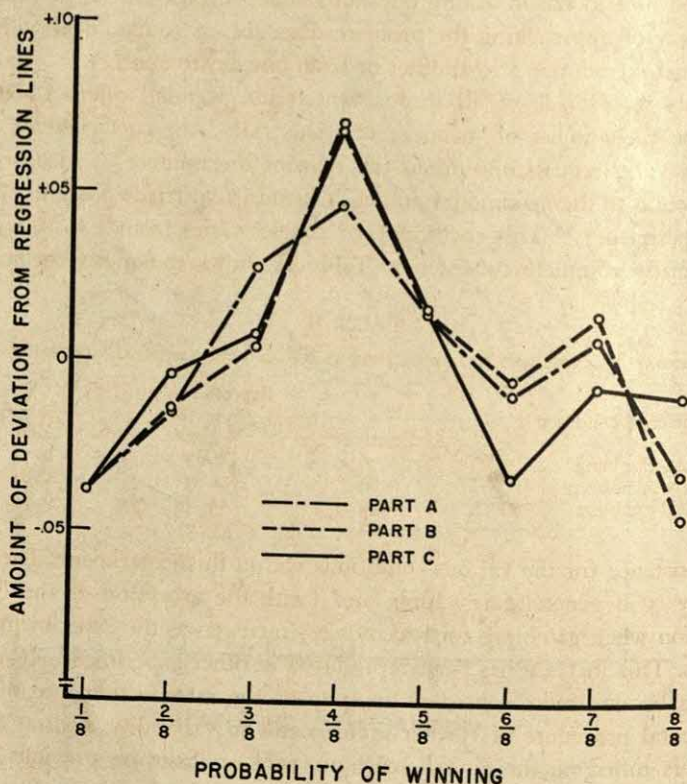


FIG. 7. DEVIATIONS FROM REGRESSION LINES FOR THE *ZEV*-BETS  
(Part A, just-imagining; Part B, worthless-chip; Part C, real-gambling.)

presumably explained by the fact that *ZEV*-bets which have low payoffs and high probabilities of winning also involve the possibility of high loss. The other difference between the *PEV* and *ZEV* bets is in the 8/8 point, which on the *ZEV*-curve represents the item "Regardless of what you roll, you neither win nor lose." The low preference for this bet means that the *Ss* in general preferred gambling to not gambling.



*Inconsistent triads.* One of the advantages of the method of paired comparisons is that it does not force consistency on *S*. One of the most interesting kinds of inconsistency in the present experiment is found when bet *A* is preferred to *B*, *B* is preferred to *C*, and *C* is preferred to *A*. Such a set of preferences will be called an 'inconsistent triad.' It can easily be shown that since more elaborate inconsistencies involving more bets will necessarily include at least one inconsistent triad, the inconsistent triad is a good unit to use in asking questions about inconsistencies in general. (This way of approaching the problem does not, of course, deal with inconsistencies from one *S* to another or from one day to another.)

Counts were made of all inconsistent triads. Kendall offers a statistic based on the number of inconsistent triads called the coefficient of consistence, which equals one minus the ratio of the number of inconsistent triads found to the maximum number of inconsistent triads possible (20 in this experiment).<sup>10</sup> This coefficient, of course, varies from 1 to 0, with 1 representing complete consistency. Table II shows the mean coefficients

TABLE II  
KENDALL'S COEFFICIENT OF CONSISTENCE BY EV AND EXPERIMENTAL CONDITION

Experimental condition	Expected value		
	Positive	Negative	Zero
Just-imagining	0.76	0.73	0.73
Worthless-chip	0.84	0.87	0.84
Real-gambling	0.84	0.82	0.84

of consistence for the various conditions within this experiment. The consistency is in general fairly high, and (with the exception of the *NEV*-condition when gambling for real money) increases as the experiment progresses. This increase has been reproduced in other experiments. It means that *Ss* became more consistent in spite of the introduction into the experimental procedure of further inducements to variability, such as actual numbers rolled on the pinball machine, wins or losses on previous rolls, the financial status of *S* as he makes his choice, and so forth.

Tabulations were made of the bets involved in inconsistent triads. The only interesting result is that for all experimental conditions in the *PEV*- and *ZEV*-bets the 4/8 probability of winning was involved in substantially fewer triads than other bets. This finding is, of course, consistent with the fact that the 4/8 bet was usually preferred to all others with which it was compared. Tabulations also were made of the number of inconsistent triads

<sup>10</sup> M. G. Kendall, *Rank Correlation Methods*, 1948.



in the records of each  $S$  for each  $EV$ , day, and condition. No systematic tendency was found for a given  $S$  to show a given level of inconsistency, a finding which suggests some randomness in behavior.

*Agreement among subjects.* The vote-count curves suggest that  $S$ s were in fairly good agreement, an impression which can be tested statistically. Kendall has developed a coefficient of agreement for paired comparison experiments which is essentially a generalization of his rank correlation coefficient ( $\tau$ ). Such coefficients were computed for all  $EV$ s and experimental conditions. All coefficients were significant beyond the 0.0001 level, showing that  $S$ s agreed with one another to a fairly large extent.

### DISCUSSION

The results of this experiment reveal two kinds of factor which determine choices between pairs of bets of equal  $EV$ . The first is a general tendency to take or to avoid big risks. The second is a set of specific preferences for some probabilities over others. The analysis leading to the identification of these factors was centered on the probabilities of winning or losing, rather than on the amounts of money won or lost. Actually, it would be possible to talk about amounts of money instead. In the experimental design, probability and amount of money are confounded variables, so that any change in one is accompanied by a change in the other. It is, however, more difficult to talk about the results in terms of amounts of money than in terms of probabilities.

Why should an  $S$  prefer a chance to win \$1.05 to a chance to win \$4.20? This question sounds unanswerable, but there is a hypothetical answer to it. If the probability of winning \$4.20 is one-fourth of the probability of winning \$1.05, and the utility of \$4.20 is less than four times the utility of \$1.05, then  $S$ s should prefer the bet involving \$1.05 to the bet involving \$4.20. This amounts to saying that it would be possible to construct a curve relating the utility of money to its dollar value (as in the experiment of Mosteller and Nogee) which would predict these results. Such a curve would look peculiar, since it would have to have at least two inflection points between \$0.53 and \$4.20, but it could be constructed. (The curve would be summed across  $S$ s, and so would not represent individual utilities. Individual curves constructed to account for these results would look equally peculiar.) The use of probability rather than amount of win or loss as the independent variable to which the results are related in this experiment is based on the belief that utility curves are not the best way of accounting for these results. A later experiment will provide evidence to support the view that these results should be attributed to preferences



among probabilities rather than to properties of utility curves for money.

The occurrence of inconsistent triads should be disturbing to those economic theorists and others who assume that the principle of transitivity applies to human choices. (According to that principle if  $A$  is preferred to  $B$  and  $B$  is preferred to  $C$ , then  $A$  should be preferred to  $C$ . The occurrence of an inconsistent triad, then, obviously means that some  $S$  has violated the principle. Since forced-choices were used, inconsistent triads might be expected to occur if  $S$ s were indifferent to the alternatives and chose at random, but the results of the vote-counts show that they did not choose at random. It seems that in this kind of economic decision-making the principle of transitivity in pure form is inapplicable. It is, of course, possible to generate a probabilistic model for decision-making in which an occasional intransitive set of choices is considered a 'random error.' The number of intransitive choices in this experiment is high enough so that a model which would accept them as satisfying the requirement of transitivity is too tolerant to be very useful.

The decrease in the number of inconsistent triads as the experiment progresses can be accounted for by assuming that  $S$ s learn to be consistent as they gain experience with the choices they must make. Apparently this learning is about complete after four sessions. It is pleasant to find that introducing complicating variables such as previous wins and losses within an experimental session does not increase the variability of  $S$ s and so increase the frequency of inconsistent triads. This finding, of course, is further evidence that the inconsistencies should not be dismissed as random errors. It is worth speculating that perhaps intransitive judgments are inevitable features of situations in which the stimuli combine several inconsistent dimensions along any one of which judgments may be made, as do the stimuli in this experiment.

The assumption that preferences among probabilities do determine choices in this experiment has serious implications for the utility curves of Mosteller and Nogee and indeed for the whole method of utility measurement proposed by von Neumann and Morgenstern. Choices among bets can be used to measure utility only if it is legitimate to assume that the probabilities which enter the equations from which the utilities are calculated are the same as the probabilities which determine the choices of  $S$ s. If  $S$ s prefer some probabilities to others, such an assumption is untenable, and any utility measurement based on it is invalid.

It would be wrong to try to generate a scale of subjective probability from this experiment or from that of Mosteller and Nogee or Preston and Baratta. There is nothing to indicate that the value which influenced the behavior of  $S$ s is identical with the objective monetary value of the prize. The situation is one in which one event (a choice) is determined by the interaction of two unknowns, probability and value as  $S$  sees them. It is obviously unreasonable to use the event to measure (in the sense of creating an interval-scale of) either of the unknowns unless we know the value of



the other unknown or can assume that it has been held constant.

This experiment has identified several phenomena involved in preferences among bets. Other such phenomena will be dealt with in subsequent experiments. Only after a detailed qualitative mapping of these phenomena has been made will it be appropriate to make a quantitative model of the complicated process of making decisions among bets.

#### SUMMARY

The Ss in gambling experiments do not make choices in such a way as to maximize their expected winnings or minimize their expected losses, although there is reason to assume that these are their goals. An experiment was designed to find out why they do not. Bets of equal expected value, stated in terms of rolls on a pinball machine, were compared with each other by the method of paired comparisons. Twelve Ss were run under three conditions: just-imagining they were gambling, gambling for worthless chips, and gambling for real money. All losses were surreptitiously returned at the end of the experiment.

The results showed that there are two main factors determining choices among bets of equal expected value. The first is a general tendency to prefer or avoid long shots (bets with low probability of winning or losing and a large amount of win or loss), depending on experimental conditions. For instance, Ss prefer long shots much more when gambling for real money than when just imagining what they would do if they were gambling. The second main determinant of choices is a set of specific preferences among specific probabilities. The most important of these are a preference for a  $4/8$  probability of winning and an avoidance of a  $6/8$  probability of winning.

Ss made choices in such a fashion that inconsistent triads of choices appeared a little less than one-fourth the number of times they could have appeared, and the number of inconsistent triads decreased as the experiment progressed. Different Ss tended to make the same choices in the same situation to a highly significant extent.

The existence of specific probability-preferences points to the inadequacy of such methods of measuring the utility of money as that proposed by von Neumann and Morgenstern and attempted by Mosteller and Noguee. Further investigation of gambling is necessary for a more detailed description of the variables which determine choices among bets before such mathematical models can be applied.



## AFTER-EFFECTS OF PROLONGED INSPECTION OF APPARENT MOVEMENT

By NORMAN H. LIVSON, University of California

The study reported here is concerned with the validity of the general isomorphic assumption of Gestalt psychology which Köhler states as follows: "experienced order in space is always structurally identical with a functional order in the distribution of underlying brain processes."<sup>1</sup> The question immediately prompted by this statement is: Does "experienced order in space," as defined by a person's report of what he sees, correspond to "a functional order in the underlying brain processes"?

To answer this question, an experiment was devised that permitted prediction based upon the isomorphic assumption. Apparent (stroboscopic) movement, in which the phenomenal experience of a moving object occurs in the absence of physical movement in the stimulus-field, was used. In such a situation, isomorphism makes a unique prediction; its designation of the phenomenal field as the datum for cortical events can be translated into an experimental hypothesis. Thus it makes the prediction that the experience of apparent movement, which is based solely upon phenomenological data, is a necessary and sufficient condition for the occurrence of corresponding cortical processes. If this deduction can be demonstrated, then the isomorphic assumption is supported.

The present study measures the cortical correlates which underlie the inspection of a series of highly similar stimulus-situations only one of which gives rise to the experience of apparent movement. The data provide a test of the hypothesis that the cortical correlate of apparent movement differs from those associated with the control conditions.

*Method.* To test the hypothesis of a differential cortical correlate, some measure of the cortical correlate itself becomes necessary. The method chosen, necessarily an indirect one, is to measure the decrease in the amount of autokinetic movement following prolonged inspection of a visual stimulus. This measure derives from the recent

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<sup>1</sup> Wolfgang Köhler, *Gestalt Psychology*, 1929, 61.



work of Crutchfield and Edwards, who found that there is a reduction in the amount of autokinesis after the inspection of a visual pattern.<sup>2</sup> Their results are regarded as indicating some central mediation of this reduction. Since, from the isomorphic assumption, all experienced visual patterns, regardless of their veridicality, possess equal cortical 'reality,' then the inspection of apparent movement can be expected to give rise to a similar reduction in autokinetic movement. Such a reduction may, furthermore, be regarded as indicative of a cortical process.

Considerable evidence indicates that both apparent and autokinetic movement are at least mediated, if not wholly determined, at a central level. Thus, any demonstrated interaction between these two phenomena will allow for the reasonable inference of a mediating central process. A brief review of the evidence for this conclusion follows.

*Historical review.* An early challenger of Wertheimer's cortical 'kurzschluss' theory for apparent movement was the eye-movement hypothesis.<sup>3</sup> A general statement of this hypothesis is that the impression of movement arises from the cognitive interpretation of involuntary eye-movements which accompany the perception of alternately flashing stimuli. By demonstrating apparent movement with rigid fixation and by showing that apparent movement occurs simultaneously in opposite directions, Wertheimer cast doubt upon the tenability of that hypothesis.<sup>4</sup> More recently, precise measurements have shown that there is no significant correlation between the momentary directions of eye-movements and apparent movement.<sup>5</sup> Also, it has been found that the characteristic frequency of eye-movements is less than the usual oscillatory frequency of apparent movement.<sup>6</sup> Frequent reports of the occurrence of apparent movement with alternate stimulation of the two eyes exist in the literature. Such results argue for central mediation of apparent movement. That this mediation is actually cortical is suggested by evidence that cortical damage results in disturbance of the ability to perceive apparent movement.<sup>7</sup> The recent observation that apparent movement is affected by previous visual stimulation has similar implications.<sup>8</sup>

With regard to autokinetic movement the evidence for central mediation is somewhat more equivocal. Indeed, the conclusion drawn from a recent extensive investigation of autokinesis was that "at the present time, there is no adequate explanation for the apparent movement of stationary objects."<sup>9</sup> While available data do not stand in exclusive support of any of the autokinetic theories,<sup>10</sup> they do suggest the presence

<sup>2</sup> R. S. Crutchfield and Ward Edwards, The effect of a fixated figure on autokinetic movement, *J. Exper. Psychol.*, 39, 1949, 561-568; Edwards and Crutchfield, Differential reduction of autokinetic movement by a fixated figure, *ibid.*, 42, 1951, 25-31.

<sup>3</sup> Max Wertheimer, Experimentelle Studien über das Sehen von Bewegung, *Zsch. Psychol.*, 61, 1912, 161-265.

<sup>4</sup> Summarized in Harry Helson, The psychology of Gestalt, this JOURNAL, 36, 1925, 501.

<sup>5</sup> J. P. Guilford and Harry Helson, Eye-movements and the phi-phenomenon, this JOURNAL, 41, 1929, 595-606.

<sup>6</sup> W. S. Hulin and Daniel Katz, Eye-movements and the phi-phenomenon, this JOURNAL, 46, 1934, 332-334.

<sup>7</sup> Adhémar Gelb and Kurt Goldstein, Analysis of a case of figural blindness, trans. in W. D. Ellis, *A Source Book of Gestalt Psychology*, 1950, 315-325; Heinz Werner and S. D. Thuma, A deficiency in the perception of apparent movement in children with brain injury, this JOURNAL, 55, 1942, 58-67.

<sup>8</sup> B. K. Deatherage and M. E. Bitterman, The effect of satiation on stroboscopic movement, this JOURNAL, 65, 1952, 108-109.



of some central and, presumably, cortical mediation. Thus the finding that autokinetic movement correlates with certain personality dimensions cannot easily be handled by any of the existing alternative theories.<sup>11</sup> Similarly, the frequently demonstrated efficacy of social factors in the modification of autokinesis<sup>12</sup> lends itself more easily to an explanation based upon some central mediation process.<sup>13</sup> Finally, Crutchfield and Edwards found that autokinetic movement was diminished even if only the left eye was used during the inspection period and only the right eye during the autokinetic trials.<sup>14</sup>

The results of the studies reviewed above indicate that the hypothesis of a cortical correlate for both apparent and autokinetic movement is plausible. Without making any specific assumptions about the mediating mechanisms, the following hypotheses are assumed in this study. (1) Both apparent and autokinetic movement are mediated, if not wholly determined, by cortical processes. (2) If both phenomena are cortical, then a modification of autokinetic movement by immediately preceding apparent movement may be attributed to the cortical correlate of the apparent movement.

Two measurements of the amount of autokinetic movement, one before and one after each inspection period, were made under four different experimental conditions, each involving alternately flashing lights. Except for a variation in the temporal intervals between flashes, which were so selected that apparent movement occurred in only *one* of the four conditions, stimulus-conditions were constant in all of them.

*Hypotheses.* Two hypotheses were tested, namely: (1) autokinesis will exhibit greater reduction after the inspection of the stimulus condition

<sup>9</sup> Ashton Graybiel and Brant Clark, The autokinetic illusion and its significance in night flying, *J. Aviat. Med.*, 16, 1945, 148.

<sup>10</sup> Some of the theories which argue against the central determination of autokinetic movement offer as alternate mediating mechanisms the following: (1) eye-movements, (2) differential eye strain based upon differences in the strength of the extrinsic eye-muscles, and (3) 'streaming' occurring across the retina. For the latter theory, see J. P. Guilford and K. M. Dallenbach, A study of the autokinetic sensation, this *JOURNAL*, 40, 1928, 83-91.

<sup>11</sup> A. C. Voth, Individual differences in the autokinetic phenomenon, *J. Exper. Psychol.*, 29, 1941, 306-322.

<sup>12</sup> For example, see Muzafer Sherif, A study of social factors in perception, *Arch. Psychol.* 27, 1935 (no. 187), 1-60. Incidentally, this report notes that the autokinetic effect is enhanced by the concomitant hum of a motor. Such inter-sensory facilitation could hardly occur on a peripheral level.

<sup>13</sup> Clearly, these data do not demand such a central process for autokinetic movement. Admittedly, such presumably central factors as personality traits and social suggestibility may modify a more peripheral process. Perhaps, in the last analysis, a preference for a central locus for autokinetic movement must be based upon its allowing for a less complex interacting link, rather than a necessarily more likely one.

<sup>14</sup> *Op. cit.*, 566.



yielding apparent movement than after conditions not giving rise to that experience;<sup>15</sup> and (2) the autokinetic decrements for the three conditions not yielding apparent movement will not differ significantly. This hypothesis follows from the constancy of the relevant stimulus variables (*e.g.* total amount of visual stimulation) over these conditions.

*Experimental design.* Every *S* was tested under each of the conditions. To control order of presentation, a sampling of possible orders was obtained by use of a 4 x 4 Latin square design. The particular square used was devised by Bugelski. It minimizes effect between conditions by having each condition precede and follow every other only once.<sup>16</sup>

*Subjects.* The *Ss* (24 men, 6 for each order of presentation) ranged, in age, from 18 to 47 yr. and, in academic status, from freshman to graduate. On the basis of post-experimental interviews, it was established that all but two *Ss* were naive with respect to the phenomena of both apparent and autokinetic movement. The sole criterion for their selection was 20/20 vision, corrected or uncorrected.

*Apparatus.* The experiment was conducted in a semi-soundproof room, 12 ft. square. With the exception of the stimulus-lights, the room was totally dark throughout the experiment. *S* was seated with his head in a chin rest, 8 ft. from the stimulus-lights. The stimulus-lights for apparent movement, two in number, were small neon lamps (G.E.—NE51, drawing 1/25 w. at 220,000 ohms). One lamp was placed directly behind each of two slits—each 2 in. long and 5/16 in. wide. These slits, covered by one thickness of onion skin paper, were parallel, being separated 2 3/4 in. (a visual angle of 1.6°). Midway between these slits, on their bisecting perpendicular, there was a circular hole, 0.06 in. in diameter, which was also masked by a single thickness of onion skin. This hole, which was illuminated from behind by a small standard flashlight bulb (3.8 v.), served as the autokinetic stimulus-light as well as the fixation-point during all four conditions of the inspection-period. A muffled buzzer, placed in series with this light during the autokinetic trials, served to facilitate the autokinetic effect. A pushbutton on the arm of the *S's* chair permitted him to report the onset of autokinetic movement.

*Experimental conditions.* Four stimulus-conditions were used for the inspection-period. Table I shows the stimulus-conditions which were varied; all others were held constant. Prior to each inspection-period, these conditions were set on the auto-

<sup>15</sup> A greater reduction in autokinetic movement rather than just more change, regardless of direction, is a necessary requirement of the technique employed for the measurement of the intensity of the cortical process, not of the isomorphic assumption. From isomorphism, only the occurrence of a cortical process for apparent movement can be predicted. However, from the Crutchfield and Edwards studies, if it is to be established that such a cortical process did actually occur, a greater *reduction* in autokinetic movement is required. An intervening cortical process reduces the amount of subsequent autokinetic movement rather than merely causing a change. Since reduction in autokinetic movement is serving here only as a measuring instrument, a spelling out of the hypothetical mechanisms underlying this visual after-effect does not seem necessary for the purposes of this report. For such a discussion see Crutchfield and Edwards, *op. cit.*, 566.

<sup>16</sup> B. R. Bugelski, A note on Grant's discussion of the Latin square principle in the design and analysis of psychological experiments, *Psychol. Bull.*, 46, 1949, 49-50.



matic timing device for the neon lights and were held constant throughout the period. The light-dark ratio was 0.5 for all conditions.

TABLE I  
STIMULUS-CONDITIONS FOR THE FOUR INSPECTION-PERIODS

Condition	Duration (min.)	Frequency (cycles/second)	Interval (milliseconds)
Optimal	5.0	2.9	120
Alternation	5.0	0.7	500
Simultaneity	5.0	17.0	20
True simultaneity	2.5	2.9	0

The orientation of the stimulus-lights, either vertical or horizontal, was randomly determined with the restriction that an equal number of Ss be tested under each orientation. Once assigned, orientation was maintained constant over the four conditions for each S.

The 'optimal' condition was intended to yield the perception of apparent movement for all Ss. 'Alternation' and 'simultaneity' were intended as controls for the after-effects of visual stimulation, effects which might arise from the flashing of the stimulus-lights. For all Ss, the experience reported during 'alternation' was a slow succession of the two lights, and, during 'simultaneity,' a rapid, simultaneous flashing of the two lights. The settings for these three conditions were determined from the data of a pilot study and, in general, yielded the desired report during the inspection of each condition.

'True simultaneity' was designed as a control for the possible after-effect which might arise from inspection of stimulus-lights flashing at the particular frequency yielding apparent movement, without regard to the temporal interval between flashes. Thus, during this condition, the two lights flashed in objective simultaneity at the frequency of the 'optimal' condition. Since this involved a doubling of the total time on for each light, and since it was necessary to equate all stimulus-conditions for total amount of visual stimulation, the inspection-period for this condition was halved.

*Procedure.* S was assigned to one of the orders of inspection-period from a sequence established by a table of random numbers. The procedure for each S is outlined in Table II. Each autokinetic trial consisted of the autokinetic stimulus-light being on for 4.0 sec., with a period of 11.0 sec. elapsing between trials to allow for report of the movement. To insure S's attention and proper visual orientation at each trial, the stimulus-light flashed a warning of 0.6 sec. duration 1.5 sec. before every autokinetic trial. After the trial had begun, S pressed a button as soon as he perceived movement. At the conclusion of the trial, S was asked to report how far the light traveled during the time that it was on. The instructions stressed that it was the total length of the path, and not the final displacement of the light, which was to be observed. The report was oral and the units employed were inches.<sup>17</sup> During all autokinetic trials, S used the chin rest.

<sup>17</sup> The oral method for reporting the amount of autokinetic movement was chosen after considerable pretesting of both the oral and the tracing methods. The pre-



The preliminary series of 10 trials allowed S to establish a personal norm of autokinetic movement and to become familiar with the procedure. A majority of the Ss exhibited a marked reduction in the variability of the reported amounts of movement during this series.

After a short rest-period, the pre-inspection series was run. It should be noted that S had been in darkness for about 15 min. before these first experimental measures were made. In the pre-inspection series, S reported the amount of movement on six consecutive trials.

Immediately following the conclusion of the last autokinetic trial, the stimulus-lights for the following inspection-period were turned on at the appropriate temporal sequence. For the duration of this period, S remained in the chin rest and was

TABLE II  
EXPERIMENTAL PROCEDURE

Series	No. of pre-inspection trials*	Inspection-period	No. of post-inspection trials*	Rest (in sec.)
Preliminary	10	—	—	—
1st test	6	Condition I	5	1.5
2nd test	6	Condition II	5	1.5
3rd test	6	Condition III	5	1.5
4th test	6	Condition IV	5	—

\*A trial is a 4-sec. exposure of the autokinetic stimulus-light.

instructed to maintain fixation on the small stimulus-light situated midway between the flashing lights. Reports of the character of the visual experience were obtained at two points during the course of the inspection-period.

Ten seconds elapsed between the conclusion of the inspection-period and the first post-inspection autokinetic trial. After five consecutive test-trials had been run, a rest-period of 1.5 min. was allowed before the next series of trials. The total time required for the experiment, including the preliminary series and the four experimental series, averaged 55 min.

*Results.* None of the Ss experienced difficulty in reporting the amount of autokinetic movement. Post-experimental inquiry established that, while some Ss doubted the 'reality' of the movement, the autokinetic experience was at all times immediate and easily reportable.

The experience during the 'optimal' condition fell far short of its definition, *i.e.* the perception of a single object in clear and uninterrupted motion. Although most of the Ss reported movement, a majority of them

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liminary data indicated that the estimates of movement by these two methods were highly correlated. It was felt that, while the tracing method might provide greater precision of measurement, its advantages were reduced by the double task of reproducing both the extent and the directional vagaries of the movement. Furthermore, several Ss spontaneously commented upon the greater 'naturalness' of the oral report which suggests that perhaps it is indeed the more valid method.



did not, to judge from their reports, perceive 'optimal' movement. A frequent observation was that "the bar of light seems to move behind a screen and pops out only at the ends." Perhaps the major factor contributing to this deficiency in the experience of apparent movement was the necessity for constant fixation during the inspection-period. Other investigations have noted the inhibiting effect of fixation on the perception of apparent movement and, indeed, several Ss explicitly mentioned that "that little light (the fixation-point) seems to get in the way." Also apparent movement seemed to break down quite frequently toward the end of the 5-min. inspection-period.

Without analyzing the factors affecting the experience of apparent movement, it can be said that, in general, the experience fell short of being 'optimal.' To the extent that this was so, the hypothesized cortical correlate of the phenomenal experience would be correspondingly diminished with

TABLE III  
MEAN DECREMENT IN AMOUNT OF AUTOKINETIC MOVEMENT  
FOLLOWING THE FOUR INSPECTION CONDITIONS

Condition	Decrement	No.	Condition	Decrement	No.
Optimal	1.97 in.	24	True simultaneity	1.15 in.	24
Alternation	1.09 in.	24	All non-optimal	1.10 in.	72
Simultaneity	1.06 in.	24	All conditions	1.27 in.	96

a consequent reduction in the measurable after-effect. Thus this deficiency in apparent movement can be assumed to have reduced the differential between the 'optimal' and the control conditions.

The amount of autokinetic movement was usually reported to the nearest half-inch. The statistical tests of differential effects of inspection-periods were performed on a score calculated as the difference in amount of autokinetic movement between the average of the last five pre-inspection autokinetic trials and the first post-inspection trial. Only the first post-inspection trial was used in order to maximize the amount of after-effect detected since the after-effect was assumed to decrease rapidly with time. A reduction in amount of movement is represented by a positive difference score. Table III shows the mean decrement in autokinetic movement following each of the inspection-period conditions. It is clear from this table that the 'optimal' condition resulted in a greater decrement in autokinetic movement than the three control conditions taken singly or combined. These conditions do not differ greatly amongst themselves. Table IV evaluates the significance of these data. The hypothesis of a greater decrement in autokinetic movement for the 'optimal' condition is



supported, for the individual comparisons of the 'optimal' condition against each of the control conditions, at about the 5-% level. For the overall comparison, the difference between the 'optimal' condition and the mean of the three control conditions is significant at the 2-% level. The non-significance of the differences among the three control conditions

TABLE IV

SIGNIFICANCE OF DIFFERENCE BETWEEN THE MEAN DECREMENTS IN AMOUNT OF AUTOKINETIC MOVEMENT FOR THE INSPECTION-CONDITIONS

Conditions compared	Difference (in.)	t*	P
Optimal-Alternation	0.88	1.66	.05†
Optimal-Simultaneity	0.91	1.72	.05†
Optimal-True Simultaneity	0.82	1.55	.06†
Alternation-Simultaneity	0.03	0.06	.96
Alternation-True simultaneity	0.06	0.13	.90
Simultaneity-True simultaneity	0.09	0.17	.86
Optimal-All non-optimal	0.87	2.02	.02†

\* Based upon the best estimate of intra-individual variance provided by the pooled error term of an analysis of variance. The estimate used was 3.37 in. based upon 66 degrees of freedom.

† Since the statement of the hypothesis demands a one-sided alternative, this probability value is based upon a one-tail test.

supports the second hypothesis that, since the *total* visual stimulation was identical for the three conditions, no differential after-effects should occur.

Although the point is not critical for the experimental hypothesis, it is interesting to consider whether this differential reduction in movement is due to an *increased latency* or to a *decreased velocity* of the movement. The report of amount of movement alone confounds these two variables. The question can be answered by an inspection of the mean increments in latency for the four conditions. These data are presented in Table V. As can be seen from this table, the variability in latency increment far exceeds

TABLE V

MEAN INCREMENT (in M.SEC.) IN LATENCY OF AUTOKINETIC MOVEMENT FOLLOWING THE FOUR INSPECTION CONDITIONS

	Conditions			
	Optimal	Alternation	Simultaneity	True simultaneity
Mean	341	377	282	421
SD	740	740	670	700
No.	22	24	23	23

the differences between the conditions. It can therefore be inferred that the greater decrement in autokinetic movement following the 'optimal' condition resulted from a decrease in the apparent speed of the movement rather than from a greater lag in the onset of the movement.



*Discussion.* Do these findings constitute a verification of the isomorphic assumption as it serves Gestalt theory? Do these data insist that events in the cortical field are topographically congruent with experienced order in space? They clearly cannot offer so precise a statement; the measuring technique employed did not map the topographical form of the cortical process. The data do, however, support the experimental hypotheses which represent deductions from the isomorphic assumption and, therefore, support that form of the assumption which allowed for successful prediction.

It seems reasonable to conclude that there exists some form of correspondence between phenomenal and cortical events, that the phenomenal field serves as a valid datum for the prediction of cortical events. Such a statement is clearly an essential postulate of isomorphism. It might be argued, however, that such an inference would not be parsimonious; after all, the autokinetic decrement still remains a function of a stimulus-variable, *i.e.* the temporal interval between the flashing lights. Therefore, if the autokinetic decrement should prove to be some simple function of the temporal interval, prediction would be possible without invoking isomorphism. True! Yet the data do not lend themselves to this argument.

If the autokinetic decrement were plotted as a function of temporal interval with the data from the three control conditions *alone*, a clear linear relationship would be established. All three values are nearly identical so that a linear function of the form, autokinetic decrement =  $f$  (constant), would be indicated. No greater decrement would be predicted for the temporal interval which allowed for the experience of apparent movement. The actual plot of *all* the data, however, would exhibit an abrupt discontinuity at that point. Furthermore, this discontinuity is directly reflected in *S*'s phenomenal field and can therefore be predicted from *S*'s statements about his perceptual experience. Thus, it would seem reasonable to interpret the data as supporting a correspondence between phenomenal and cortical events as predicted by the isomorphic assumption. To argue further that isomorphism is still an unnecessary assumption since the autokinetic decrement could still be described as a function, albeit a complex one, of the stimulus-variables would be to admit the argument. Such an argument, rather than questioning the isomorphic assumption, would be merely specifying the observational basis for the assumption.

An additional finding further supports the isomorphic assumption. As was mentioned above, there was considerable inter-individual variation in the 'goodness' of the apparent movement experienced during the 'optimal' inspection-period. This makes possible a breakdown of the data in terms



of a 'good' or 'fair' movement-group and a 'poor' movement-group. The mean decrement in autokinetic movement for the 'optimal' condition was 0.7 in. for the 'poor' group; for the other group it was 2.6 in. Though these results do not reach the usual requirements for significance,<sup>18</sup> they do suggest that variation along an experiential dimension, *i.e.* 'goodness' of movement, is associated with a variation in the cortical correlate as measured by the after-effect. Such a concomitant variation again represents a deduction from the isomorphic assumption and therefore argues for its validity. It may be inferred, furthermore, that if 'goodness' of apparent movement had been experimentally maximized, the data would have reached a higher level of statistical significance.

It may be pertinent at this point to anticipate a possible alternate explanation of these results. One view which might be advanced is that the greater reduction in autokinetic movement in the 'optimal' condition is due to some kind of 'contrast' effect. The reasoning, in this case, would be that the very vigorous movement perceived by *S* during the 'optimal' inspection-period would set up a frame of reference which would make subsequent autokinetic movement appear diminished. Therefore, since such vigorous movement is experienced only in the 'optimal' condition and not in any of the control conditions, this 'contrast' effect can explain the demonstrated differential in autokinetic movement.

There are at least two answers to this argument. In the first place, such a new frame of reference should be expected to have more than a momentary duration. Contrast effect should, therefore, be operative over the five post-inspection autokinetic trials since only 74 sec. elapse between the conclusion of the inspection-period and the end of the last autokinetic trial. An examination of the data reveals that, for the second post-inspection trial, occurring only 15 sec. after the first trial, the autokinetic decrement has already been reduced from 1.97 in. to 0.60 in.

Secondly, the magnitude of the contrast effect must be considered to be a function of the discrepancy between the magnitude of the pre-inspection autokinetic movement and the magnitude of the 'optimal' movement. The less the pre-inspection autokinetic movement, the greater the 'contrast' effect which should occur as a result of the vigorous, sweeping 'optimal' movement and consequently the greater reduction in post-inspection auto-

<sup>18</sup> There were 8 *Ss* in the 'poor' movement-group, 16 in the 'good' or 'fair' movement-group. The difference in the distributions of the mean decrements of the two groups yielded a *P*-value of 0.09 by the non-parametric Mann-Whitney *U*-test. (See H. B. Mann and D. R. Whitney, On a test of whether one of two random variables is stochastically larger than the other, *Ann. Math. Statist.*, 18, 1947, 50-60.)



kinetic movement. Therefore, if the magnitude of the 'optimal' movement is assumed to be constant, the 'contrast' explanation would require a negative correlation between amount of preinspection autokinetic movement and autokinetic decrement. The product-moment correlation coefficient for these two variables is  $\pm 0.84$ . These data tend to rule out the 'contrast' explanation. Furthermore, it would be equally reasonable to suggest that 'assimilation' rather than 'contrast' was operative. If it were indeed true that the 'optimal' movement tended to enhance the subsequent autokinetic movement, then the demonstrated reduction would be all the more striking.

If this experiment were to be repeated, several modifications of the present procedure would be useful. The most basic change would be to maximize the occurrence of 'good' apparent movement by: (1) determining the best temporal interval for apparent movement for every *S* prior to the experimental session and using that setting for his 'optimal' condition; (2) on the basis of preexperimental testing, rejecting those *Ss* who show frequent breakdown in apparent movement during a long inspection-period; and (3) reducing the intensity of the fixation-point, or eliminating it entirely and controlling fixation by another device. It might also prove advisable to lengthen the preliminary autokinetic series and then reject all *Ss* who fail to stabilize their perceived movement to some personal norm. Such a procedure would minimize intra-individual variability in autokinetic movement and thus make it a more sensitive instrument for measuring the visual after-effect.

*Summary and conclusions.* The purpose of the present study was to test the hypothesis that the phenomenal experience of apparent movement, deriving from certain stimulus-conditions, will possess a cortical correlate different from other phenomenal experiences deriving from very similar stimulus-conditions but not involving the impression of movement. This hypothesis was derived from an assumption of the theory of isomorphism which postulates correspondence between the phenomenal field and cortical events.

This hypothesized differentiation of cortical correlates was tested in terms of the immediate after-effects upon autokinetic movement of prolonged exposure to different temporal patterns of a pair of flashing lights. It has been shown that changes in autokinetic movement, which can be assumed to occur centrally, result from prolonged inspection of a visual pattern.

It was predicted that the inspection of apparent movement would give



rise to a greater decrement in the amount of autokinetic movement than would occur after the three control conditions in which the phenomenal experience was restricted to the perception of discrete stimuli. No difference in decrement was predicted for the three control conditions.

Measures of the extent of autokinetic movement were determined for each *S*. Secondly, each *S* observed alternately flashing lights for 5 min. The phenomenal experience during the inspection-period varied with the temporal sequence of these lights. Lastly, another series of autokinetic measures was taken.

The *Ss* (24 men) varied in age between 18-47 yr. and in academic status between freshmen and graduate students. Most of them were experimentally naïve.

Comparisons of the extent of autokinetic movement before and after the four inspection-periods yielded the following results.

(1) The decrement in amount of autokinetic movement is larger following the apparent movement condition than following any of the three control conditions.

(2) The three control conditions show no difference in autokinetic decrement.

(3) The changes in latency of autokinetic movement following an inspection-period show no differences among the four conditions.

These results support the experimental hypotheses which were deduced from the isomorphic assumption. They indicate that differences in cortical correlates correspond to differences in the phenomenal field of the observer. Since none of its more explicit corollaries have been investigated, only the most general form of the isomorphic assumption is tested by this study.



## BEZOLD'S COLOR-MIXTURE EFFECT

By ROBERT W. BURNHAM, Rochester, New York

Evans recently described an unusual and paradoxical visual effect which appears under what seem to be the conditions for color contrast, yet is phenomenally the reverse of contrast.<sup>1</sup> This effect was reported by Bezold in 1874 but has not apparently been clearly explained.<sup>2</sup>

Color contrast, however, is not the simple, straightforward phenomenon it was once considered to be. Koffka pointed out some time ago, for example, how the classical conception of color contrast as "a *summative* and an *absolute* affair" required modification because of the effects produced when configuration was appropriately varied, and he reviewed a number of Gestalt studies to support his view.<sup>3</sup> Newhall made the point even more clearly when he stated more recently that "with the simple figure-ground relations often found in demonstrations, this classical contrast-effect has long been known to be remarkably regular and predictable. Nearly all observers report the same kind of effect which varies in degree according to known laws of relative size and reflectance . . . but striking departures from classical uniformity can be demonstrated when certain complications of the figure-ground relations are introduced."<sup>4</sup> He went on in the same report to review the literature on contrast-reversals and described observations of his own in which the expected effects of contrast were reversed due not only to intentional configural alterations but to other factors such as set and viewing conditions.

To illustrate the Bezold effect, Evans' Plate XI has been reproduced here in color as Fig. 1.<sup>5</sup> There are three inks used in the four designs, a red, a blue, and a black, and there are no overlapping areas of the inks. Also, as Evans states, "the left and right ends of the design on the second line differ only in the fact that between the red and blue of the right end of the design, black lines have been drawn which are missing on the left. Otherwise the two ends of the design are identical and identical inks were used (the black is printed from a separate plate carrying the lines only and no shading)."<sup>6</sup> The effects produced are related to the presence of these black lines. Bezold originally used a similar plate with red, blue, and black inks, but with eight designs, and stated that "it is the purpose of this plate to show the effect produced

\* Accepted for publication October 6, 1952. From the Color Technology Division of the Eastman Kodak Company.

<sup>1</sup> R. M. Evans, *An Introduction to Color*, 1948, 181 and Plate XI opposite 192.

<sup>2</sup> W. von Bezold, *Die Farbenlehre*, 1874, Plate III and xx, 118, 200, 215, 245-246. Translation by S. R. Koehler, *The Theory of Color*, 1876, Plate V and xxvii-xxviii, 170-171, 182-183, 204, 208-209.

<sup>3</sup> Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 133-138.

<sup>4</sup> S. M. Newhall, The reversal of simultaneous brightness contrast, *J. Exper. Psychol.*, 31, 1943, 393-409.

<sup>5</sup> Evans, *op. cit.*, Plate XI opposite 192.

<sup>6</sup> *Ibid.*, 181.



upon each other by two colors, when juxtaposed in compartments differing in area, and with or without borders."<sup>7</sup>

The mixture-like effects described by Bezold may be seen in Evans' plate (Fig. 1). For example, when viewed casually at a distance the red in the first design looks bluer than the red in the third design, as if it had been mixed with the surrounding blue. The red and blue separated by white lines on the left end of the second design look lighter (with casual distant viewing and without rigid fixation) than the red and blue separated by black lines on the right end of the design. It looks as though black had been added visually to the colors on the right, making them darker than the colors on the left which seem to be mixed with white. The black lines also produce a stronger effect than the white lines. In addition, as Evans states, "to many observers, the right end also is more saturated. . . . These effects are so strong to most observers that a hazy line is seen vertically through the figure at the ends of the black-line part of the figure. . . . It need hardly be pointed out that the brightness changes are directly contrary to what would be predicted from our knowledge of simultaneous contrast."<sup>8</sup> The same mixture-effect shows even more strongly in the red and blue, respectively, of the third and fourth designs. Bezold gave comparable examples, but, in addition, showed quite similar designs in which the classical effect of contrast appeared clearly for adjacent colors, and still other related designs with more elaborate black-and-white borders in which there was neither the effect of contrast nor of the phenomenally reverse mixture.

The particular effect produced seemed to be largely a matter of the size, shape, and position of the borders. Bezold did, in fact, account for the effects of contrast and mixture entirely in terms of these configural conditions. Concerning the elimination of these effects Bezold said that "a line separating the two colored surfaces may almost neutralize their effect upon each other, while a more complicated border acts still more powerfully in the same direction." The most perfect separation of two colors . . . can . . . be obtained by very prominent and expressive outlines, such as may be produced by a border consisting of a number of lines. Such outlines, for the execution of which it will be best to select gold or silver, accompanied by black, or by black and white, have the power of separating colors so completely, that each appears in the hue which it would show if seen by itself upon a neutral ground."<sup>10</sup>

It was apparently no puzzle to Bezold that very similar designs should produce such different effects; certain configurations led to an appearance of mixture, others to contrast, and still others to neither effect. It was not obvious, however, until Evans made it clear, that Bezold's explanation may be oversimplified. The implicit assumption is that configuration alone determines the particular effect seen. At any rate, since these effects have apparently not been subjected to systematic study, the present investigation was undertaken in an attempt to determine what factors, configural or otherwise, may be involved.

#### PRELIMINARY OBSERVATIONS

Informal introspective observations were first made to discover condi-

<sup>7</sup> Bezold (Koehler's translation), *op. cit.*, xxvii.

<sup>8</sup> Evans, *op. cit.*, 181.

<sup>9</sup> Bezold (Koehler's translation), *op. cit.*, 170-171.

<sup>10</sup> *Ibid.*, 209.







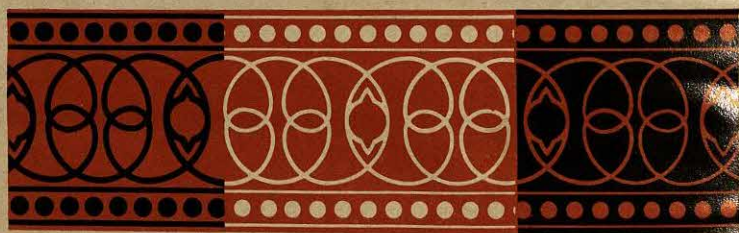


FIG. 1. DESIGNS ILLUSTRATING THE BEZOLD EFFECT

The classical laws of contrast are reversed here as the black elements in the designs produce dark colors, the white elements light colors, the red elements reddish colors, and the blue elements bluish colors.



tions which should be investigated on a more formal basis. Patterns exhibiting the mixture effect were taken from Bezold's book on the theory of color, from Koehler's translation of Bezold's book, and from Evans (see Fig. 1).<sup>11</sup>

In viewing the Bezold and Evans patterns monocularly it was found that near fixation (6 to 8 in.) eliminated the mixture effect or reversed it to give classical contrast. Elimination of the effect for both blue and red was particularly noticeable if fixation was maintained on the area between the right (or left) end and the center part of the second pattern in Fig. 1. Far fixation (18 to 24 in. or greater) and binocular vision, especially with active eye movements, were the optimal condition for producing the effect of mixture. When parts of the pattern were blocked off to reduce its complexity, the probability of seeing the effect of mixture was reduced.

One presbyopic *O* reported a strong effect of mixture when viewing the patterns without glasses within the near-point of clear vision and a reduced effect when he viewed them clearly through bifocal glasses.

Other observations were concerned with an apparent difference between the configural or areal requirements for contrast and for mixture. In the classical situation for contrast, one finds usually a relatively large chromatic area surrounding a smaller area of a different color; in the situation for mixture one finds a relatively large chromatic area surrounded by, or overlaid with, smaller areas of a different color. Since the visual effect in the case of mixture is the reverse of that in contrast, it seemed possible that there might be a critical width of border at which the one effect would change to the other. Several *O*s made observations with a set of blue paper patches (1 in. sq.) prepared with white borders varying in width from 1/64 in. to 3 in. by small steps. For all but one of the *O*s, the blue squares consistently showed the effect of mixture if the *O* looked from one square to the next beginning with the one which had the narrowest border and progressing to squares with wider and wider borders, and showed contrast if he followed a reverse procedure beginning with the square with the broadest border. Either effect continued throughout the series, depending on which was first apparent, and became more intense as the width of the border changed. One *O* reported only classical contrast regardless of the sequence of viewing. A comparable alternation of mixture and contrast was obtained using a set of blue (or red) S-shaped figures, outlined in black (or white), borders of increasing width. Fig. 2 shows a

<sup>11</sup> Bezold, *op. cit.*, Plate III; Bezold (Koehler's translation), *op. cit.*, Plate V; Evans, *op. cit.*, Plate XI.



reproduction of a black-and-white photograph of a sample of the red figures. The two effects may be seen even with the colors reduced to gray.

Another *O* was presented all possible pairs of the bordered square patches by the method of paired comparisons and asked to judge which blue of each pair was the lighter in color. He was unable to distinguish a difference in the blue of any of the pairs, so all pairs were judged to be equally light. Suggestion seemed to be the principal cause of this result, since the *O* thought that all the blue squares had been cut from the same piece of paper.

Another series of preliminary observations was undertaken to determine whether there were distinguishable gradients of the effect of mixture. It



FIG. 2. SIMPLE DESIGNS THAT SHOW EITHER THE EFFECT OF MIXTURE OR OF CONTRAST

was apparent by then that the overlay of lines rather than a simple border was probably required for the most consistent and clear-cut effect. Consequently, a set of green paper patches (1 in. sq.) was prepared with fine white lines of uniform width overlaid in a square check pattern. The number of lines per unit of area was varied in small steps from 16 to 1 per inch. These were viewed tachistoscopically by three *O*s using the method of paired comparisons, at time intervals of  $1/100$ ,  $1/25$ , 1, and 2 sec. The *O*s were asked to judge which of the green areas in each pair appeared lighter. For the longer intervals (1 and 2 sec.) the *O*s reported a progressively greater effect of mixture as the number of lines was increased. For shorter exposures ( $1/100$  and  $1/25$  sec.) the *O*s judged on the basis of white lines only, expressing unanimously the opinion that the



pairs of green patches looked equally light but that a more congested overlay of white lines suggested 'lighter' as a response.

These relatively casual observations indicated that, aside from configural elements, there were factors of suggestion, eye movements, locus of fixation, focus, and viewing distance which determined whether the phenomenal effect was contrast, mixture, or neither. They also indicated that variation in the width of a simple border was usually not enough to account for a change from mixture to contrast. There was, too, an encouraging indication that gradients of the effect of mixture could be estimated and possibly compared with gradients of contrast.

### EXPERIMENT 1

A more formal series of observations was undertaken to test the differential effect of eye movements vs. fixation on the appearance of the effect of mixture, and incidentally to get an estimate of relative intensity of the effects of mixture and contrast produced by a related set of designs.

*Procedure.* Six *Os* judged the lightness of physically identical blue patches (Munsell 5PB 5/6) presented with surrounds intended to show the effects of mixture and of contrast to varying degrees. Fig. 3 is a reproduction of a black-and-white photograph of the actual paper and cardboard materials used. In it may be seen the relative lightness differences among test patches (photometrically identical), borders, overlays, surrounds, and viewing surface. The materials are arranged to express the results, which will be discussed below.

Seven surrounds (3 x 3 in.) were used, some with white and some with black borders or overlays. The surrounds on Cards 1, 4, and 7 were designed to show lightness gradients in the central blue patch by classical contrast (*C*). The surrounds on Cards 2, 3, 5 and 6 were designed to show one series of lightness gradients for the effect of mixture (*M*), and an opposite series for contrast (*C*). For example, a white border (Card 5) and a white overlay (Card 6) should produce successively lighter blues than no border (Card 4) on the same surround if the effect was one of mixture, and successively darker blues if the effect was one of contrast. On this basis, the order of lightness for the effect of mixture should be: Cards 1, 2, 3, 4, 5, 6, 7; and for the effect of contrast: Cards 1, 6, 5, 4, 3, 2, 7.

The cards were presented in pairs by the method of paired comparisons with a full set of judgments to balance out any space error. The *Os* judged whether the left or right patch was the lighter of the two; no equality judgments were permitted. Observations were made under 7500°K Macbeth daylight at 45 footcandles on a 28 x 30 in. neutral (Munsell N5/) mat cardboard surface at an angle of 80° with the line of regard.

Four *Os* made judgments while fixating a point between each pair of cards, and two *Os* judged while actively looking back and forth from one card to the other. No time limit was imposed, but the *Os* were encouraged to make judgments as quickly as possible. A single judgment was reported typically in a few seconds.



*Results.* The mean frequencies with which each of the seven blue patches was chosen as 'lighter' are shown in the pictorial graph in Fig. 3. Results for individual *O*s within a group were so similar that they were

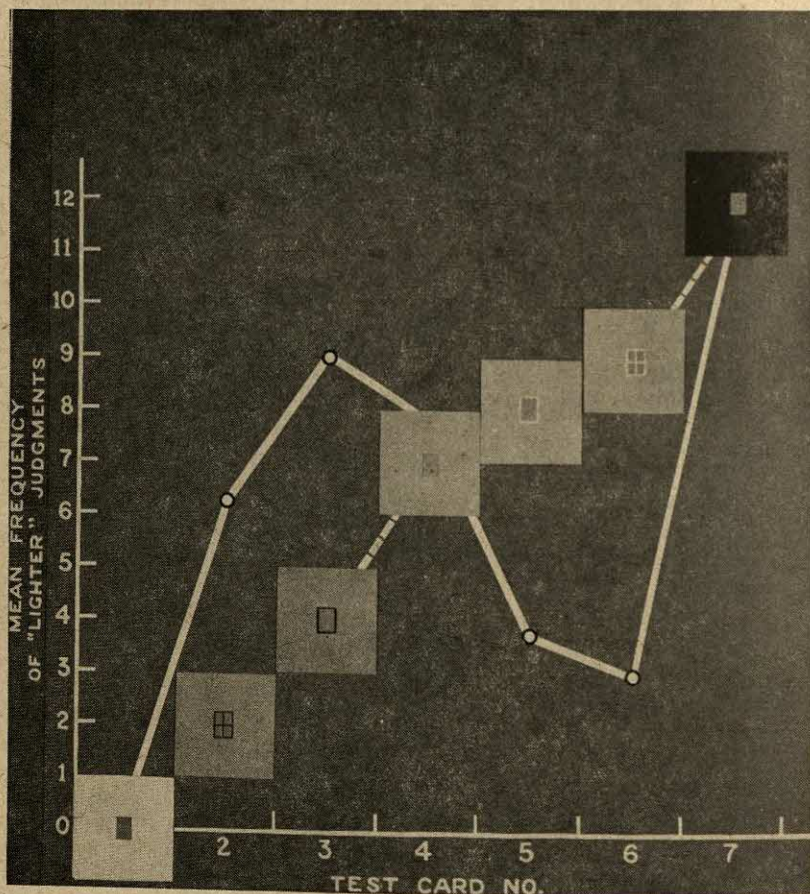


FIG. 3. MATERIALS AND RESULTS OF EXPERIMENT 1

Identical light blue patches were mounted on neutral surrounds varying in lightness and in complexity of borders and overlays. Location of the patches on the ordinates represents rank of lightness when judged by pairs and with eye movements. The dots on the solid graph show difference in judgments of lightness when the *O*s fixated between pairs. The former show the Bezold effect of mixture, the latter the effect of contrast.

averaged according to whether judgments were made with or without voluntary eye movements. The centers of the test-patches in Fig. 3 are placed with reference to the ordinate to show the relative lightness of the test-patches for the 'eye movement' group, and points on the line graph show relative lightness for the 'fixation' group. It can be seen that the



order of lightnesses for the 'eye movement' group was Cards 1, 2, 3, 4, 5, 6, 7 which shows (as was pointed out above) that the effect of mixture prevailed, and for the 'fixation' group the frequencies for Cards 2 and 3, and Cards 5 and 6, moved up and down respectively indicating that the effect of contrast prevailed. The exact order of lightnesses for the cards of the 'fixation' group was, however, Cards 1, 6, 5, 2, 4, 3, 7 instead of Cards 1, 6, 5, 4, 3, 2, 7 as predicted. The significance of the differences in frequency among Cards 2, 3, and 4 for the 'fixation' group and among Cards 4, 5, and 6 for the 'eye movement' group is questionable. This probably indicates some oscillation between the effects of contrast and mixture. The mean frequencies for both groups for Cards 1, 4, and 7 (which showed only contrast) were almost exactly the same and reflect inter-group stability in judgment. Frequencies for Cards 1 and 7 indicate greater phenomenal intensity for the contrast effect shown by those cards than for any of the intervening cards which showed either the effect of contrast or of mixture. The amount of effect can be estimated by considering Card 4 as the 'normal' (middle gray surround) situation. It may also be said that the effect of mixture produced by the black borders and overlays (Cards 2 and 3) was greater (as in Evans' figures) than those produced by the white borders (Cards 5 and 6).

## EXPERIMENT 2

The first experiment verified the notion that eye movements may be expected to contribute to the effect of mixture. The second experiment was designed to test this notion further under more restricted conditions of viewing and to take into account the factors of figural complexity and suggestion which seemed to affect the phenomenal result in the preliminary observations.

*Procedure.* Nine Os judged the lightness of the blue pattern in each of the three sections of the second design in Fig. 1. The sections were presented in pairs in a Dodge mirror tachistoscope by the method of paired comparisons at a surround luminance level of 1.6 footlamberts. Two exposure-intervals were used—0.1 sec. to eliminate voluntary eye movements, and 2.0 sec. to permit eye movements. Two gradients of figural complexity were introduced by exposing one or two loops of the blue pattern (with its surrounding red, white, and black).

An attempt was made to counteract the suggestion of 'lighter' or 'darker' blue due to the presence of white or black borders. This was done by using two different sets of instructions for different Os. Five Os were instructed simply to judge which of the two blues was lighter, and the other four Os were instructed in addition to "ignore the black and white border lines and the red background. Attend strongly only to the blue in each pattern. Do not say that the blue appears darker or lighter in one pattern compared to the other simply because there are black and white border lines. Judge only the blues, disregarding the border lines and the red background."



No time limit was imposed though the *O*s were encouraged to make judgments as quickly as possible. In the case of the 2-sec. exposure-time, most judgments were reported while the stimulus was still present.

*Results.* The results may be summarized by saying that the reports of the *O*s were not affected by the differences in exposure-time, complexity of material, nor instruction. The mean of all observations showed that 96.6% of all the judgments of 'lighter' were made for that section of the design which should appear lighter if the effect of mixture was operative. This is a very striking and consistent result. It looks as though the restricted perceptions permitted by short tachistoscopic exposures are probably largely determined by the total configuration, and that the suggestion of 'lighter' or 'darker' created by the presence of white and black lines is not counteracted by instructions to ignore them.

#### DISCUSSION

Bezold indicated that configural factors of size, shape, and position played the major rôle in creating effect of contrast or of mixture, and that, given the right configural relationships, one had all that was needed to produce either or neither effect.<sup>12</sup> In the present study it became clear that, with only a single configuration, either or neither effect could be produced depending on the extent to which a number of other factors were operating.

Diffusive color mixture may probably be isolated as the principal factor involved in the effect of mixture, for anything which interfered with sharp definition seemed to promote that effect. Interference with sharp definition would be produced by such conditions as distant viewing and lack of sharp focus. It seems possible that scattering in the ocular media of the light reflected from thin borders could also be partially responsible. The interaction of such factors with configural factors like size, shape, and location of borders (complexity of the pattern and viewing distance) would be expected to produce the effect of mixture most definitely and consistently in designs with complicated, lacy, overlaid figures and small areas of continuous color; in these cases local edge-effects would predominate over a more general effect produced by larger areas. These facts are certainly well known to fabric designers and, as Judd points out, "artists take advantage of this kind of color mixture in their mosaics and pointillistic paintings. . . . Lithographers take advantage of it in half-tone printing."<sup>13</sup>

<sup>12</sup> Bezold (Koehler's translation), *op. cit.*, xxvii-xxviii, 170-171, 208-209.

<sup>13</sup> D. B. Judd, *Color in Business, Science, and Industry*, 1952, 60.



On the other hand, sharp definition would be aided by fixation, and when fixation was maintained on the area where sections of the Bezold designs meet, the effect of mixture was reduced or even reversed to show contrast, particularly in monocular vision. The probability of diffusive mixture was also reduced when simplified designs with larger chromatic areas were used, or when near viewing increased the visual angle of particular areas.

Suggestion could account for at least part of the effect of mixture in several situations. The last three of the Bezold designs in Fig. 1 strongly suggest a difference in lightness or darkness by the sharp cut-off of the light and dark trim and other details. This suggestion apparently carried over to the chromatic areas which were compared in trying to estimate the amount of the effect. When the *O*s concentrated closely on the specific chromatic areas to be compared, the influence of suggestion fell away. Suggestion probably contributed a large part of the effect in short tachistoscopic exposures where *O* received a hint of lightness or darkness and ascribed it to the adjacent chromatic area. If it were not for this factor, the short exposures without voluntary eye movements might have eliminated the effect of mixture. Suggestion apparently persisted even with instructions designed to decrease its effect.

Alternate adaptation may have been responsible for some of the effect of mixture in those cases where the direction of regard was alternated from one section of the design to another. Thus, while looking at the section of the design with dark borders, the retina would be partially resensitized so that the chromatic area, when the eye was moved to the section of the design with the light trim, would look lighter (rather than darker as in contrast). Similarly, while looking at the section with light trim the retina would be desensitized that the chromatic area, when the eye was again so moved back to the section with dark trim, would look darker.

Over-all viewing, *i.e.* viewing the design as a whole, favored the appearance of the effect of mixture in a Bezold design, whereas concentration on specific parts of the design tended to destroy the effect. Over-all viewing favored the operation of the above three factors and perhaps contributed something more.

It can be concluded that the Bezold effect does not seem to depend for an explanation on any new assumptions, but is probably to be resolved in terms of factors which are known to operate in other visual situations. The effect is, however, not a simple one of configuration as Bezold's comments might indicate.



## THE DURATION OF THE AFTER-SENSATIONS OF WARMTH AROUSED BY PUNCTIFORM STIMULATION

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After-sensations of temperature, both cold and warm, have long been noted in the literature on cutaneous sensation.<sup>1</sup> Until comparatively recent times, however, no attempts have been made to measure their durations. In 1947, Hall and Dallenbach measured the duration of the after-sensation of cold.<sup>2</sup> Our purpose in the present study was to do for warmth as they did for cold.

### APPARATUS AND PROCEDURE

Except for the modifications necessitated by the substitution of warm stimuli for cold and improvements in the device for timing the duration of the after-sensation, the apparatus and procedure used in this study were the same as those employed by Hall and Dallenbach.

*Stimulator.* A Cornell esthesiometer was used as the stimulator.<sup>3</sup> It was warmed by a stream of water flowing through it by the pressure of gravity. A reservoir (a gallon thermos jug) of warm water was placed high upon an adjustable rack. Employing the principle of Mariotte's bottles,<sup>4</sup> water was siphoned from this reservoir, through the esthesiometer, and on into the top of a terminal container (see Fig. 1). Thus for given positions of the reservoir and the terminal container, the flow of water through the esthesiometer was constant despite the decreasing head of water in the system. A change in the elevation of either jug altered the rate of flow.

The stimulus-temperature desired in this study was 40°C. This value was selected because it is adequate to intense warmth but not to paradoxical cold or to heat.<sup>5</sup> To obtain this temperature, the water in the reservoir had to be about 49°C. because there was a heat loss in the tubing between the reservoir and the esthesiometer of approximately 8.4°C.

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<sup>1</sup> For a review, see N. B. Hall, Jr., and K. M. Dallenbach, The duration of the after-sensation of warmth aroused by punctiform stimulation, this JOURNAL, 60, 1947, 260-271.

<sup>2</sup> *Op. cit.*, 260-271.

<sup>3</sup> Dallenbach, Some new apparatus, this JOURNAL, 34, 1923, 90-95; see also, The temperature spots and end-organs, *ibid.*, 413 f.

<sup>4</sup> E. L. McCarthy, Mariott's bottles, *Science*, 80, 1934, 100.

<sup>5</sup> Eleanor Lowenstein and Dallenbach, The critical temperatures for heat and for burning heat, this JOURNAL, 42, 1930, 423-429; The limen of heat and some conditions affecting it, *ibid.*, 49, 1937, 302-306.



If the siphon ran continuously, the reservoir was emptied in approximately 20 min. During this interval the heat loss, when the water was originally  $49^{\circ}\text{C}$ ., was about  $0.3^{\circ}\text{C}$ . Since the temperature of the water was practically unchanged during the 20-min. interval required for the reservoir to drain, the temperature of the stimulus-point was equally constant. Small changes in the temperature of the stimulus could quickly

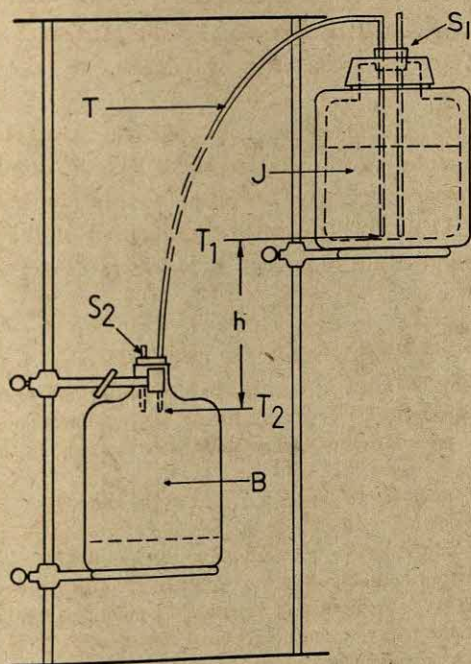


FIG. 1. DIAGRAM OF WATER-SUPPLY SYSTEM SHOWING PRINCIPLE OF MARIOTTE'S BOTTLE

A thermos jug (J) is fitted with a rubber stopper containing an air inlet-tube ( $S_1$ ) and a siphon outlet-tube (T) which extend to the same depth near the bottom of the jar. Since the air pressure at the bottom of the air inlet-tube is atmospheric, the pressure at the bottom of the siphon outlet ( $T_1$ ) will also be atmospheric despite the amount of water within the jug. If the siphon inlet-tube ( $T_2$ ) and the air outlet-tube ( $S_2$ ) are at the top of the terminal container (B), the pressure within the system, hence the rate of the flow of water, is constant and is determined by the distance (h).

and easily be made, however, simply by varying the elevation of the reservoir or of the terminal container.

When the reservoir was drained, it was refilled by water from the terminal container together with a sufficient amount of hot water to bring it to the temperature desired. The temperature of the water in the system was taken at two wells: one just before the water entered the esthesiometer; and the other just as it emerged from the esthesiometer. Since the wells were equidistant from the stimulus-point, the average



of these two readings was taken as the stimulus-temperature. The stimulus-temperature for all the experiments with all the *O*s averaged  $40.6^{\circ} \pm 0.1^{\circ}\text{C}$ .

The esthesiometer, suspended from a horizontal support above the experimental table by small flexible rubber tubing,<sup>6</sup> was brought into contact with *O*'s skin by means of an adjustable stand operated by a rack and pinion gear (see Fig. 2).

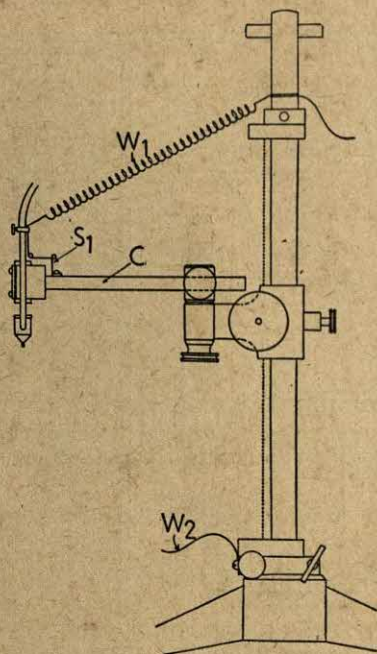


FIG. 2. THE ESTHESIOMETER AND THE ADJUSTABLE STAND

The esthesiometer is connected to the relay of the timing circuit by wires  $W_1$  and  $W_2$ . The switch  $S_1$ , which connects with  $W_2$  through the frame of the stand, completes the circuit between the two wires by bridging the insulation between  $C$  (the clamp holding the esthesiometer by a non-conducting plastic handle) and the esthesiometer to which  $W_1$  is attached. When the clamp carries the weight of the esthesiometer,  $S_1$  completes the circuit; when the weight of the esthesiometer is transferred to *O*'s skin, the gap becomes too great to be bridged by  $S_1$  hence the circuit is broken.

Constancy of pressure among the different trials was obtained by lowering the esthesiometer until the spring which carried its weight was released to a designated point upon the pressure-scale.

*Timing devices.* The duration of stimulation (1 sec.) was timed by means of a silent pendulum, set to beat seconds, which was placed behind *O* but clearly in *E*'s marginal vision.

<sup>6</sup> A photograph of a set-up very similar to ours is shown by Hall and Dallenbach, *op. cit.*, 263.



The duration of the after-sensation was measured in milliseconds by an electrical stop-clock which was connected to the esthesiometer through a relay. The esthesiometer was so mounted that it broke an electrical contact when it was applied at the desired pressure to *O*'s skin. (This device is designated as  $S_1$  in Figs. 2 and 3 which show, respectively, the set-up and the wiring diagram of the relay circuit.) When it was lifted from the skin, the contact was again closed. The 'break' and the 'make' of this contact permitted the timing to be done in the following manner.

Before beginning of a trial, the switch ( $S_1$ ) on the esthesiometer is closed by the weight of the apparatus. *O*'s response-key ( $S_2$ ) and the two relays (Rel. 1 and 2)

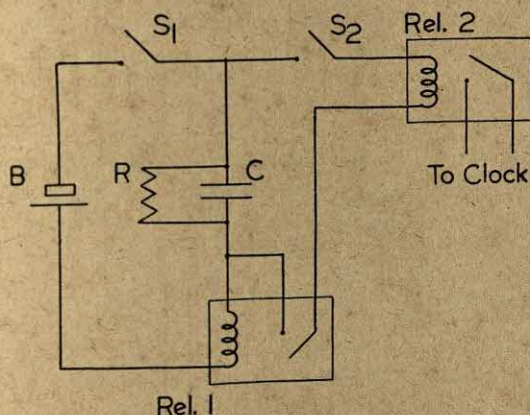


FIG. 3. SCHEMATIC DIAGRAM OF THE TIMING CIRCUIT

B is a 16.5-v. battery; Rel. 1 and 2 are 750- $\Omega$ , 11.5-v. relays; C is a 30 microfarad capacitor; R is a 10,000- $\Omega$  resistor;  $S_1$  is the switch on the esthesiometer; and  $S_2$  is the telegraph key operated by *O*.

are open. Under these conditions the condenser (C) is charged by the battery (B) and a current flows through the resistor (R). This current, however, is insufficient to activate the relay (Rel. 1). When *O*, at *E*'s direction, closes his key ( $S_2$ ), the esthesiometer is brought into contact with his skin. This breaks the connection at ( $S_1$ ). When this occurs, the resistor (R) drains off any charge in the condenser; hence, when the esthesiometer is lifted and  $S_1$  is again closed, the condenser, which is now discharged, offers no impedance to the current hence the coil of the relay (Rel. 1) is activated and the relay switch is closed. This completes the circuit for the second relay (Rel. 2), causing it to close. When it closes the stop-clock starts. When *O* releases his key ( $S_2$ ), the circuit for the relay (Rel. 2) is broken and the clock is stopped. During the operation of the clock, the condenser becomes charged, and ceases to conduct. Thus the first relay (Rel. 1) is opened and the circuit is returned to its original state.

The length of the after-sensation may be timed accurately with this device since the stop-clock registers the length of time elapsing between the removal of the stimulator from *O*'s skin and the release of his key ( $S_2$ ), signifying the cessation of the after-experience.



Though there is a slight lag in the triggering mechanism of this device, the lag is constant from make to break, and from trial to trial, hence it was not taken into account in computing our results.

*Other controls.* The temperature of the experimental room was checked at the beginning of every experimental period. During the course of the entire experiment, it ranged from  $24.0^{\circ}$ – $26.5^{\circ}\text{C}.$ ; and averaged  $25.8^{\circ}\pm 0.7^{\circ}\text{C}.$  During any given experimental hour, the temperature of the room never varied more than  $0.5^{\circ}\text{C}.$ , and seldom as much as that.

The relative humidity of the room was not recorded because evidence indicates that humidity does not affect the sensation of warmth or cold so long as the temperature of the skin is higher than that of the immediately surrounding environment.<sup>7</sup>

The temperature of the area of the skin stimulated was taken at the beginning of every experimental hour with an iron-constantan thermo-couple and galvanometer. It ranged among the *Os* during the course of the study from  $28.9^{\circ}$  to  $33.8^{\circ}\text{C}.$

*Observers.* Five *Os*, two men (*R* and *H*) and three women (*B*, *T*, and *V*), all graduate students, were used in this study. Their ages ranged from 22 to 33 yr. and all were in good health. All were, moreover, naïve in cutaneous observation—not one of them had previously experienced the punctiform arousal of the temperature spots. All were aware of the nature and purpose of the study.

*Procedure: (a) Preliminary training.* Since none of the *Os* had experienced punctiform warmth, all were given preliminary training to familiarize them with the quality and the intensive differences of that experience. Training proceeded in stages through which *O* passed as he became proficient. First, *O*'s warm spots were found and stimulated until he was quite familiar with the quality of warmth. Then he was trained in classifying his experiences upon a three-point intensive scale: weak, moderate, and strong. When he could do that with assurance, he then turned to the observing and reporting of the after-experiences which followed upon the removal of the stimulator.

The training periods varied in length for the different *Os*. In every case, however, *O*'s training was continued at every stage until his observations became consistent and reliable. Before the main experiments were begun, every *O* was given a few dress rehearsals to acquaint and to familiarize him with the general over-all procedure. In this final period of training, *O* became acquainted with the timing mechanism and practiced pressing his reaction-key, when instructed by *E*, and releasing it when the after-sensation of warmth disappeared.

*(b) Main experiments.* The area stimulated was from 2 to 6 cm. above the ulnar prominens on the dorsal surface of the forearm. At the beginning of every experimental period, this area was shaved and a 2- by 3-cm. grid, having cross-section lines 2 mm. apart, was stamped upon it. After a rest-period of 15 min., which was given to permit *O*'s skin to recover from the after-effects of shaving and stamping and also to allow *E* time in which to put the apparatus in readiness, the temperature of the

<sup>7</sup> C. E. A. Winslow, L. P. Herrington, and A. P. Gagge, The reaction of the clothed human body to variations in atmospheric humidity, *Amer. J. Physiol.*, 124, 1928, 692-703; J. D. Hardy and E. F. DuBois, The significance of the average temperature of the skin, in *Temperature: Its Measurement and Control in Science and Industry*, 1941, 537-543; S. H. Bartley and Eloise Chute, *Fatigue and Impairment in Man*, 1947, 122-123.



skin was taken at the middle of the stamped area, and the main experiments were begun.

O sat, with a blindfold over his eyes at the experimental table opposite E, with his left arm held firmly in a comfortable position by a plaster of paris arm-rest that was cast specially for him. At the beginning of every experimental session, the warm spots in the stamped area were mapped by the continuous method. The positions of the spots were marked, as they were discovered, directly on the skin. After the mapping, O was given another rest of 10 min. to avoid the effects of fatigue and tuning.

After the second rest, the marked spots were stimulated again—in the same order in which they were mapped—and the duration of the after-sensation of warmth was measured. Care was taken to stimulate a given spot only once within a 20-min. period.<sup>a</sup>

A total of 1,500 measurements of the duration of the after-sensation of warmth was recorded under these conditions—100 measurements at each of the 3 intensive levels for every one of the 5 Os.

*Instructions.* The instructions to O during the main experiments were as follows:

Report 'yes' when you experience the sensation of warmth and then note in particular its intensity. When the quality of warmth disappears, release your reaction-key and report the intensity of the original sensation and then describe the course of your experience.

*Results.* Although none of the Os had previously experienced punctiform sensations of warmth, all learned quickly to identify them and to disregard the pressury experiences which accompanied the application and removal of the stimulator.

Once the Os became familiar with the quality of warmth, they had little difficulty in classifying their experiences as 'weak,' 'medium,' or 'strong.' In point of fact, four of the Os found the task so easy that they wished to classify their experiences upon a five-point scale—to add 'moderately weak,' and 'moderately strong'—and objected to being forced to use the three-category classification. Since we wished, for comparative purposes, to duplicate Hall and Dallenbach's method,<sup>9</sup> we adhered to the three-category classification. Despite this, the Os at times reported intermediate intensities. When these reports were received the results were discarded and the experiment repeated with another spot. It is probable, however, because of our insistence upon the three-point scale, that a considerable number of experiences were classified upon it that would have been placed at intermediate categories if their use had been permitted. From our results it seems that an intensive classification of warmth is more easily made than of cold.

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<sup>a</sup> A frequency recommended by R. H. Earhart and Dallenbach, *The response of warm spots under successive stimulation*, this JOURNAL, 45, 1933, 722-729.

<sup>9</sup> *Op. cit.*, 267.



After the first stages of their preliminary training, none of the *O*s reported any confusion between the removal of the stimulator and the cessation of the after-sensations of warmth. Neither their comments nor their data give any evidence that they fell into that stimulus-error, as did one of the *O*s in Hall and Dallenbach's study with cold.

*B*: I have gotten so that I hardly know when you remove the esthesiometer. I just concentrate on the warmth and it goes along without a break when you lift the esthesiometer.

*T*: The pressure doesn't bother me at all. The warmth is so different from pressure that the two are easy to keep separate.

All the after-sensations obtained were positive; *i.e.* they were the same in quality as the primary sensations which they followed. According to the *O*s' reports, most of the after-sensations aroused by the stimulation of 'strong' or 'moderate' spots had much the same intensity as the original sensations and ceased rather abruptly. The 'weak' after-sensations, on the other hand, decreased rapidly and immediately.

*V*: The after-sensations for the stronger spots are about as strong as the actual sensations until just before they disappear. Then they fall rapidly.

*H*: The weak after-sensation is even weaker than the sensation, and it starts decreasing as soon as you take the esthesiometer up. It decreases so fast it's hard to focus on.

Occasionally the *O*s reported intermittent after-sensations of warmth. Though relatively rare, the reëmergence of these after-sensations was reported by all *O*s. No attempt was made to measure the time or duration of their reappearance, however, since the apparatus was not adequate for that purpose.

The average duration of the after-sensations of warmth for each intensity level and every *O* is given in Table 1. As these figures show, the averages vary directly with the intensity of the primary sensation, *i.e.* the more intense the experience of warmth aroused by the stimulator, the longer the after-sensation persisted. The relation between the intensity of the original sensation and the length of its after-effect are shown graphically in Fig. 4. Spots yielding 'weak' sensations of warmth gave after-sensations averaging 0.30, 0.49, 0.39, 0.32, and 0.48 sec. for *B*, *H*, *R*, *T*, and *V* respectively; spots yielding 'moderate' sensations gave averages of 0.83, 0.96, 0.83, 0.73, and 1.01 sec.; and spots yielding 'strong' sensations gave averages of 1.54, 1.83, 1.61, 1.98, and 1.84 sec. Differences between the three levels are statistically significant in all cases.

As the statistics given in Table I indicate, the *SD*s of all the measure-



ments from all the *O*s are fairly large. They vary from 12% of *H*'s mean for 'strong,' to 58% of *T*'s mean for 'strong.' To determine whether these variations were due to effects of practice, critical ratios for the first and second halves of the data were computed. These figures are given in Table II.

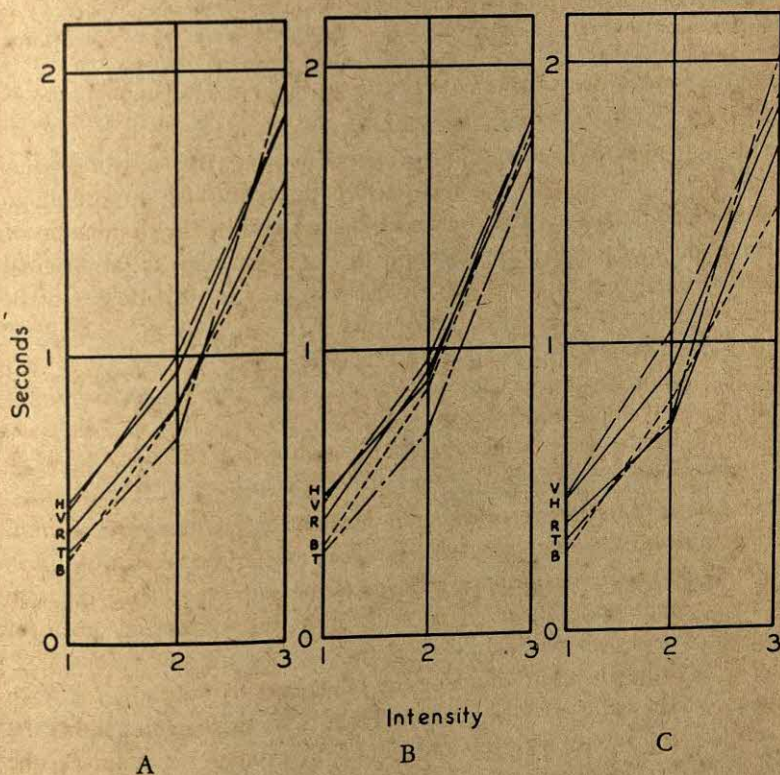


FIG. 4. GRAPHIC REPRESENTATION OF THE RELATION BETWEEN THE INTENSITY OF THE SENSATION OF WARMTH AND THE DURATION OF ITS AFTER-SENSATION

Graph A shows the results of all the experiments and every point within it is the average of 100 measurements; B shows the results of the first half; and C the results of the second half.

Except for *B* with 'weak' and 'strong' and *R* with 'moderate' sensations of warmth, none of the differences between halves is large enough to be statistically significant. In each of these three instances the after-sensations in the first half of the results are longer than those in the second half.

As Table II shows, three of the *O*s gave differences in the averages of



TABLE I

MEAN DURATION (IN SEC.) OF THE AFTER-SENSATIONS OF WARMTH FOR EACH LEVEL OF INTENSITY OF THE PRIMARY SENSATION, TOGETHER WITH RANGE SD, AND CR OF THE DIFFERENCES AMONG THE MEANS  
(N=100)

O	Intensity	Mean	Range	SD	SD/M	CR
B	(1) weak	.30	0.1-1.05	.08	26%	(1)-(2) 9.90
	(2) moderate	.83	0.3-1.75	.31	38%	(2)-(3) 7.84
	(3) strong	1.54	0.3-4.8	.85	55%	(1)-(3) 13.05
H	(1) weak	.49	0.2-1.1	.22	45%	(1)-(2) 12.21
	(2) moderate	.96	0.4-1.6	.25	26%	(2)-(3) 27.42
	(3) strong	1.83	0.8-4.2	.21	12%	(1)-(3) 43.58
R	(1) weak	.39	0.1-1.0	.20	51%	(1)-(2) 13.80
	(2) moderate	.83	0.4-1.5	.25	30%	(2)-(3) 8.64
	(3) strong	1.61	0.5-4.5	.84	52%	(1)-(3) 11.44
T	(1) weak	.32	0.1-0.8	.15	50%	(1)-(2) 15.23
	(2) moderate	.73	0.3-1.7	.23	33%	(2)-(3) 11.81
	(3) strong	1.98	0.8-4.3	.95	58%	(1)-(3) 15.09
V	(1) weak	.48	0.1-1.0	.15	32%	(1)-(2) 17.20
	(2) moderate	1.01	0.5-1.9	.33	33%	(2)-(3) 14.30
	(3) strong	1.84	0.9-3.1	.49	29%	(1)-(3) 26.12

TABLE II

MEAN DURATION (IN SEC.) OF THE AFTER-SENSATIONS OF WARMTH FOR THE HALVES OF THE EXPERIMENT, TOGETHER WITH THE SD, COEFFICIENTS OF VARIABILITY, DIFFERENCES BETWEEN THE MEANS OF THE HALVES, AND THE CR OF THE DIFFERENCES  
(N=50)

O	Intensity	First Half (B)			Second Half (C)			Diff.	CR
		Mean	SD	SD/M	Mean	SD	SD/M		
B	weak	.32	.05	16%	.27	.11	40%	+ .05	3.12*
	moderate	.87	.30	35%	.80	.40	50%	+ .07	1.04
	strong	1.78	.93	52%	1.27	.75	59%	+ .51	2.95*
H	weak	.50	.22	44%	.47	.22	45%	+ .03	0.30
	moderate	.90	.24	27%	.93	.27	29%	-.03	0.47
	strong	1.82	.63	35%	1.84	.73	39%	-.02	0.17
R	weak	.42	.19	46%	.37	.21	57%	+ .05	1.06
	moderate	.94	.25	26%	.72	.19	27%	+ .22	5.67*
	strong	1.74	.85	41%	1.47	.81	55%	+ .27	1.60
T	weak	.31	.18	58%	.33	.13	38%	-.02	0.62
	moderate	.734	.25	34%	.735	.20	27%	-.001	0.02
	strong	1.93	1.07	56%	2.03	.83	41%	-.10	0.52
V	weak	.476	.16	33%	.475	.151	32%	+ .001	0.03
	moderate	.98	.28	30%	1.04	.38	36%	-.06	0.84
	strong	1.81	.51	28%	1.88	.47	25%	-.07	0.72

\* Statistically significant at 1% level of confidence.



the halves of the experiment that were consistent in direction. Those for *T* were longer in the second than in the first half, while those for *B* and *R* were shorter in the second than in the first half. The results for *H* and *V* are not consistent in direction, being longer in the first half than in the second for 'weak' and longer in the second half for 'moderate' 'strong.'

Table II also shows the coefficients of variability for every *O* at every intensity level of sensation for the two halves of the experiment. There is no marked uniformity in the direction of these coefficients. They are large for both halves of the results for all *O*s and intensive levels. The *CR*s for the halves of the data indicate that combining measurements obtained under different degrees of practice cannot account for all the variability obtained. A number of factors or combinations of factors may be responsible for such variability. Among these are: differences in skin temperature and consequent differences in physiological zero; differences in the tissues stimulated on different days; and the possible classification of spots of intermediate intensity in the three categories of 'weak,' 'moderate,' and 'strong.' These conditions varied unsystematically and it is impossible from the data obtained to determine their role in the shaping of results.

#### SUMMARY

The length of the after-sensation of warmth aroused by punctiform stimulation of 1-sec. duration and of relatively constant temperature ( $40.6 \pm 0.1^\circ\text{C}.$ ) was measured. The results obtained are summarized below.

(1) The after-sensation of warmth is positive. No evidence was found of the negative after-sensations reported in the older literature. It may well be that, by their use of areal stimulation, the earlier experimenters aroused cold sensations along with warmth, and that the longer and more definite after-sensations of cold masked the shorter, more diffuse after-effects of warmth.

(2) The duration of the after-sensation varied directly with the intensity of the sensation aroused. The greater the intensity of the original sensation, the longer the after-sensation.

(3) The after-sensations varied considerably in length; ranging from 0.1 to 3.2 sec. for the *O* with the least variability to 4.8 sec. for the *O* with the greatest variability. The average duration of the after-effects of 'weak' sensations of warmth varied among the *O*s from 0.30 to 0.49 sec.; of 'moderate' sensations, from 0.73 to 1.01 sec.; and of 'strong' sensations from 1.54 to 1.98 sec.

(4) The differences between the average durations for the various



intensive levels of sensation are large and statistically significant.

(5) The duration of the after-sensation of warmth varies for a given individual at different times. The reason or reasons for this variation could not be established. Possibly it is due to uncontrolled experimental factors such as differences in tissue stimulated at different times and differences in physiological zero on different days, or to restricting the classification of the intensity of the sensations aroused to three categories when it seemed to fall more naturally into five.



# EVALUATION OF THE NEURAL QUANTUM THEORY IN VISION

By H. RICHARD BLACKWELL, University of Michigan

Considerable interest has been aroused in recent years in the theory of the neural quantum. This theory is concerned with the fundamental nature of sensory discrimination. Its principal support comes from the studies of Stevens, Morgan, and Volkmann,<sup>1</sup> and Miller and Garner.<sup>2</sup> These authors worked in the field of audition. The present study carries the theory into the field of vision.

The theory may be tested only on the basis of predictions concerning the precise form of the data obtained in threshold-measurements. Let us consider the predictions made by the theory, describing the theory itself only in the most cursory fashion.

The theory postulates units of neural excitation which are designated neural quanta. An assumption from this theory is that a stimulus-increment will be discriminated whenever it excites an additional neural quantum. Under this assumption, an  $S$  is said to have adopted the 'one-quantum criterion' of discrimination. The stimulus-increment required to excite one additional neural quantum varies from moment to moment because of variations in the excitation produced by the prevailing stimulus-level. The prevailing stimulus may just excite a given number of quanta, or it may excite a given number and leave a residual. This residual, although insufficient to stimulate an additional quantum, reduces the stimulus-increment required to excite an additional quantum.

We may make certain assumptions about the nature of the variability of excitation. From these it follows that the distribution of monetary threshold increments will vary rectilinearly from zero to  $Q$ — $Q$  being the increment that just excites an additional neural quantum when the residual excitation from the prevailing stimulus is zero. When the method of constant stimuli is employed, data will be represented by the integral of the rectilinear distribution, like the curve presented in Fig. 1, which is designated 1 quantum. We note that no discriminations are made when the luminance-increment is zero and that they were made 100% of the time at a luminance-increment equal to  $0.2Q$ —which in this case equals  $Q$ .

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\* Accepted for publication August 26, 1952. This research was supported by Project NR 142-106; Contract N5ori-116, Task Order V, between the University of Michigan and the Office of Naval Research, U.S. Navy.

<sup>1</sup> S. S. Stevens, C. T. Morgan, and John Volkmann, Theory of the neural quantum in the discrimination of loudness and pitch, this JOURNAL, 54, 1941, 315-335.

<sup>2</sup> G. A. Miller and W. R. Garner, The effect of random presentation on the psychometric function: Implications for a quantal theory of discrimination, *ibid.*, 57, 1944, 451-467.



It is difficult for an *S* to adopt the 'one-quantum criterion,' since the excitation produced by the prevailing stimulus will occasionally vary by one quantum. A more likely basis for discrimination requires that the stimulus-increment excite two additional quanta for discrimination to occur. The excitation produced by the prevailing stimulus cannot vary by two quanta, because the excitation produced by the prevailing stimulus varies continuously from quantum to quantum. With these assumptions, threshold-data will conform to the curve in Fig. 1, designated 2 quanta. We note that there are no discriminations until the luminance-increment equals 0.2, which is the stimulus-increment required to raise the level of excitation by one quantum. These are 100% discriminations when the luminance-increment equals 0.4, which is the stimulus required to raise the level of excitation by two quanta.

It is possible for the *Ss* to require that more than two additional quanta be excited by the stimulus-increment for discrimination to occur. If three quanta are required, the data will conform to the curve in Fig. 1, designated 3 quanta. No discriminations will occur until the luminance-increment is sufficient to excite two additional quanta. Discriminations will occur 100% of the time when the luminance-increment is sufficient to excite three additional quanta.

Fig. 1 was constructed by assuming that  $Q$ , the luminance-increment required to excite one quantum, was constant when the quantum criterion changed. Changes of  $M$ , the threshold, are very large when the quantal criterion changes. Such changes should be easily differentiated experimentally from changes in the value of  $Q$ , which would be expected to be much smaller.

Since the form of threshold-data is the only prediction of the quantum theory, our experimental task is to evaluate the extent to which actual data conform to the theoretical quantum curves. This task is made difficult by a number of conditions. Essentially, the quantum theorists have so restricted the allowable conditions of measurement and the analysis of the data that it is difficult to obtain an unambiguous evaluation of the theory.

In the first place, the quantum theorists have specifically restricted applicability of the theory to data collected with one psychophysical procedure. Throughout their writing, only one *indicator-response*, *phenomenal report*, is considered for use in threshold-measurement. With this indicator-response, *S* is required to assess directly his conscious awareness of discrimination. Other indicator-responses may be used in threshold-measurement. In fact, it is shown elsewhere that another indicator-response, designated *forced-choice*, is more adequate than phenomenal report for routine use in threshold-measurement.<sup>3</sup> Forced choice is defined by two conditions: (a) *S* is required to indicate discrimination by correctly identifying some verifiable attribute of the stimulus, such as its spatial location or temporal interval; and (b) he is required to select an answer on each stimulus-presentation—even if he has to guess.

The quantum theorists place the further restriction that stimulus-magnitudes must not be randomly ordered in the psychophysical series; they must be grouped into blocks of presentations of the same magnitude. If stimulus-magnitudes are randomly ordered, it is considered that *S* will be unable to maintain a fixed quantal criterion.

Restriction of the applicability of the quantum theory to data collected under these

<sup>3</sup> H. R. Blackwell, Psychophysical thresholds: Experimental studies of methods of measurement, Eng. Res. Inst. Bull., Univ. of Mich. Press, No. 36, 1952, P. 226.



conditions of measurement is unfortunate, since this method gives *S* a unique opportunity for an invalid mode of response.<sup>4</sup> It was found that *Ss* who utilized the general experimental conditions specifically advocated by the quantum theorists exhibited *positive response channelization*, defined as an increase in the frequency of 'Yes' responses toward the end of the blocks of stimuli for which the predominant re-

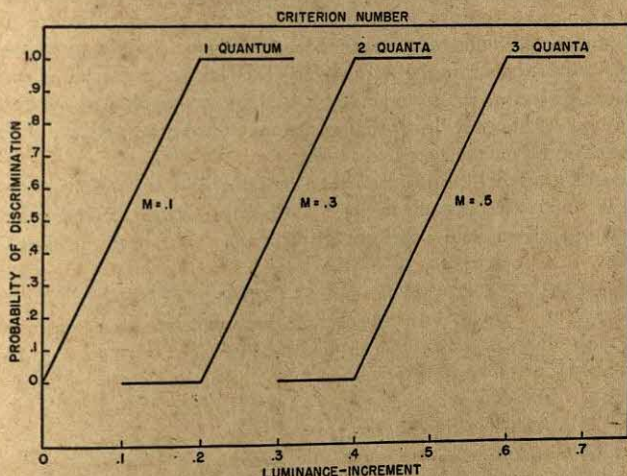


FIG. 1. THEORETICAL CURVES REPRESENTING PREDICTIONS OF THE FORM OF THRESHOLD-DATA BASED ON THE THEORY OF THE NEURAL QUANTUM. Each curve corresponds to a criterion number of quanta, as indicated.

sponse was 'Yes.' It would appear that the *Ss* are unable to ignore their knowledge that stimuli are blocked into groups having the same magnitude, but rather let this knowledge distort the pattern of their responses. The experiments in which positive channelization occurred utilized a procedure which differed from that specified by the quantum theorists only in the inclusion of 'catch stimuli.' The 'catch stimuli' were grouped. One would expect on a priori grounds that the presence of 'catch stimuli' would reduce rather than increase the likelihood of response-channelization. Thus, it appears that the procedure advocated by the quantum theorists is a most unfortunate choice when data are desired to establish the fundamental character of sensory discrimination.

The quantum theorists made the theory difficult to establish in a second way, by specifying that the theoretical predictions apply only to data collected in single experimental sessions. It is assumed that either the quantum criterion, or the value of *Q* may vary from session to session. In the event that either of these kinds of variability occurred, data combined from several experimental sessions would be distorted from their essential character. As shown by our earlier work, session-to-session variability does not occur to any appreciable extent when practiced *Ss* use forced choice as the indicator-response.<sup>5</sup> Unfortunately, the same cannot be said

<sup>4</sup> *Ibid.*, 132-136.

<sup>5</sup> *Ibid.*, 21-33.



for data obtained with phenomenal report as the indicator-response, hence this restriction on the applicability of the quantum theory is probably necessary.

Restricting the theory to data from individual sessions is particularly troublesome if we wish to establish positively the adequacy of the quantum theory, *i.e.* if we wish to reject other assumptions of the form of threshold-data. Since Fechner, many investigators have assumed that threshold-data will conform to a normal ogive, or integral of a normal frequency distribution. It has been shown elsewhere that data obtained with forced choice may usually be fitted adequately with normal ogives.<sup>6</sup> This is true both for combined data and for data obtained in single experimental sessions. Since, however, the quantum theorists have not applied the theory when forced choice is used as the indicator-response, we presumably cannot use this evidence to evaluate the neural quantum theory.

Fig. 2 has been prepared to illustrate the similarity between a normal ogive and a quantal curve. In its construction, we have selected the quantal curve corresponding to the 2-quanta criterion, since this curve is the one which the quantum theorists

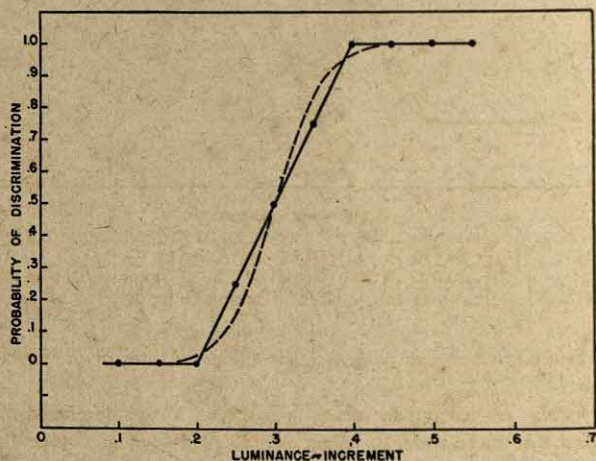


FIG. 2. THEORETICAL CURVES

The solid curve is the quantal curve based on the 2-quanta criterion; the broken curve is the most similar normal ogive.

have usually used to fit their data. The normal ogive was selected to fit the quantal curve as closely as possible. We note the comparatively great similarity between the two curves.

Other investigators, such as Crozier,<sup>7</sup> have assumed that threshold-data will be fitted by log-Gaussians, *i.e.* by ogives expressed in terms of logarithmic scales of stimulus-magnitude. The similarity between a quantal curve and a log-Gaussian may be judged from Fig. 3. The quantal curve in Fig. 3 is the one exhibited in Fig. 2.

<sup>6</sup> *Ibid.*, 42-46.

<sup>7</sup> W. J. Crozier, On the visibility of radiation at the human fovea, *J. Gen. Physiol.*, 34, 1950, 87-136.



The log-Gaussian was selected for maximal agreement with it. We note the comparatively great similarity between these two curves.

It should be apparent from Figs. 2 and 3 that comparatively large amounts of experimental data will be required to differentiate between quantal curves and either normal ogives or log-Gaussians. An analysis has been undertaken to determine exactly how many experimental data will be required for a normal ogive, or a log-Gaussian,

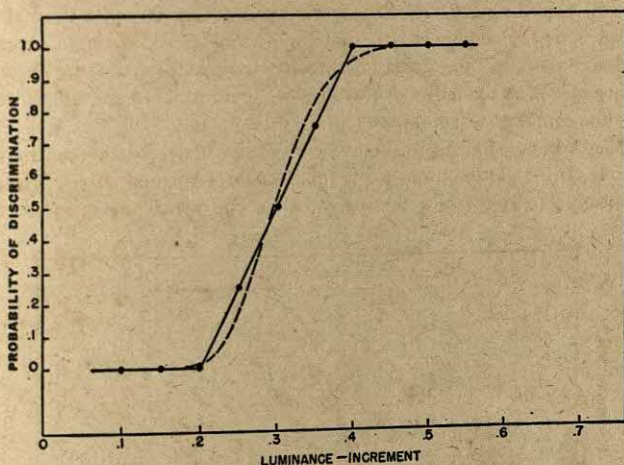


FIG. 3. THEORETICAL CURVES

The solid curve is the quantal curve based on the 2-quanta criterion; the broken curve is the most similar log-Gaussian.

to be inadequately fitted to data which satisfy the predictions of the neural quantum theory exactly.

The analysis was carried out by means of a special  $X^2$ -test developed by Kincaid.<sup>8</sup> It was assumed that there were 10 'experimental' points, equally spaced in terms of stimulus-magnitude, which fell exactly on the quantal curve exhibited in both Figs. 2 and 3. First a normal ogive, and then a log-Gaussian, was fitted to the 'data' by the *probit analysis*. This technique has been recently described by Finney,<sup>9</sup> in terms of insect mortality rates but it may readily be adapted for use with psychophysical data. The procedure involves fitting a curve to the experimental data in accordance with Fisher's principle of maximum likelihood. It differs from the familiar *constant process* by being reiterative where the constant process consists of a direct solution based on a first approximation. The probit analysis yields a value of  $X^2$ , representing the goodness of fit of the experimental data to the theoretical curve. The curves in Figs. 2 and 3 are actually the curves fitted to the 'data' by the probit analysis.

The special  $X^2$ -test provides us with a value of  $n$ , the number of experimental data at each of the 10 stimuli which we must use in order for a theoretical curve to be

<sup>8</sup> See Blackwell, *op. cit.*, 83-91; 104-106.

<sup>9</sup> D. J. Finney, *Probit Analysis*, 1947, 256.



inadequately fitted to the experimental 'data.' It is possible to compute the value of  $n$  for each of various values of  $k$ , the number of individual sets of data. Results of the analysis are summarized below:

Normal ogive		Log-Gaussian	
$k$	$n$	$k$	$n$
1	122	1	112
2	94	2	86
3	83	3	76
$\infty$	40	$\infty$	36

It is clear that no matter how many sets of experimental data are available, at least 36 presentations must be made at each of 10 stimulus-values in each experimental session if normal ogives and log-Gaussians are to fail to fit the data, even though the data actually conform precisely to the predictions of the quantum theory. If only one set of experimental data are available, more than 100 presentations at each of the 10 stimulus-values will be required. These figures reveal how difficult it will be to prove the adequacy of the quantum theory positively, so long as it is not possible to combine data from several experimental sessions.

The situation is worsened by the fact that no adequate procedure exists for fitting quantal curves to experimental data. It is a simple matter to fit a straight line to experimental data, but it is quite another matter to fit three straight lines to them, as is demanded by the quantum theory. The quantum theorists have apparently not solved this important problem.

Fortunately, there are two special predictions of the quantum theory which provide very sensitive tests of its adequacy. The first of these is the prediction that probabilities of discrimination equal to 0% and 100% will occur at low and high stimulus-values, respectively. If the theoretical probability has either of these values, absolutely no experimental departures from these values are allowable. This prediction of the theory provides a very sensitive basis for its verification. Unfortunately, the quantum theorists recommend discarding all experimental probabilities below 0.03 and above 0.97, hence this test of the theory may apparently not be used.

It is probably unreasonable to expect to use this prediction of the theory to test it. It is difficult to imagine that an  $S$  can maintain such perfect attention to the experiment that no departures from the expected probabilities ever occur. Furthermore, in vision experiments, at least, we may expect that  $S$  will occasionally blink or fail to direct his eye in the required direction. Such aberrations would result in some few missed responses at high luminances, which would provide a spurious basis for rejection of the neural quantum theory.

The second special feature of the theory is that it predicts the relation between mean and slope of the curve fitted to the psychophysical data. The prediction made by the theory is that there are only a few simple relations which can occur between mean and slope. These relations correspond to the different possible quantal criteria. To clarify this point, let us define a quantum index,

$$Q = S_1 / (S_1 - S_0) \dots\dots\dots [1]$$

where  $S_1$  is the smallest stimulus at which 100% discrimination occurs; and  $S_0$  is the largest stimulus at which 0% discrimination occurs. The integral values of  $Q$



represent quantal criteria of one, two, or three, quanta. Restricting allowable values of  $Q$  to integral values provides a comparatively sensitive means of verifying the quantum theory.

### EXPERIMENTAL DATA

The experimental data considered here have been presented in another context elsewhere.<sup>10</sup> Each of four Ss discriminated differences in luminance under the following general conditions.

S viewed a square screen, subtending a visual angle of  $20^\circ$ , whose luminance was 4.71 foot-lamberts, with normal binocular viewing and natural pupils. He directed his eyes at a fixation-spot located in the center of the screen. The stimulus was a

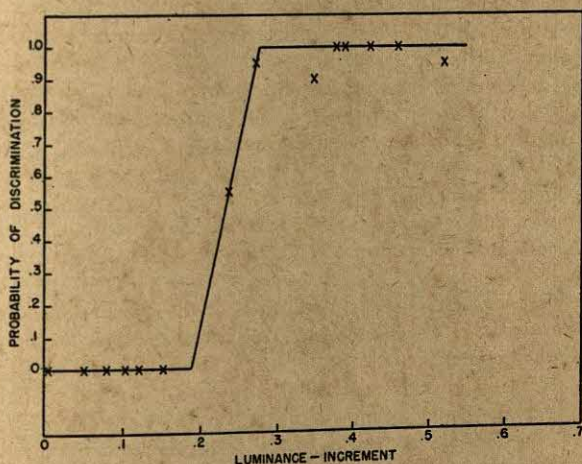


FIG. 4. EXPERIMENTAL DATA FOR S 1  
The curve is a theoretical quantal based upon a 3-quanta criterion.

circular luminance-increment, subtending  $18.5'$ , located  $7^\circ$  to the right of the fixation-spot. The increment was presented for a duration of 0.06 sec., once every 12.25 sec.

The method of constant stimuli was employed. Each of 14-18 values of the luminance-increment was presented 20 times. The 20 values of the same luminance-increment were presented consecutively, with S's knowledge. Two groups of 20 'catch stimuli' were also presented. The order of the groups of luminance-increments and 'catch stimuli' was randomized. The Ss indicated discrimination by responding 'Yes' or 'No' to each stimulus-presentation. They signaled their indicator-response with coded positions of a manual switch.

Sample data obtained in individual experimental sessions by each of the 4 Ss are presented in Figs. 4-9. The data for S 1, presented in

<sup>10</sup> Blackwell, Studies of psychophysical methods for measuring visual thresholds, *J. Opt. Soc. Amer.*, 42, 1952, 607-611.



Fig. 4, were selected from among four sets of available data as the most adequately fitted by a theoretical quantal curve. Unfortunately, the stimuli were spaced so widely over the critical range that these data are of little use in evaluating the quantum theory. We note that two experimental values are less than 100%, even though the theoretical curve predicts perfectly consistent response. These departures are probably due to losses of attention, eye blinks, or misdirection of the eyes.

Data for *S* 2, presented in Fig. 5, are selected from among 18 sets of data as the most adequately fitted by the quantal curve based upon a 2-

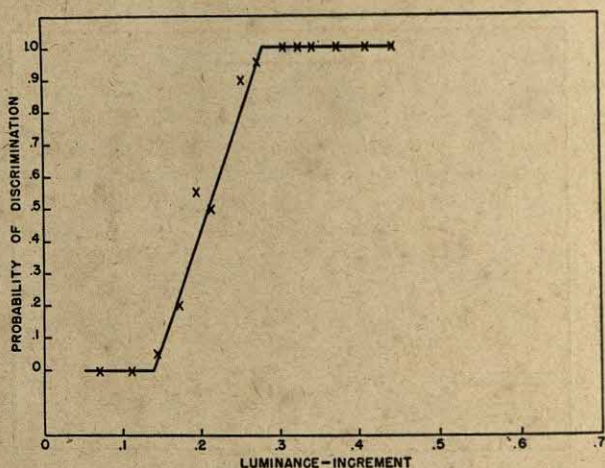


FIG. 5. EXPERIMENTAL DATA FOR *S* 2

The curve is a theoretical quantal based upon a 2-quanta criterion.

quanta criterion. Data in Fig. 6 were selected from among the same 18 sets of data as the most adequately fitted by a 3-quanta curve. The experimental data appear to fit the two theoretical quantal curves very well.

Data for *S* 3, presented in Fig. 7, are selected from among 24 sets of data as the most adequately fitted by a 2-quanta curve. The fit appears to be very good. The data in Fig. 8, however, are much more typical of the 24 sets of data obtained by *S* 2. It is impossible to fit a theoretical quantal curve to these data. We may, however, fit three straight lines without regard to the mean-slope relation. The lines constructed in Fig. 8 represent a 'quantal' curve with  $Q$ , defined by Equation (1), equal to 1.48. The 'quantal' curve appears to fit the data in Fig. 8 satisfactorily.

In Fig. 9 are presented sample data for *S* 4. Ten sets of data are available



for this  $S$ , all of which exhibit the extreme scatter evident in Fig. 9. Data as scattered as those of  $S$  4 are worthless for the purpose of evaluating the quantum theory. There is reason to believe that  $S$  4 felt obligated to randomize his responses, even though the stimulus-magnitudes were blocked.

The data for  $S$ s 1 and 4 cannot prove useful to us in our attempt to evaluate the quantum theory. We may inquire to what extent the data for  $S$ s 2 and 3 allow us to accept or reject the quantum theory. We may begin by noting that data such as those presented in Fig. 8 are entirely incompatible with the theory. The existence of such data allows us to conclude

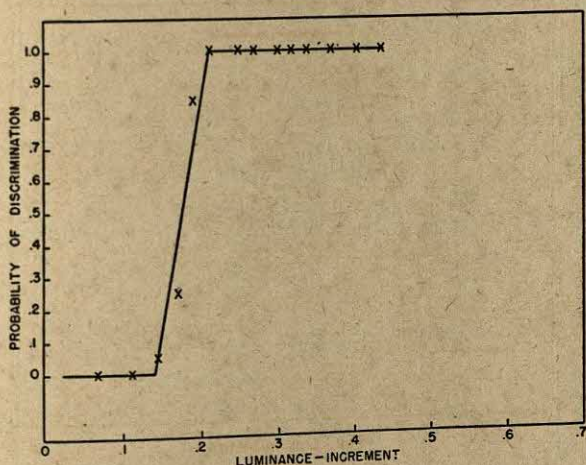


FIG. 6. EXPERIMENTAL DATA FOR  $S$  2

The curve is a theoretical quantal based upon a 3-quanta criterion.

that threshold-data collected under the general conditions specified by the quantum theorists do not always conform to predictions made by the theory. The data obtained by  $S$  3 which do fit quantal curves acceptably are sufficiently rare to be explained by chance. Visual fits of 'quantal' curves to the 24 sets of data obtained by this  $S$  suggest that values of  $Q$  vary from 1.13 to 2.0, with the greatest frequency at 1.5. Only one set of data, those shown in Fig. 7, were best fitted by a 'quantal' curve with  $Q$  equal to 2.0. The existence of this one set of data which apparently conforms to quantal predictions does not, however, constitute much evidence for the theory.

All the data obtained by  $S$  2 may be fitted by 2- or 3-quanta curves about as well as the data exhibited in Figs. 5 and 6. The data obtained by this



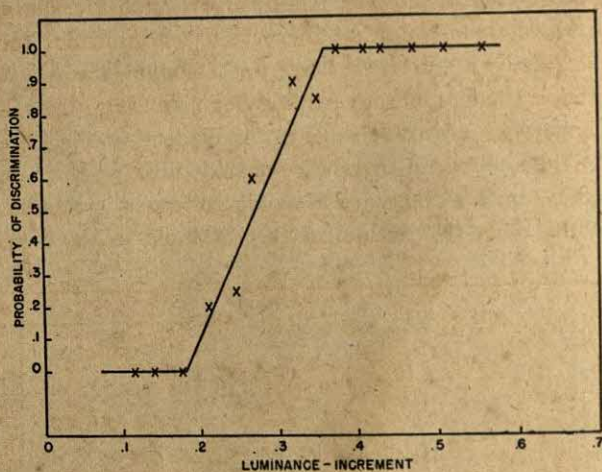


FIG. 7. EXPERIMENTAL DATA FOR S 3

The curve is a theoretical quantal based upon a 2-quanta criterion.

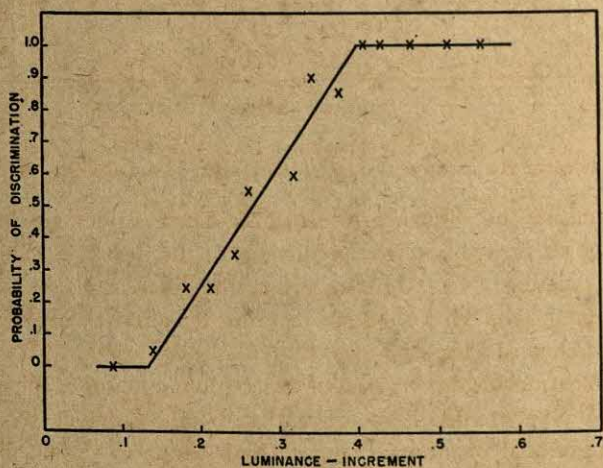


FIG. 8. EXPERIMENTAL DATA FOR S 3

The curve is a 'quantal' with  $Q = 1.48$ .



S appear at first glance to offer strong support to the quantum theory.

The apparent confirmation of the theory is probably spurious. This assertion is based upon failure of the data to conform to a further prediction which may be derived from the quantum theory. When the quantal criterion changes, all other things being constant, the value of the threshold undergoes a gross change, as is evident in Fig. 1. The data presented in Figs. 5 and 6 may be compared directly. Their exact form suggests that a 2-quantal criterion was employed in one case and a 3-quantal criterion in the other. The value for the threshold in the 2-quantal case

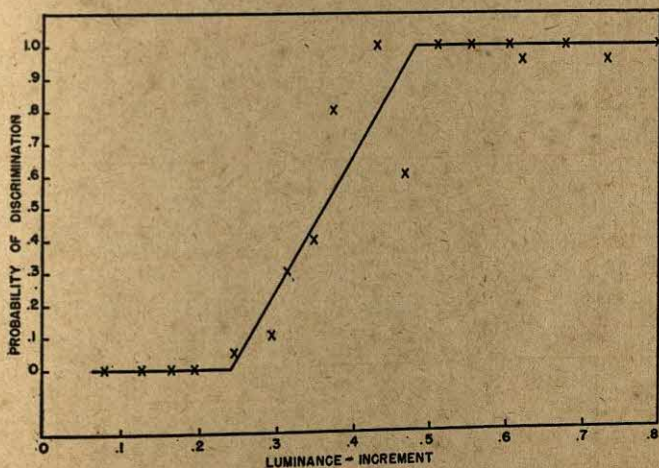


FIG. 9. EXPERIMENTAL DATA FOR S 4

The curve is a theoretical quantal based upon a 2-quantal criterion.

is 0.210 and in the 3-quantal case, 0.176. These values are much too similar to be reconciled with the predictions of the theory, as exhibited by Fig. 1. As a matter of fact, the *direction* of the difference is the reverse of what was predicted. This suggests that the quantum theory is entirely inadequate to describe the data of S 2.

The explanation for this reversal of expectation, in terms of the quantum theory, would apparently have to be that  $Q$ , the stimulus required to excite an additional quantum, changed from session to session in such a way as to slightly over-compensate the change in threshold to be expected on the basis of a change in quantal criterion. Such an assumption is hardly parsimonious.

The change in the quantal criterion without the expected corollary change in the threshold may be rationalized outside the neural quantum theory in



the following way: S 2 exhibited a positive channelization of his responses. It is evident from his data that channelization became more pronounced on successive days. The data shown in Fig. 6 were obtained chronologically after those in Fig. 5. The increased extent of channelization steepened the curve and decreased the threshold. Thus, channelization is apparently responsible for the form of the data. It scarcely seems likely, however, that the data would conform to the predictions of the neural quantum theory even if channelization were absent. The expected change in threshold when the quantal criterion is changed is too large. It appears that our experimental data provide no satisfactory evidence at all in support of the neural quantum theory.

As has been suggested elsewhere, channelization may actually be responsible for the fact that some experimental data appear to conform to quantal curves.<sup>11</sup> A positive channelization of the responses will spuriously increase the probability of the 'Yes' responses which are near unity. Probabilities of 'No' responses, which are near zero, will be spuriously decreased by the *negative channelization* of the responses, defined as the tendency to give an increasing frequency of 'No' responses toward the end of blocks of stimuli for which the predominant response is 'No.' If these response mechanisms were both operative, threshold-data would be distorted in a general way which would increase their resemblance to quantal curves.

### CONCLUSIONS

It appears that visual threshold-data, obtained with the general psychophysical procedure advocated by the quantum theorists, do not confirm predictions based on the neural quantum theory. It is suggested that the psychophysical procedure specifically advocated by the quantum theorists is poorly suited to the purpose, since the procedure permits an invalid mode of response which may distort the form of the threshold-data.

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<sup>11</sup> Blackwell, The influence of data collection procedures upon psychophysical measurement of two sensory functions, *J. Exper. Psychol.*, 44, 1952, 306-315.



## SPEED OF ACQUIRING A SIMPLE MOTOR RESPONSE AS A FUNCTION OF THE SYSTEMATIC TRANSFORMATION OF KNOWLEDGE OF RESULTS

By EDWARD A. BILODEAU, Lackland Air Force Base

When an operator of some device makes a response it is customary that some stimulus-event follow closely in time and that this event enable the operator to assess the consequence of the response. This process is often called knowledge of results. If, for example, turning a handle of the two-hand coördinator moves the tracking contact away from the target-button, the operator who perceives this should then decide to rotate the handle in the opposite direction. Knowledge of results allows *S* to correct a response so as to achieve some goal. Quite often the goal is one of reducing the discrepancy between the responding member of the body or apparatus and the goal-stimulus. Reducing the discrepancy may involve, for example, getting a pointer or contact closer in space, time, or number to the goal. In any of the above stimulus-response systems there exists a true discrepancy and a reported discrepancy. Within the limits of apparatus-error the discrepancy reported is usually equal to the true discrepancy but this situation does not always obtain. (The discrepancy reported can be defined as the difference between the goal-stimulus and the responding member, reported to *S* by *E* or any other scoring instrument.) In some types of situations the discrepancy reported can bear any relation whatever to the true discrepancy. If the hypothesis holds that *S* acts in accord with what he accepts as the discrepancy associated with given responses, then experimental manipulation of the discrepancy should lead to systematic effects upon behavior.

One way of testing this hypothesis is to devise a situation where the learning of the required response is contingent upon a numerical score given *S* by *E*. Thus, the score reported after each response must be the condition sufficient to acquire the proper response. With such a restriction placed upon knowledge of results, it should be fairly obvious that the reported-score, hence, the goal-discrepancy, is directly under experimental control.

Knowledge of results is often provided in the way described by the line  $1.0 X + 0$

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\* Accepted for publication August 26, 1952. From the Human Resources Research Center, Lackland Air Force Base, San Antonio Texas. The opinions or conclusions contained in the present report are those of the author. They are not to be construed as reflecting the views or indorsement of the Department of the Air Force.



of Fig. 1, where the independent variable is the true-score ( $X$ ) achieved and the dependent variable is the reported-score ( $Y$ ). Whenever these conditions are present, the reported-score is identical with the true-score for  $Y = X$ . For example, if a score of 200 is arbitrarily set as a goal-score, and  $S$ 's response obtains a true-score of 50, 50 is reported.  $S$  may assume that he is 150 units from his target (goal discrepancy), and that four times as much response is necessary for a score of 200.

Of many linear possibilities available the remaining equations of Fig. 1 represent a sample of one particular class. These transformations of true-score to reported-

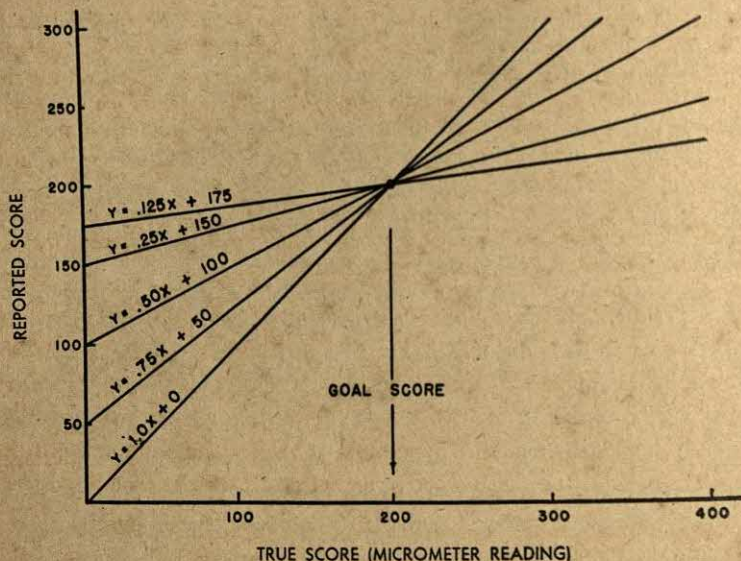


FIG. 1. LINEAR TRANSFORMATION OF TRUE SCORES TO REPORTED SCORES

When a true score of 200 is made, the reported score is 200 for each transformation represented.

score represent systematic variations in both the slope and intercept, with a constant target. If the transformation  $Y = 0.125 X + 175$  is used as one experimental treatment as opposed to a condition  $Y = 1.0 X + 0$ , two different effects on the reported-score will result: amount of response constant, group  $Y = 0.125 X + 175$ , (a) will obtain a reported-score closer to the goal-score, and (b) will approach the goal-score more slowly.

Let it be assumed, for example (a) that 200 is the goal-score; and (b) that, for such a situation, the true-score 20 is made by both treatment groups  $Y = 0.125 X + 175$  and  $Y = 1.0 X + 0$ . Then the reported-scores will be as follows:

Group	X (True-score)	Y (Reported-score)
$Y = 1.0 X + 0$	20.0	20.0
$Y = 0.125 X + 175$	20.0	177.5



The reported-score is then 20.0 for one group and 177.5 for the other. If  $S$ s actually assume a reported-score, true-score relationship of  $Y = 1.0 X + 0$ , and utilize their reported-scores towards a solution, they can predict for the subsequent trial the amount of movement necessary for the goal-score. If  $M$  is the extent of movement for group  $Y = 1.0 X + 0$ , then

$$M/20 = X/200 \dots\dots\dots [1]$$

and if  $M$  is the extent of movement for group  $Y = 0.125 X + 175$ , then

$$M/177.5 = X/200 \dots\dots\dots [2]$$

Substituting a value of one (1.0) for  $M$  in each case and solving for  $X$ , it can be seen that values of 10 and 1.13 are obtained for [1] and [2], respectively.  $S$ s for the regimen  $Y = 1.0 X + 0$  expect that 10 times as much response will achieve the goal-score, whereas  $S$ s of the other group expect that 1.13 times as much response will achieve the goal-score. Unfortunately, this solution will not lead to the goal-score for group  $Y = 0.125 X + 175$  because 175 (not a value of 0.0) is the intercept for a true-score of 0.0.

Thus, if  $S$ s tacitly or otherwise assume a reported-score vs. true-score relationship of  $1.0 X + 0$ , and some other constants obtain, such  $S$ s will be confronted with an unexpected turn of events when their scores are finally reported.

The experiment which follows describes the behavior of groups which have been treated as indicated in Fig. 1 and suggests that, as a first approximation, the behavior is characteristic of individuals who (a) have assumed a relationship  $Y = 1.0 X + 0$  between true-score and reported-score, and (b) extinguish this hypothesis at rates commensurate with the discrepancy between the goal-score and that score which is reported.

(a) *Subjects.* Two hundred basic airmen trainees at Lackland Air Force Base were divided unsystematically and equally into five groups. None of these  $S$ s had had previous experience with the test or with a micrometer. All had completed at least eight years of elementary school.

(b) *Apparatus.* The apparatus was a micrometer (size 3 to 4 in.) mounted behind a wooden screen so that its turning knob projected through a small, round aperture in the middle of the screen.<sup>1</sup> A chair was oriented at a right angle to the plane of the screen and faced a point of fixation upon a distant wall. The micrometer scale, seen only by  $E$ , was read after each response by withdrawing the micrometer momentarily from the screen aperture and then reset to zero before the next response was allowed. Tables of score transformations were provided for  $E$  behind the screen.

(c) *Experimental procedure.* The five groups differed with respect to the manner in which true-scores ( $X$ ) were transformed to reported-scores ( $Y$ ). The linear transformation equations were as follows:  $Y = 1.0 X + 0$  (Group 100),  $Y = 0.75 X + 50$  (Group 75),  $Y = 0.50 X + 100$  (Group 50),  $Y = 0.25 X + 150$  (Group 25) and  $Y = 0.125 X + 175$  (Group 12.5).

<sup>1</sup> It should be fairly obvious that the experiment which follows is not concerned with the learning of a micrometer response qua micrometer response. The reason for selecting the micrometer as a task was one merely of laboratory utility.



The *Ss* were run one at a time in a simple counterbalanced order, 100, 75, 50, 25, 12.5, 12.5, 25 . . . 100, until all had been tested. All groups were equated with respect to the number of reported-scores that were possible. This was done by rounding the true-score to the nearest ten before transforming the score. Thus, for all groups 20 was the total number of possible reported-scores between 0 and 200.

The instructions were carefully standardized and communication between *E* and *S* was limited as much as possible to instructions and reported scores.

In essence the instructions explained that on each trial (a) a score of 200 was the goal-score; (b) the more turns given the knob, the higher the score; (c) the knob could be turned as many times as *S* thought necessary to obtain a score of 200; and (d) a standard grip and turning motion were required (thumb and tip of forefinger on the knob and wrist motion). While the instructions were given, *S* held a card upon which a scale was drawn. The scale ranged from 0.0 to 500 and the goal-score was clearly marked upon the card. The card was used as a device to help *S* understand the implication of a score for his next response.<sup>2</sup>

Each *S* was given a total of 16 trials (responses), each of which was followed by a transformed score. About 5 sec. elapsed between the response and the reported-score; and 10 sec. between the giving of the score and the signal to turn again.

To equate the groups as to initial mean and variance one important restriction was placed upon the first of the 16 responses. The *Ss* were told, "Now, just to try the knob out; when I say, 'turn,' I want you to turn the knob around once—one full turn—and we'll see what practice score you get." Once around with the knob ( $360^\circ$ ) actually equals a true-score of 25. Consequently, for each group eight  $360^\circ$  turns were necessary to achieve a score of 200. The 'once around' technique on Trial 1 allowed for (a) comparable true-scores on Trial 1 (*ca.* 25); (b) a sizable deviation from the goal-score (learning possibilities); and (c) room for the action of any hypothesis behavior.

Counting the number of knob turns was neither encouraged nor discouraged in *S*.

(d) *Scoring.* The micrometer reading constituted the response measure or true-score. It was necessary to run 247 *Ss* to obtain the required 200 valid cases. True-scores on Trial 1 beyond  $25 \pm 12.5$  constituted a basis for voiding individuals (21 gave more than  $1\frac{1}{2}$  turns and 21 gave less than a half a turn). This arbitrary criterion was chosen to reduce the number of *Ss* who did not understand the concept, one turn, or the instructions in general. Those *Ss* (5 in number) with true-scores of less than 150 on Trial 16 were also voided. The voiding criteria were established before conducting the experiment.

Each true-score (*X*) was converted to a deviation from the goal-score (200), *i.e.*  $X - 200 = \text{deviate}$ . Thus a negative mean indicates undershooting the goal-score (not enough knob turning), and a positive mean indicates that the knob was turned too far.

*Results.* The mean absolute deviations from the goal-score on successive trials are plotted in Fig. 2. All curves are negative in trend indicating that the deviation from target-score was progressively reduced as a func-

<sup>2</sup> This scale does not imply a one to one relation of reported-scores to true-scores. Nevertheless, it is conceivable that some *Ss* at first infer from the card that  $Y = X$ .



tion of successive trials. Each group begins with a mean deviate of about 178, and each group ends with a target deviation of about 12. Inasmuch as the empirical points are so regular in trend, the means for each group are represented by curves drawn by inspection. A family of curves is suggested; each member with the same origin and apparently the same per-

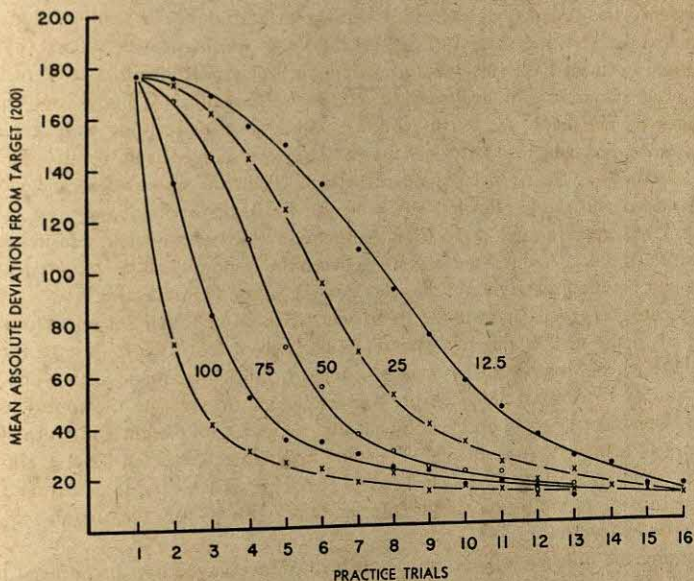


FIG. 2. MEAN ABSOLUTE DEVIATION OF TRUE SCORES FROM GOAL SCORE, PLOTTED AGAINST SUCCESSIVE TRIALS

The curve for each group has been drawn by inspection. See text for an explanation of the empirical points omitted.

formance asymptote.<sup>3</sup> Using accuracy of response as a criterion of learning, the groups are ranked from the one with greatest slope (largest value of *a*) and smallest intercept (smallest value of *b*) to the one with least slope and largest intercept, *i.e.* (1) 100, (2) 75, (3) 50, (4) 25, and (5) 12.5. Group 12.5 appears ogival in trend whereas Group 100 appears negatively accelerated throughout.

The data can be treated in terms of algebraic deviations since some

<sup>3</sup> Since the differences between means for Trials 1, 14, 15, and 16 were too small to be shown graphically, the grand mean is the value plotted for Trial 1 and means for the Groups 12.5 and 100 are shown for Trials 14, 15, and 16. It should also be noted that the curves for Groups 50 and 100 have not been extended beyond Trial 13.



scores over 200 were obtained. In general, the less the mean absolute deviation (see Fig. 2), the greater the number of true-scores over 200; that is, positive deviations occurred more frequently near the performance asymptote. Since there were few such scores, however, plots of mean algebraic deviation resembled closely those of Fig. 2. The true-score stand-

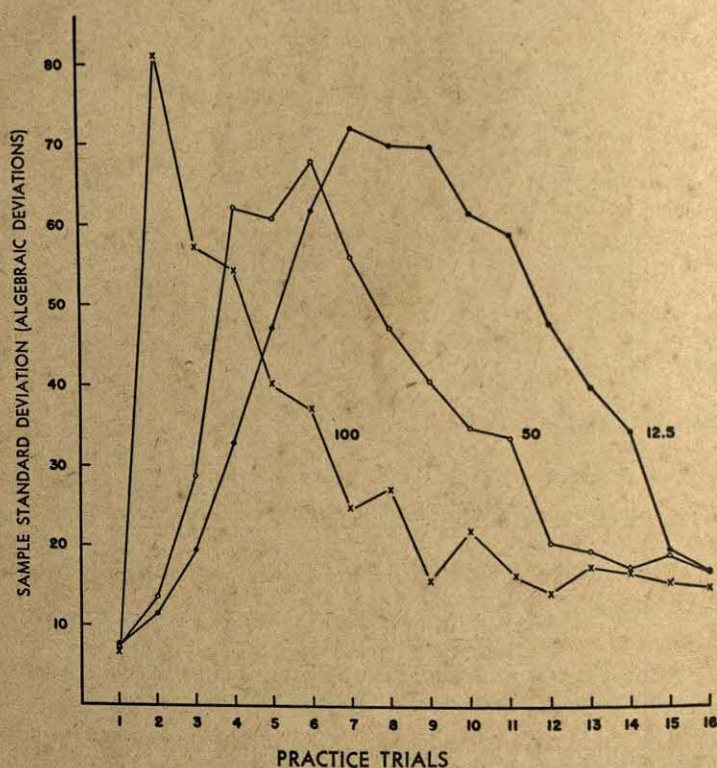


FIG. 3. STANDARD DEVIATION OF TRUE SCORES FOR GROUPS 100, 50, 12.5  
PLOTTED AGAINST SUCCESSIVE PRACTICE TRIALS

To simplify the figure, Groups 75 and 25 were omitted.

ard deviations are of greater interest, and these are plotted for Groups 100, 50, and 12.5 in Fig. 3. The trend of all groups is an increase in variability followed by a decrease. The maximal variability is approximately 10 times the variability of Trial 1, and the locus of the maximum is a function of the experimental treatment. Thus maximal variability is obtained on Trial 2 for Group 100, but is obtained on Trial 7 for Group 12.5. The trial as-



sociated with maximal variability for the unplotted groups is 4 and 6 for Groups 75 and 25, respectively. It is possible to conclude (1) that there are non-monotonic changes in variability as a function of practice trials, and (2) that the point of maximal variability varies directly with departure from the condition  $Y = 1.0 X + 0$ . The standard deviations sug-

TABLE I  
FREQUENCY OF Ss WITHIN CLASS-INTERVALS OF 20 ON  
TRIAL 2 FOR EACH GROUP

Class interval From To	Groups				
	12.5	25	50	75	100
-199-180*	18	9	9	2	1
-179-160	19	29	19	3	4
-159-140	2	2	11	14	3
-139-120	1	—	1	9	—
-119-100	—	—	—	8	—
-99-80	—	—	—	3	3
-79-60	—	—	—	1	4
-59-40	—	—	—	—	6
-39-20	—	—	—	—	5
-19-0	—	—	—	—	4
+1+20†	—	—	—	—	—
+21+40	—	—	—	—	4
+41+60	—	—	—	—	1
+61+80	—	—	—	—	4
+81	—	—	—	—	1

\* Upper interval of negative deviations. † Lower interval of positive deviations.

gested that the shape of the true-score distributions should be examined carefully. Plots of these distributions for each group for each trial suggested the following:

(1) The scores for Trial 1 are probably normally, or at least symmetrically, distributed.

(2) The scores of intermediate trials are at first skewed toward positive and then toward negative deviations. The trials of maximal skewness are closely related to the variability maxima. For example, maximal skewness for Group 12.5 occurs after the maximal skewness for the other groups has been reached.

(3) The scores for the terminal trials approach normality or symmetry. For illustration, the entries of Tables I and II are presented to suggest the shapes of the score distributions early in practice (Trial 2) and for an intermediate trial (Trial 8). The entries of these tables are the number of Ss of each group falling within true-scoring deviate intervals of 20, the scale ranging from maximal negative deviations of 199 to a maximal posi-



tive deviation of over 81. Examination of the entries of Table I shows the frequency of deviate scores for Group 12.5 to be clustered around high negative deviations, and the deviate-frequency dispersion to increase regularly from column to column (as Group 100 is approached). There is a suggestion of non-unimodality for Group 100 or a tendency for one major grouping of scores about zero (the goal-deviate) and another, though smaller, about a larger deviate.<sup>4</sup>

A glance at Table II for the frequencies at Trial 8 will illustrate a marked rearrangement of the distributions both within and between groups.

TABLE II  
FREQUENCY OF Ss WITHIN CLASS-INTERVALS OF 20 ON  
TRIAL 8 FOR EACH GROUP

Class interval From To	Groups				
	12.5	25	50	75	100
-199-180*	—	—	—	—	—
-179-160	6	2	1	—	—
-159-140	6	1	1	—	—
-139-120	3	2	1	1	—
-119-100	4	2	—	—	—
-99-80	5	5	—	—	—
-79-60	1	1	1	1	—
-59-40	2	1	3	—	3
-39-20	5	5	5	6	5
-19-0	3	6	12	13	10
+ 1+20†	3	10	12	14	14
+ 21+40	1	1	2	2	5
+ 41+60	—	4	2	2	2
+ 61+80	—	—	—	—	—
+ 81	1	—	—	1	1

\* Upper interval of negative deviations. † Lower interval of positive deviations.

Group 12.5 now suggests bimodality and Group 100 unimodality—with the intermediate groups fairly appropriately distributed between the extremes.

The bimodality shown above, although based on very few cases, indicates within-group differences in speed of appropriately utilizing the information provided. That is, some of the Ss appear capable of making use of the reported-score much more quickly than do other Ss of the same group. Further, as distortion of true-scores increases, the trial at which the two classes of within-group scores becomes distinguishable is delayed.

<sup>4</sup> Possibly this curious distribution is trimodal, *i.e.* see the negative class intervals 179-160, 59-40, and the positive class interval 41-60. For the entire distribution the mean (-41.6) and median (-40.9) are fairly similar.



*Predictions of scores for Trial 2.* The hypothesis can be tested that  $S_s$  of all groups at first, tacitly or otherwise, assume that the relationship between the response and reported-score is  $Y = 1.0 X + 0$ . To make the calculation it was necessary to use the true-scores of the individuals on Trial 1. Thus a separate prediction was made for every  $S$  of each group in order to take account of the motor error associated with knob turning. To illustrate the calculation, let it be assumed that an  $S$  from Group 12.5 has obtained a true-score of 22.45. This score was reported as 178. If this hypothetical  $S$  has assumed that one full turn results in a score of 178 and 200 is the goal-score, then  $1/178 = X/200$ ,  $X = 1.12$  and 1.12 turns should be required to obtain a score of 200. Since the best estimation of the true-score-equivalent of what this  $S$  thinks is one full turn is  $1.12 \times 22.45$  (22.45) = 25.14 is the predicted true-score for the subsequent trial. In deviate terms this score is  $-174.86$ . When the

TABLE III  
PREDICTED AND OBTAINED TRUE-SCORE DEVIATIONS FOR TRIAL 2 WITH A  $t$ -TEST  
OF THE ADEQUACY OF THE PREDICTION TECHNIQUE

Group	Predicted algebraic mean	Obtained algebraic mean	Diff.	$t$
12.5	-175.91	-176.48	0.57	0.40
25	-170.97	-173.05	2.08	2.49*
50	-161.66	-166.82	5.16	3.51†
75	-135.49	-135.40	-0.09	0.03
100	+ 8.37	- 41.62	49.99	3.81†

\* Significant beyond the 5% level,  $df = 39$ .

† Significant beyond the 1% level,  $df = 39$ .

above procedure is repeated for the members of Group 12.5, the mean predicted Trial 2 deviate is  $-175.91$ . The obtained deviate is  $-176.48$ . The difference between these two values is not significant by the  $t$ -test when evaluated by means of the standard error of the difference between true and predicted scores. Both predicted and obtained true-score deviates for Trial 2 are given in Table III along with the appropriate  $t$  for each of the experimental groups. Three of the ratios are significant beyond the 5% level. These  $t$ s can be interpreted to mean that the prediction technique is probably not adequate for retention of the null hypothesis. The percentages of error in the prediction of the mean of the true-score distribution are  $+2.4$ ,  $+7.7$ ,  $+15.6$ ,  $-.1$ , and  $+31.6$  for groups 12.5, 25, 50, 75, and 100, respectively. These errors may be tolerable at present for all groups, except 100. Further research with refinement of experimental procedure and the prediction technique may reduce the error.

*Discussion.* It is possible to distinguish between two general sources of error, motor and judgmental, which are confounded in the treatments of the present study. Roughly speaking, there is a difference between what an  $S$  wants to do and what he actually does with respect to amount of knob turning. A future experiment where a scale is mounted alongside the micrometer knob to reduce the turning error is certainly justifiable. The



present discussion, however, is primarily concerned with the part played by hypothesis in *S*'s total behavior.

The groups were defined by a concomitant variation in both intercept, *b*, and rate of approach, *a*, to the goal-score. Obviously, the value of *a* varied inversely with that of *b*. If the groups had been defined by a variation in the value of *b* alone, then by necessity different goal-scores would have to have been set up for each group. The same would be true with *b* held constant and several values of *a* as the experimental treatment. It is reasonable to suppose that the particular level of goal-score used for any group is of some behavioral import, and this problem is presently under investigation.

The results obtained so far have indicated that the speed of acquisition of the goal-response (eight turns of the micrometer) varies directly with the magnitude of goal-discrepancy. That is, the larger the difference between the reported-score and the goal-score, the faster a group learns to give the knob approximately eight full turns. Groups given initial scores closer to the goal-score, however, will in time learn to position the knob about as accurately as groups given initial scores more removed from the goal-score.

Why the delay in reducing the goal-discrepancy increased with smaller values of *a* and larger values of *b* can be at least superficially interpreted as following from the deceptive character of the score-transformation where *a* is small and *b* is large. Such a transformation is deceptive provided individuals expect (habits, stereotypes of the relation of indicated-scores to true-scores brought to the experimental situation) a simple additive effect of the response. If early in practice the hypothesis  $1.0 X + 0$  is employed by the *S*s of condition  $0.125 X + 175$ , the indicated nearness to the goal-score (since  $b \neq 0$ ) may be the factor misleading *S* about the required response. In other words, during the early trials it is as if the value of *b* is more potent than the value of *a*. With succeeding trials, however, the failure to obtain a favorable rate of approach to the goal-score is in conflict with *S*'s hypothesis about the value of *a*. Apparently several trials are required for *S* to respond as if *a* were not equal to unity. Either *S* interprets the seemingly contradictory information appropriately or proceeds by trial and error. The more the *E*'s schedule departs from  $Y = 1.0 X + 0$ , it may be presumed, the more difficult it becomes to reject the  $Y = 1.0 X + 0$  hypothesis (the value of *b* again or goal-discrepancy). This accounts for the mean differences among the groups.

The results also showed marked changes in true-score variability as a function of the experimental treatment. At least three aspects of this



variability appeared to have behavioral significance. First, variability increased and then decreased as a function of practice; secondly, the point of maximal variability was related to the experimental treatment (the larger the value of  $a$ , the sooner the maximum); and thirdly, the shape of the distribution of scores varied with both trials and treatment. The observed increasing skewness or non-unimodality of the scores with trials suggested individual differences in the latency of catching-on (correct hypothesis behavior) to the number of knob turns required. With succeeding trials (as greater numbers of  $S$ s had achieved approximate solutions) variability decreased. If a problem is eventually possible of solution by all or nearly all individuals, it seems reasonable that the variability of the scores will vary according to the number of  $S$ s attaining the solution at any time. If, because of problem-difficulty, the solution for many of the individuals, say one-half, is to occur fairly late, then large variability will be fairly late. This describes why a group such as  $Y = 0.125 X + 175$  approaches its point of maximal variability more slowly than, say, Group  $Y = 1.0 X + 0$ .

The notion that the  $S$ s were operating under the  $Y = 1.0 X + 0$  hypothesis for Trial 2 was tested. Although the test resulted in the rejection of the null hypothesis for three groups, the percentage of error for the prediction was, with one exception, nevertheless small. The test was most inadequate for condition  $Y = 1.0 X + 0$  where the rationale was apparently most appropriate. This may not be surprising since the  $S$ s of Group 100, with many turns predicted on Trial 2, should be expected to make more counting errors than  $S$ s of other groups.  $S$ s' verbal reports support this expectation and also suggest that an  $S$  starting out to make the required eight turns may lose faith in his estimate and as a consequence stop turning prematurely. Four  $S$ s (10%) contributed heavily to the discrepancy from prediction by repeating the Trial 1 response. It should also be pointed out that the calculational technique involved the assumption that the Trial 1 true-score was the best estimate of  $S$ 's approximation of one full turn and that this value was also the best estimate of the extent of one turn for all turns of the following trial. This assumption receives its severest test with Group 100.

*Summary.* Five seconds after responding,  $S$ s received a score according to the schedule  $Y = aX + b$ , where:  $Y$  represents reported-score;  $X$ , true score; and  $a$  and  $b$  transformation constants. For each of five groups, (1) rate of approach to the goal-score ( $a$ ) and (2) nearness to the goal-score or intercept with a zero response ( $b$ ) was varied. On each of 16 trials, except the first,  $S$  turned a knob until he *thought* a score of 200 (goal-score) had been reached. The score reported by  $E$  after every trial gave  $S$  sufficient knowledge of his results for him to learn to turn the knob the proper number of times, *i.e.* 8. For Trial 1,  $S$  was instructed to turn the knob but once to see what score would result from a single turn.



The true-score ( $X$ ), *i.e.* the response measures, were treated as deviates from the goal-score of 200. The mean deviations plotted against trials indicated (1) that there was a family of curves which approached a common asymptote as a limit; and (2) that  $S$ 's speed of acquisition varied directly with the magnitude of the slope ( $a$ ) or inversely with the magnitude of the intercept ( $b$ ).

The trends noted in the data were tentatively explained in terms of the size of the goal-discrepancy for  $S$ s who initially adopted the hypothesis that the relationship between the reported and the true score was  $1.0 X + 0$ . The suggestion was made that this hypothesis is extinguished, or replaced by others, at a rate which varies with the magnitude of the transformation constants—a suggestion which accounts for the means and the variances obtained.

The method employed in this investigation offers a new approach to an objective study of the formation of hypotheses.



## ASSIMILATION OF SEQUENTIALLY ENCODED INFORMATION

By IRWIN POLLACK, Washington, D.C.

The primary aim of this paper is to describe an approach to verbal learning that is based on the theory of information and to present the results of an illustrative experiment. The approach considered here, which is derived from the work of communications engineers,<sup>1</sup> is principally concerned with 'input-output' relationships. The basic schema is shown in Fig. 1. The outstanding feature (and the requirement often most difficult to satisfy) is a defined *message-source*—a *message-set*, composed of a defined class of possible messages, together with the rules for their selection or *choice*. After a message is chosen from the set of possible messages, it is sent by a *transmitter* over a *channel* to a *receiver* and thence to a *destination*. In terms of Fig. 1, the paradigm for an experiment on the immediate recall of verbal materials is as follows: the *message-set* is composed of identifiable verbal symbols—English consonants and numerals; *choice* is based on a table of random numbers; the *transmitter* is a talker; the *channel* is the path of the sound waves; the *receiver* is an *S* who listens to the message and, after the message has been presented, records it at the destination—on the answer sheet.<sup>2</sup>

*Informational input.* With the schematic framework outlined above, let us first consider the method of numerically specifying the learning materials

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<sup>1</sup> R. M. Fano, The transmission of information: I. *Res. Lab. Electronics, Mass. Inst. Tech., Technical Report*, No. 65, March, 1949; No. 149, Feb. 1950; C. E. Shannon, A mathematical theory of communication, *Bell Syst. Tech. J.*, 27, 1948, 379-423; Communication theory of secrecy systems, *ibid.*, 28, 1949, 656-715; Memory requirements in a telephone exchange, *ibid.*, 29, 1950, 343-349; Prediction and entropy of printed English, *ibid.*, 30, 1951, 50-64; W. G. Tuller, Theoretical limitations on the rate of transmission of information, *Res. Lab. Electronics, Mass. Inst. Tech., Technical Report*, No. 114, April 1949; N. Wiener, *Cybernetics*, 1949, 74-81.

<sup>2</sup> For many psychological problems, the exact description of the component parts of the entire informational system is often impossible. For example, we are often unable to distinguish between the limits of the transmitter and the channel. This, however, does not disallow the informational approach because we can generally identify the input and the output of the receiver.



presented to *S*. We shall consider a measure of the message-source rather than the individual messages selected. The measure will be the amount of information that *potentially* can be transferred from the message-source to *S* and it will be called the *informational input*. We first ask the following question: "After the transmitter chooses a message from the message-set, and *before* the particular message is transmitted, what is the uncertainty

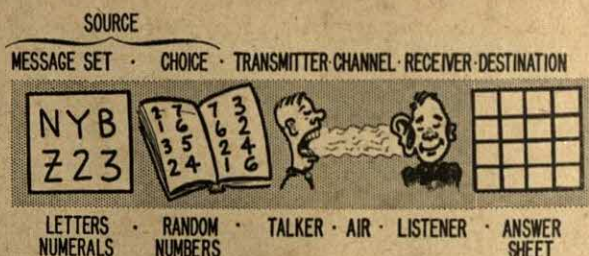


FIG. 1. SCHEMA FOR THE IMMEDIATE RECALL OF VERBAL MATERIALS IN TERMS OF A COMMUNICATION SYSTEM

of the *receiver* in specifying the particular message selected by the transmitter?" This *a priori* uncertainty of the receiver is a measure of the informational input.<sup>3</sup> Operationally, the question of the receiver's uncertainty is equivalent to asking: "What is the minimal number of selections, decisions, or choices, by the receiver that would be necessary to specify any

<sup>3</sup>In terms of a better known psychological experiment, the concept of the receiver's uncertainty in a communication system, before a message is transmitted, is operationally equivalent to the receiver's uncertainty of specifying the correct card in an ESP experiment. In either case, the receiver's uncertainty will be a function of the statistical characteristics of the source (message-set plus the rules for selection from the set). The receiver's uncertainty will be minimal when he has obtained full knowledge of the statistical characteristics of the source. It is not *necessary*, however, for the receiver to have such knowledge. He may be required to learn the statistical characteristics of the source, or he may incidentally learn the statistical characteristics of the source (See D. A. Grant, H. W. Hake, and J. P. Hornseth, Acquisition and extinction of a verbal conditioned response with differing percentages of reinforcements, *J. Exper. Psychol.*, 42, 1951, 1-5.) In the present study, we attempted to short-circuit this step by specifically giving *S* full knowledge of the message-set and the rules of selection from the message-set.

Emphasis is placed on the statistical characteristics of a message-source since, if a source is to convey information, it *must* have statistical properties. Stated otherwise, (our best description of) the output of an informational source is a set of probability distributions. If the output of the source can be exactly described by a deterministic statement, our source will convey no information, because once the deterministic statement is attained there is no uncertainty of specifying any selected message from the source. It may be noted that most learning experiments require *S* to learn the deterministic statement of a *fixed*, rather than a statistical, message-source—for example, the usual serial learning experiments employing the method of anticipation.



particular message in the message set?" The average number of binary decisions necessary is taken as the measure of the informational input to the receiver. The binary selection has been called the *bit*.

The manner of making the selections from a class of possible messages will be illustrated by a few examples. We shall first consider the particular case where all messages have an equal probability of occurrence and the size of the class of messages is an integral power of two. If we have a class of two possible messages,  $N$  and  $Y$ , how many binary selections are necessary to specify either member of the class? Only one. The first decision is: is it  $N$ ? If so, the message is specified. If not, then the other message of the class,  $Y$ , is determined.<sup>4</sup> Similarly, if the class of messages consists of four equally-likely messages,  $B$ ,  $N$ ,  $Y$ , and  $Z$ , then a minimum of

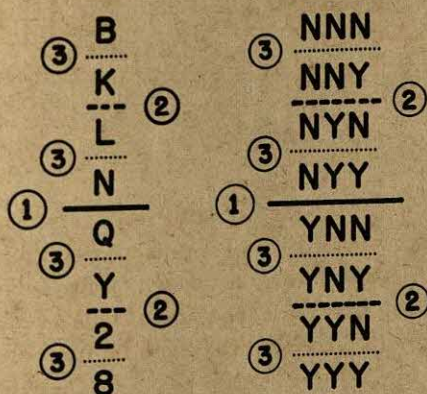


FIG. 2. SCHEMATIC ILLUSTRATION OF THE METHOD OF MAKING SUCCESSIVE BINARY SELECTIONS FROM A MESSAGE-CLASS

On the left is a message-class of eight equally-likely messages, each one unit long, each unit chosen from among eight alternatives. On the right is a message-class of eight equally-likely messages, each three units long, each unit chosen from among two alternatives. The dividing lines indicate the number of successive binary selections.

two simply binary selections—two bits—are necessary to resolve the uncertainty of specifying any one message. The first decision tells us to which pair ( $B$ ,  $N$ ) or ( $Y$ ,  $Z$ ) the message belongs. The second decision tells which member of the pair is chosen ( $B$  or  $N$ ), ( $Y$  or  $Z$ ).<sup>5</sup> In the same way (Fig. 2) if the class consists of eight possible messages, a minimum of three binary selections—three bits—are

<sup>4</sup> The operation of choosing one from a class of two equally-likely alternatives has, in fact, been the starting point of the operational definition of information by Fano (*op. cit.*, 4). The initial treatment of the topic here closely follows Fano because of its intuitive simplicity. Most of the concepts and terminology, however, follow Shannon (A mathematical theory of communication, *loc. cit.*, 4).

<sup>5</sup> More than the minimal number of binary selections will be necessary if the message-class is not divided into sub-classes of equal probabilities of occurrence.



necessary in order to resolve the uncertainty of specifying each message of the class. In general, the minimal number of simple binary selections necessary to specify any message of a class of equally-likely possible messages is equal to the logarithm to the base two of the number of possible messages of the class.

The method of successively subdividing the message-source into classes with equal probabilities of occurrence will also yield a measure of the uncertainty associated with message-sources in which (1) the number of possible messages is *not* an integral power of two and (2) the probabilities of occurrence of the individual messages of the message-source are not equal. Individual message-units are encoded into longer message-units, and this expanded class of messages is then successively subdivided into message-groups with equal probabilities of occurrence.<sup>6</sup> By this procedure, it can be shown that the average number of binary selections necessary to specify any message of the message-source, with optimal encoding, is a weighted logarithmic transformation of the probability distributions of the members of the class of messages.<sup>7</sup>

It is convenient to distinguish between two quantities which determine the class of possible messages. The first is the number of possible, or admissible, *alternatives* ( $a$ ) that any message-unit may take. The second is the *length* ( $n$ ) of the message in terms of the number of message-units. The rôle of these two variables is illustrated in Fig. 2. In each case, we have a class of eight possible messages. On the left, any one of eight possible alternatives per unit ( $a = 8$ ) makes up a single message-unit ( $n = 1$ ). On the right, any one of two possible alternatives per unit ( $a = 2$ ) constitutes each of the three-message-units ( $n = 3$ ). The two classes of messages are equivalent informationally because the same number of binary selections is necessary to specify any message from either class of messages.

The important thing to note is that we look at the *class* of possible messages rather than at the intrinsic characteristics of the selected messages themselves, such as their associativeness, meaningfulness, or emotionality. It is this characteristic which primarily differentiates this approach from current methodologies in verbal learning. Colloquially, information is conveyed by what *can* be said, *not* by what *is* said.<sup>8</sup>

<sup>6</sup> An example of this procedure may be found in Fano (*op. cit.*, 9). The operation of 'optimal encoding' essentially involves *storage* or *delay* for the purpose of averaging or smoothing out 'peak' surges of information.

<sup>7</sup> For a discussion of the weighted logarithmic function, see W. R. Garner and Hake, The amount of information of absolute judgments, *Psychol. Rev.*, 58, 1951, 446-459. Tabulations of the weighted logarithmic function may be found in E. B. Newman, Computational methods useful in analyzing series of binary data, this JOURNAL, 64, 1951, 252-262.

<sup>8</sup> The price of quantification—disregarding the intrinsic characteristics of the selected messages—may, perhaps, be too dear for those interested in the intrinsic characteristics of verbal learning materials. On the other hand, it should be recognized that even when investigations are specifically directed at the intrinsic characteristics of verbal materials, it is usually necessary to consider the relative characteristics of the class of messages from which the selected messages are chosen. Furthermore, as Miller points out, once quantification of the class is attained by the methodology presented here, there is nothing to prevent one from examining the selected messages for further analysis with any tool, objective or subjective (G. A. Miller, Speech and language, *Handbook of Experimental Psychology*, Edited by S. S. Stevens 1951, 789-810).



*Information lost and gained.* The information lost by the receiver, here called the informational error output, or, more briefly, the *error output*, is the discrepancy between the input to the receiver and the output from the receiver. The amount of information assimilated—the *informational gain* or the amount of learning obtained—is the difference between the informational input and the information lost by the receiver.<sup>9</sup>

The method of specifying the error output is similar to that of specifying the informational input. We ask the same type of question, but we look at the system from the position of the transmitter, rather than from that of the receiver. Thus, instead of determining the uncertainty of the *receiver* in specifying the selected message *before* transmission, we determine the uncertainty of the *transmitter* in specifying the received message at the destination *after* transmission of the message.<sup>10</sup>

A schematic example of the information lost by the receiver is given in Fig. 3. On the left of Fig. 3 the condition for the perfect assimilation or gain of information is shown. For each input there is one, and only one, output. In this case, if the transmitted message to the receiver is known, the uncertainty associated with specifying

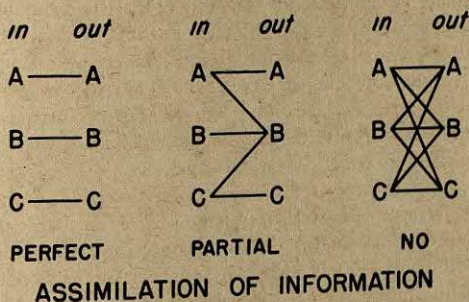


FIG. 3. SCHEMATIC ILLUSTRATION OF CONDITIONS BETWEEN INPUT AND OUTPUT MESSAGES

ing the message at the destination is zero. Stated otherwise, the information lost by the receiver is zero. The amount of information gained—the difference between the informational input and zero error-output—is equal to the input. On the right

<sup>9</sup> For many psychological applications, the terminology of Garner and Hake (*op. cit.*, 446-459) is superior to that of the present paper. In particular, their *response equivocation* is closer to psychological terminology than *information lost*. They employ *information transmitted* rather than *information gained*.

<sup>10</sup> An assumption implicit in this treatment, pointed out to the author by Dr. F. C. Frick, is that the number of possible response-categories is always equal to the number of possible message-categories. If we start out with a large number of message-categories and require responses from our receiver within a small number of response-categories, there will be a loss of information even though the receiver performs his task with perfect accuracy. To avoid this effect, special care was introduced in the present study to give the Ss full knowledge of the statistical characteristics of the message-source, and they were instructed to respond accordingly (Garner and Hake, *op. cit.*, Table II, 361).



of Fig. 3, the condition for no assimilation of information is shown. For all input-messages, all output-messages are obtained with the same probabilities of occurrence as corresponding input-messages. In this case, knowledge of the message transmitted to the receiver does not reduce the uncertainty associated with specifying the message at the destination. The information lost is equal to the informational input. The amount of information gained—the difference between the informational input and the error-input (numerically equal to the input)—is zero. A condition following between the two extremes also is illustrated. In this case, knowledge of the transmitted message reduces the uncertainty of specifying the receiver's output although it does not completely determine the output.<sup>11</sup>

*Ideal experiment and present approximation.* The ideal experiment that will satisfy the experimental paradigm requires tabulation of all possible output-messages for each input-message. Strict adherence to this requirement was not possible under the conditions of the present experiment. For example, a complete analysis of one of the conditions of the experiment would have required the enumeration of over  $10^{34}$  different categories.

The ideal experiment was, however, approximated by the method employed by Shannon in studying the information transferred by printed English<sup>12</sup>—essentially, a multiple-response technique which allows partially-learned responses to emerge. *S*'s first reproduction of the message is sequentially scanned for errors. When a message-unit of the first reproduction is not correct, *S* is instructed to make successive responses until the message-unit is corrected. In the interest of economy of time, the correct message-unit is furnished by *E* if the message-unit is not corrected within an arbitrary number of guesses (beyond which it is assumed that *S* would have been effectively guessing at random). In the present study, it was assumed that the level of performance was not appreciably higher than chance after the fourth successive incorrect response.

*Selection of the messages.* Each of the *a* admissible alternatives per message-unit

<sup>11</sup> The method employed for calculating the amount of learning is not strictly correct. If we look at the system from the input end, the amount of information assimilated,  $H_R$ , is the difference between the information associated with the source,  $H(x)$ , minus the uncertainty associated with the source when the response is known,  $H_y(x)$ . Or, looking at the system from the output end, the amount of information assimilated is the difference between the information of the output,  $H(y)$ , minus the uncertainty associated with the output when the input is known,  $H_x(y)$ . (See Shannon, *A mathematical theory of communication*, *loc. cit.*, 409.) The measure of the amount of information assimilated used in the present report is  $H(x) - H_x(y)$ . The justification for using this incorrect measure is a practical one. It is extremely difficult to obtain a measure of  $H(y)$  and  $H_y(x)$  unless the entire class of possible messages is investigated, a task which quickly becomes impossible if a wide-range functional analysis is to be performed. Fortunately, it is possible to show that  $H(y)$  will nearly equal  $H(x)$  when the entire class of alternatives is investigated. (See, Garner and Hake, *op. cit.*, Table II, 361.)

<sup>12</sup> Shannon, Prediction and entropy of printed English, *loc. cit.*, 50-64.



—English consonants and numerals—was assigned an equal number of digits in the table of random numbers. No restrictions were placed upon the selections. A series of  $n$  successive random numbers thus yielded a message  $n$  units long with each message-unit selected independently from among the  $a$  alternatives.

*Experimental procedure.* The messages were read, with as little rhythmic stress and emphasis as possible, at the rate of about 6 message-units in 5 sec. The Ss were permitted to scan their answer sheets visually as they listened to the message.

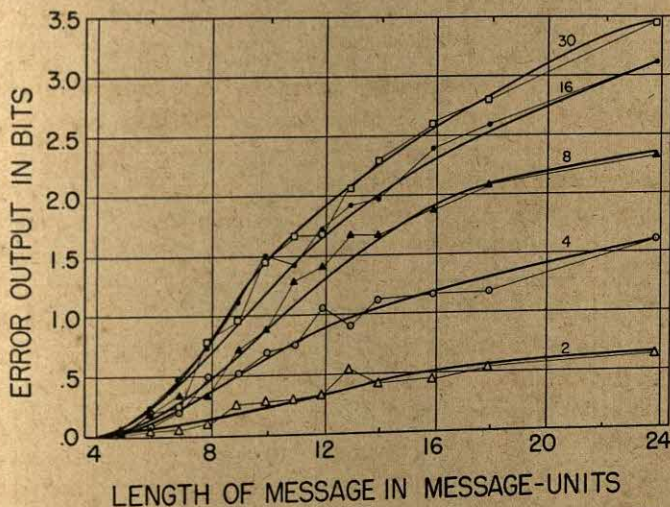


FIG. 4. AVERAGE INFORMATION LOST BY RECEIVER PER MESSAGE-UNIT. The number of possible alternatives per message-unit is the parameter. Each point on Figs. 4-11 is the average of four determinations with each of 25 Ss.

The messages were recorded on high-fidelity magnetic tape and were played back to groups of 1-10 Ss. Serving with each S was an assistant who corrected S's response. Each S served no more than one hour each day.

*Subjects.* The Ss were 25 undergraduate students of the University of Maryland. They were reimbursed for their services.

*Statistical knowledge of message-source.* Before each message was presented, the length of the message and the possible alternatives per message-unit were announced, i.e. the Ss were given full knowledge of the statistical characteristics of the message-source from which the message to be presented was chosen. In preliminary tests, the Ss were indoctrinated with the use of tables of random numbers and the selection of several messages was illustrated.

*Results.* The basic results of the experiment are presented in Fig. 4. They shall be considered in detail, since all other presentations are simply transformations based upon them.

Presented in Fig. 4 is the average information per unit length of the



message lost by the receiver. In general, *the information lost per message-unit (i.e. the error output) increases as the length of the message increases and as the number of possible alternatives per message-unit increases.* This is the first major conclusion.

We next inquire more closely into the rôle played by the number of possible alternatives per message-unit. If we replot each point of Fig. 4 relative to the informational input, we ask the following question: "For messages of a given length, what percentage of the information presented (the informational input) is lost?" The answer is given in Fig. 5. While there are slight systematic deviations among the curves as a function of the para-

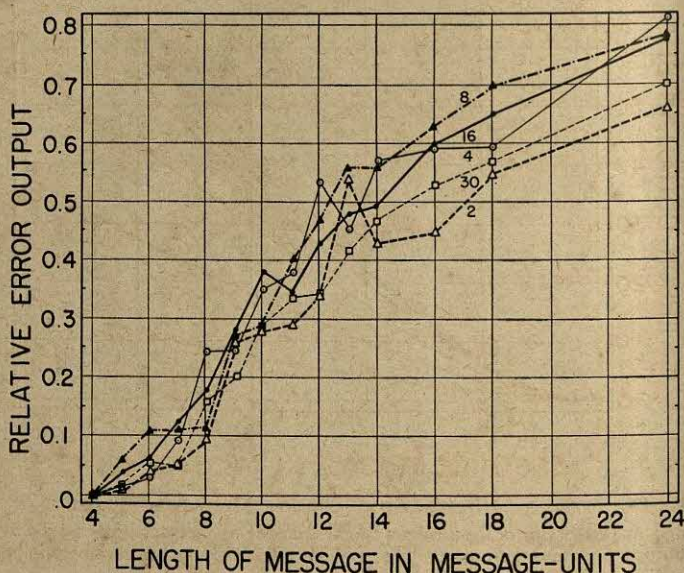


FIG. 5. AVERAGE INFORMATION LOST BY RECEIVER RELATIVE TO THE INFORMATIONAL INPUT

Data of Fig. 4; every point of Fig. 4 plotted as a ratio relative to the corresponding informational input. The parameter is the number of possible alternatives per message-unit.

metric value (the number of possible alternatives per message-unit), one function describes all the data reasonably well. That is, the information lost, relative to the information presented, is independent of the number of possible alternatives. Stated otherwise: *for messages of a given length, the percentage of the information presented that is lost is approximately independent of the number of alternatives per message-unit and is simply a*



function of the length of the message. This is the second major conclusion.

Thus far, we have simply considered errors per message-unit. Next we shall consider performance over the entire message. If we multiply the number of message-units per message by the average information lost per message-unit (Fig. 4), we shall obtain the information lost per message (Fig. 6). The previous generalizations obtained with the error output per message-unit also hold for total error output per message; namely, the

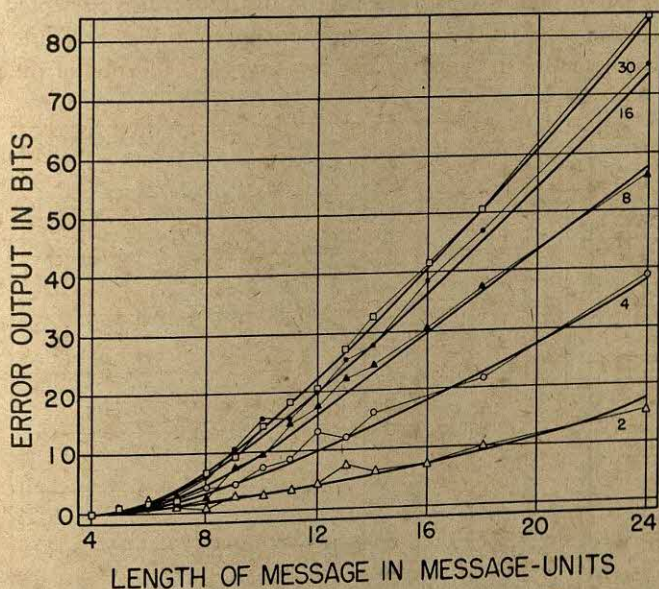


FIG. 6. TOTAL INFORMATION LOST PER MESSAGE BY RECEIVER

Presented in this graph is the product of the number of message-units by the average error output per message-unit of Fig. 4. The parameter is the number of possible alternatives per message-unit.

information lost per message increases as the length of the message increases and as the number of possible alternatives per message-unit increases.

We next consider measures of positive performance—the information gained by the receiver. Consider the schematic representation in Fig. 7 of relations between informational input, error output, and amount of information gained. For short messages, where the information lost by the receiver is zero, the amount of information gained is equal to the input and thus directly proportional to the length of the message. For somewhat longer messages, the error output remains small, and the amount of in-



formation assimilated therefore increases. For still longer messages, the difference between the informational input and the error output is approximately constant irrespective of the length of the message. When the messages become very long the information lost by the receiver increases relatively faster than the informational input and the amount of information gained therefore decreases.

The amount of information gained for the messages of the present study is presented in Fig. 8. The general relationships pointed out in the schematic are verified; namely, the amount of information gained increases

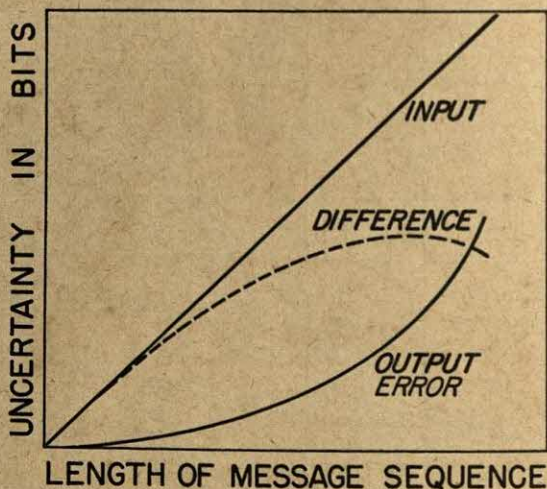


FIG. 7. SCHEMATIC REPRESENTATION OF THE INFORMATION GAINED

The information gained is the difference between the informational input and the information lost by the receiver—the error output.

as the length of the message is increased until a broad maximum is reached beyond which the information gained decreases. That is, *there is a region of message-lengths for maximal gain of information.* This is the third major conclusion. In addition, *the amount of information gained increases as the number of possible alternatives increases.* This is the fourth major conclusion. Nevertheless, the percentage of information presented that is assimilated is independent of the number of possible alternatives per message-unit and is simply a function of the length of the message.

Thus far, we have considered the informational analysis of immediate recall as a function of the length of the message and as a function of the number of possible alternatives per message-unit. Let us take stock of the generalizations. The main generalization is that one cannot obtain simultaneously both minimal information



loss and maximal information gain by simply varying either the length of a message or the number of possible alternatives per message-unit. If minimal error is required, one must usually be willing to pay with a lower assimilation of information. If maximal gain of information is required, one must usually be willing to pay with a greater loss of information. These statements appear paradoxical. Let us examine them more closely.

Consider the case in which the length of message is held constant and the number of possible alternatives per message-unit is varied. A large number of possible

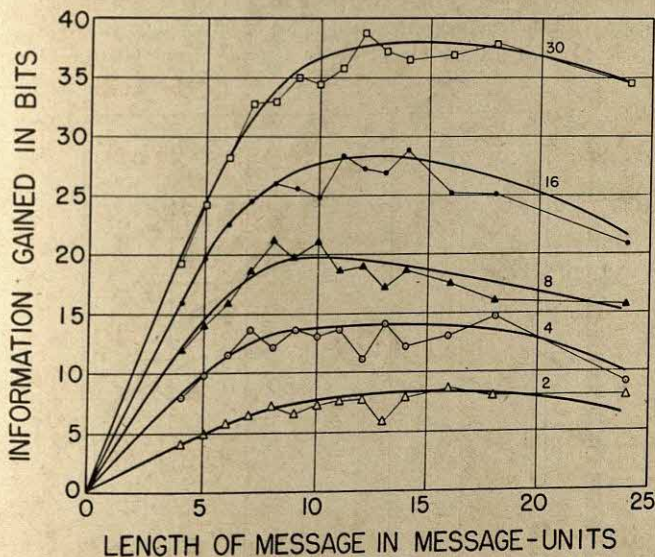


FIG. 8. INFORMATION GAINED PER MESSAGE

The parameter is the number of equally-likely possible alternatives per message-unit. Plotted is the difference between the informational input and the total information lost by the receiver.

alternatives per message-unit will yield a large assimilation of information (Fig. 8), but also a large output error (Fig. 6). A small number of alternatives, on the other hand, will yield a low informational loss (Fig. 6) but also a low information gain (Fig. 8). These relations stem from the fact that the percentage of information presented that is lost or gained is independent of the number of alternatives per unit and is simply a function of the length of the message (Fig. 5).

These relations may be illustrated by a short example. For a 12-unit message, the uncertainty per message-unit is approximately 0.4, 0.8, 1.2, 1.6, and 2.0 bits per message-unit for messages with 2, 4, 8, 16, and 30 possible alternatives per message-unit, respectively. Thus, relative to the informational input, the uncertainty is about 0.4 and is independent of the number of alternatives. On the other hand, the difference between the informational input and error output per message-unit, the amount of information gained, is approximately 0.6, 1.2, 1.8, 2.4, and 3.0 bits for messages with 2, 4, 8, 16, and 30 possible alternatives per message-unit, respectively. Con-



versely, relative to the informational input, the information lost is about 0.6 and is independent of the number of possible alternatives. Note that both the information lost and the information gained increase as the number of possible alternatives per message-unit increases. This apparently impossible relation is obtained, of course, because the baseline from which the information gain is calculated (the informational input) increases as the number of possible alternatives increases.

We stand a better chance of obtaining both minimal loss and maximal gain in information when the number of possible alternatives per unit length is held constant and the length of the message is varied. We can choose a message length at the beginning of the optimal region of assimilation of information (messages of 6, 7, or 8 units long) such that we almost obtain a maximal output-error (Fig. 6)—

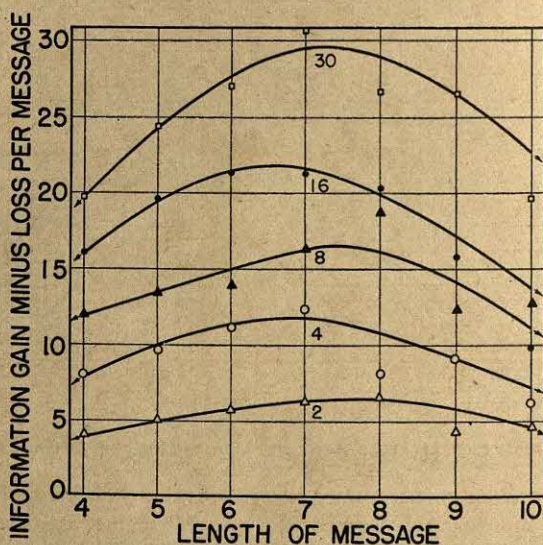


FIG. 9. MINIMAL-MAXIMAL SOLUTION FOR OPTIMAL MESSAGE LENGTH

On the ordinate is the difference, in bits, between the information gain and the information lost. The parameter is the number of alternatives per message-unit.

not quite maximal and minimal, but almost. More specifically, if we choose long messages (9 units in length and longer), we must accept high information loss along with high information gain. If we choose short messages (5 units in length and shorter), we must accept a low information gain along with low information loss. Messages in the intermediate region provide a compromise solution. Quantitatively, the compromise solution may be taken as the point of maximal difference between the information gain and the information loss (Fig. 9). On this basis, the maxima tend to fall in the region of 7 message-units.

Let us now consider the results from a somewhat different view. Instead of inquiring about immediate recall as a function of the length of the mes-



sage, let us examine this performance as a function of the informational input of the message (Figs. 10 and 11). To do this, we simply transform the abscissae of Figs. 6 and 8 on the basis of the number of alternatives per message-unit. This transformation changes the entire tenor of the conclusions. For a given informational input (rather than for a given message-length, as in Figs. 6 and 8, a smaller information loss (Fig. 10) and a larger information gain (Fig. 11) are obtained from short messages as-

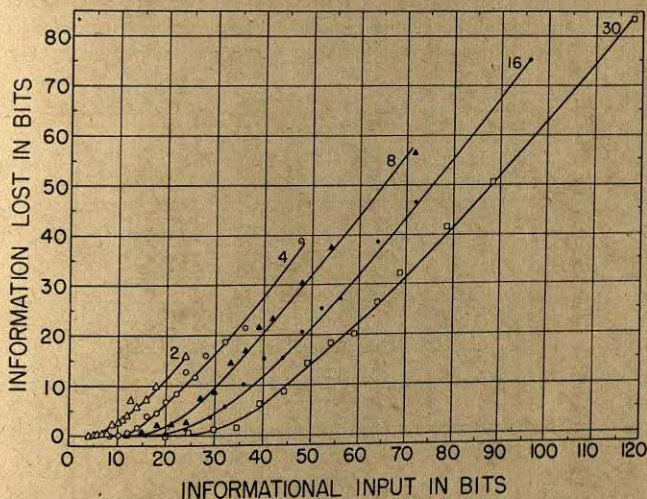


FIG. 10. TOTAL INFORMATION LOST AS A FUNCTION OF THE INFORMATION PRESENTED

The parameter is the number of possible alternatives per message-unit. Data of Fig. 6 with abscissa transformed.

sociated with a large number of alternatives per message than from longer messages associated with a smaller number of possible alternatives.

*Generality of informational measure.* It should be noted that the informational measure employed is not specific to the experimental manipulations of an investigation in verbal learning. As Miller and Frick have pointed out, the same measure can be used in a wide variety of psychological investigations.<sup>13</sup> Typical applications already reported are: the information conveyed by assignment of absolute judgments;<sup>14</sup> the description of operant conditioning,<sup>15</sup> the description of behavioral

<sup>13</sup> Miller and Frick, Statistical behavioristics and sequences of responses, *Psychol. Rev.*, 56, 1949, 311-324.

<sup>14</sup> Garner and Hake, *op. cit.*, 446-459.

<sup>15</sup> Frick and Miller, A statistical description of operant conditioning, this JOURNAL, 64, 1951, 20-36.



stereotypy;<sup>16</sup> the perception of visual and of auditory displays;<sup>17</sup> the description of linguistic structure;<sup>18</sup> the description of contextual restraints of language;<sup>19</sup> and its possibility of use in the recognition of words in noise.<sup>20</sup> The reason for the general applicability of the measure is that it is a function only of the probability distribu-

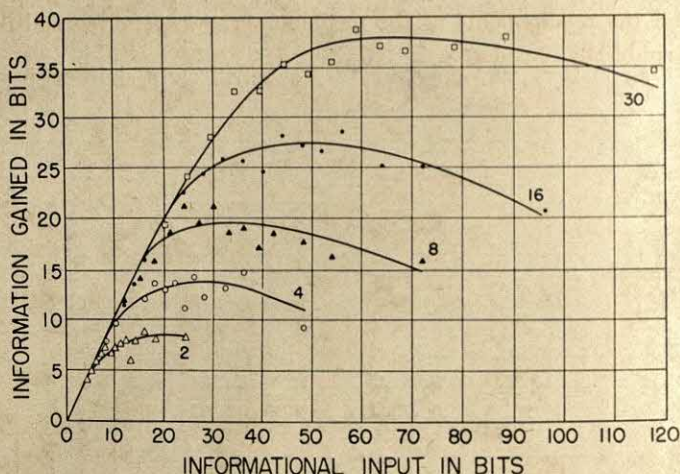


FIG. 11. TOTAL INFORMATION GAINED AS A FUNCTION OF THE INFORMATION PRESENTED

The parameter is the number of possible alternatives per message-unit. Data of Fig. 8 with abscissa transformed.

tions of the members which, collectively, constitute the source and the destination. The actual identity of the members of the source is not crucial for the quantification.

### SUMMARY

This paper presents a methodology for verbal learning based on the theory of information. The approach allows an objective quantification, in units not specific to the particular experimental operations considered, of (1) the learning materials employed (the informational input), (2) the

<sup>16</sup> *Ibid.*, 36; Miller and Frick, *op. cit.*, 311-324.

<sup>17</sup> Hake and Garner, The effect of presenting various numbers of discrete steps on scale reading accuracy, *J. Exper. Psychol.*, 42, 1951, 358-366; Garner, An equal discriminability scale for loudness judgments, *J. Exper. Psychol.*, 43, 1952, 232-238.

<sup>18</sup> Newman, The patterns of vowels and consonants in various languages, this JOURNAL, 64, 1951, 369-379.

<sup>19</sup> Miller and J. A. Selfridge, Verbal context and the recall of meaningful material, this JOURNAL, 63, 1950, 176-185; Shannon, Prediction and entropy of printed English, *loc. cit.*, 50-64.

<sup>20</sup> Miller, G. A. Heise, and W. Lichten, The intelligibility of speech as a function of the context of the test materials, *J. Exper. Psychol.*, 41, 1951, 329-335.



information lost (the error output), and (3) the information gained (the difference between the informational input and information lost). The units are sufficiently general to allow for comparison of the results of a diversity of experiments. An illustrative experiment in learning designed to fulfill the requirements of the methodology, was reported and the results were briefly considered.



## THE SPACE BETWEEN DISTINCT CONTOURS

By S. J. GERATHEWOHL and PAUL A. CIBIS, Randolph Field, Texas

In a paper on anisopia and the perception of space, Cibis and Haber demonstrated that position and size of surfaces in the visual space are determined by the apparent position and size of their contours.<sup>1</sup> It was found that position and size of contours vary with the brightness contrast between the target and its background and with differences in the retinal illumination.

This report deals with the space between contours. In concurrence with Gibson, the hypothesis was accepted that visual space perception is reducible to the perception of visual surfaces; and that distance, depth, and orientation in space may be derived from the properties of an array of surfaces depending upon the appearance of contours against their background.<sup>2</sup> To test this hypothesis, a series of experiments was conducted in which the apparent shape, slant, and curvature of homogeneously and heterogeneously illuminated surfaces was investigated in a purely descriptive and qualitative way.

### METHOD

*Subjects.* Four Ss (a, b, c, and d) were used in this study. They were professional draftsmen from the Department of Instructional Aids, USAF School of Aviation Medicine, Randolph Field, Texas. A careful selection of the draftsmen volunteering to serve was made with regard to accuracy and professional skill to secure precise drawings of the observed phenomena and especially of the apparent changes in the shape and position of the target under the various test-conditions.

*Procedure.* S was seated in front of the target and was told that this was an experiment on depth perception. It was explained that the target would be exposed under various eye illuminations which might cause it to appear differently. S's task, then, was to reproduce the appearance of the size and position of the target. It was also pointed out by E that the sketches should be drawn to scale; but due allowance was also given for slight exaggerations in the elaboration of finer details.

Two drawings were requested in each of the experiments: (1) front view of the target, whereby slant and curvature, if any, were to be reproduced by applying linear perspective and shading; and (2) projection of the target as seen from above.

\* Accepted for publication October 16, 1952. From the USAF School of Aviation Medicine.

<sup>1</sup> P. A. Cibis, and H. Haber Anisopia and perception of space, *J. Opt. Soc. Amer.*, 41, 1951, 676-683.

<sup>2</sup> J. J. Gibson, The perception of visual surfaces, this JOURNAL, 63, 1950, 367-384.



To facilitate the identification of the contours, capital letters and figures were used as shown in Fig. 2. The contours were drawn as solid lines; and the apparent deviations from the frontoparallel plane were shown in the bird's-eye view by broken lines. In addition, a brief description of deflections was requested if they occurred. The first sketches were later converted into illustrations displayed in this paper. All sketches were made while the target was exposed; and *S* was allowed to compare and modify his drawings until he was satisfied with the result.

#### EXPERIMENT 1

##### APPARENT SHAPE OF HOMOGENEOUSLY ILLUMINATED SURFACES SEEN AT DIFFERENT RETINAL ILLUMINATIONS OF THE TWO EYES

*Experimental setup.* The test-object used in all experiments consisted of an H-shaped target as shown in Fig. 1. The dimensions of the target were as follows. Length of the vertical bars, 6 in.; width of the vertical bars, 3 in.; interspace be-



FIG. 1. PHOTOGRAPH OF THE H-SHAPED TEST-FIGURE USED IN EXPERIMENT 1

tween these bars, 1 in.; and vertical diameter of the bridge between the vertical bars, 2 in. The surface was painted a flat white color and was seen in symmetrical convergence against a black background at a contrast of 400.<sup>3</sup> The target was posted upright in a frontoparallel plane with the center of the bridge at *S*'s eye-level. It was observed at a distance of 2 m.

*Test 1.* The target was seen with the naked eye as a homogeneously illuminated surface (Figs. 2 a-d). Fig. 2a gives the true or 'physical' image of the target. In Fig. 2b the inner contours of the vertical bars appear tilted. The same observation was made in Figs. 2c and 2d. In addition, one *S* saw the upper edges (A, C) bent backward (Fig. 2c).

*Test 2.* The evenly illuminated target was viewed with a Wratten neutral density filter of 2 log units in front of one eye. In Figs. 3a and 3b the drawings refer to the filter before the right eye. Corresponding effects were obtained when the filter was placed in front of the left eye, (Figs. 3c and 3d). The drawings indicate that the spatial distortion of the target is generally produced by a contour shift through the interposed filters. As stated elsewhere, anisopic stereo-effect is the effect on space perception caused by the difference in position and size of contours associated with differences in eye illumination.<sup>4</sup>

<sup>3</sup> Contrast =  $\Delta L/L$ , where  $\Delta L$  is specified as the difference between the luminance of the signal and the luminance of its background.

<sup>4</sup> Cibis and Haber, *op. cit.*, 677.



*Test 3.* The target was seen with the naked left eye and with the right eye looking through a rotating sector which cut the total light energy down to 10%. The spatial distortion resulting from this arrangement is strikingly similar to the one caused by neutral density filters, (see Figs. 3a-d).

Summing up our findings it can be stated that in dealing with the per-

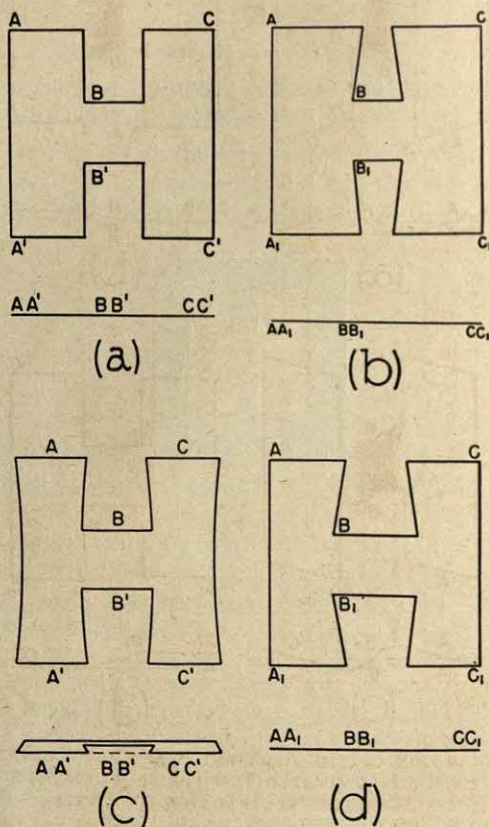


FIG. 2. REPRODUCTION OF THE HOMOGENEOUSLY ILLUMINATED TEST-FIGURE WHEN SEEN WITH THE NAKED EYES

The *Ss* are designated by the letters (a), (b), (c), and (d) placed below the drawings.

ception of a plain and homogeneously illuminated H-shaped surface, the phenomena of apparent slant and curvature were obtained merely by introducing binocular disparities of the position and size of contours. The transition from two-dimensional into three-dimensional state, and the



appearance of slant and curvature can be greatly enhanced when a more complex situation is applied.

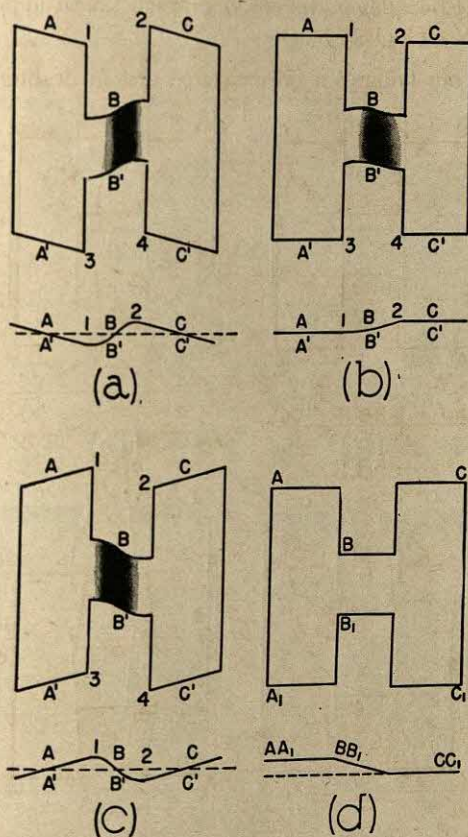


FIG. 3. REPRODUCTION OF THE APPARENT SLANT AND CURVATURE OF THE HOMOGENEOUSLY ILLUMINATED TEST-FIGURE WHEN SEEN THROUGH A WRATTEN NEUTRAL-DENSITY FILTER

For (a) and (b) the filter is in front of right eye; for (c) and (d), the left eye.

#### EXPERIMENT 2

#### INVESTIGATION OF THE APPARENT SHAPE, SLANT, AND CURVATURE OF HETEROGENEOUSLY ILLUMINATED SURFACES

*Experimental setup and procedure.* The experimental setup and procedure were the same as described above with the difference that the target surface was not homogeneously illuminated, and that the target was always observed with the naked eye. Dark and black shadows were cast upon selected parts of the target by introducing solid and transparent objects in the path of light.



*Test 1.* Spatial deviations of the contours and corners concerned became apparent when the left lower corner of the left vertical bar was darkened, (Figs. 4a and 4b). Again, differences in the interpretation of the spatial distortion were found among the four Ss. In all cases, however, the deflection of the corner was seen backward; in two cases, smoothly curved; and in the other cases, sharply bent. As in the first experi-

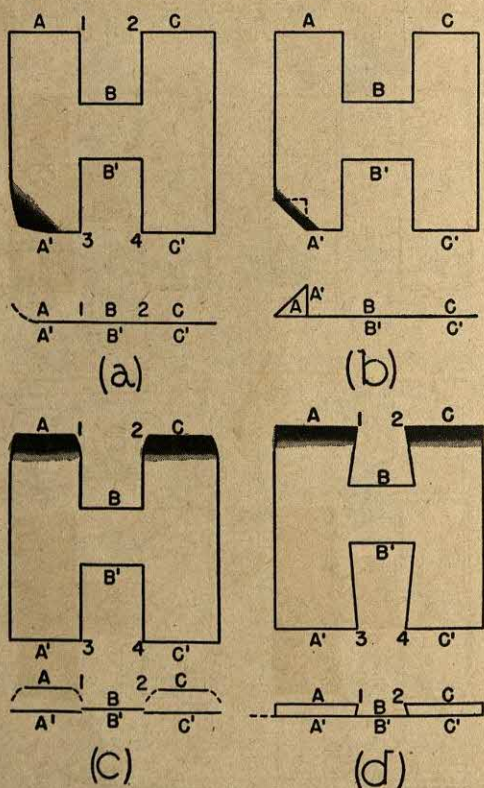


FIG. 4. APPARENT DISTORTION OF THE TEST-FIGURE

For (a) and (b) when the left lower corner of the left bar is overshadowed; for (c) and (d) when the upper ends of the vertical bars are overshadowed.

ment, all drawings were made individually and no time limit was set for either observation or reproduction.

*Test 2.* A dark shadow was cast upon the upper ends of the vertical bars. The spatial effect was uniform in that these ends appeared bent backward either with or without rounded corner as shown in Figs. 4c and 4d.

*Test 3.* The right half of the target was shaded. The spatial distortion seen under this condition is shown in Figs. 5a and 5b.

*Test 4.* The bridge and the right vertical bar were shaded differently by using



overlapping Wratten neutral density filters. The spatial bar appeared darker than the bridge. The spatial effects are depicted in Figs. 5c and 5d.

*Test 5.* A shadow was cast on the bridge. In all cases the bridge appeared curved backward; in one case the whole right bar was apparently displayed backward, although the illumination of this bar was the same as that of the left one.

*Test 6.* A horizontal shadow was cast on the upper half of the target. In this test four different interpretations were obtained.  $S_1$  noted: "Target appeared broken in

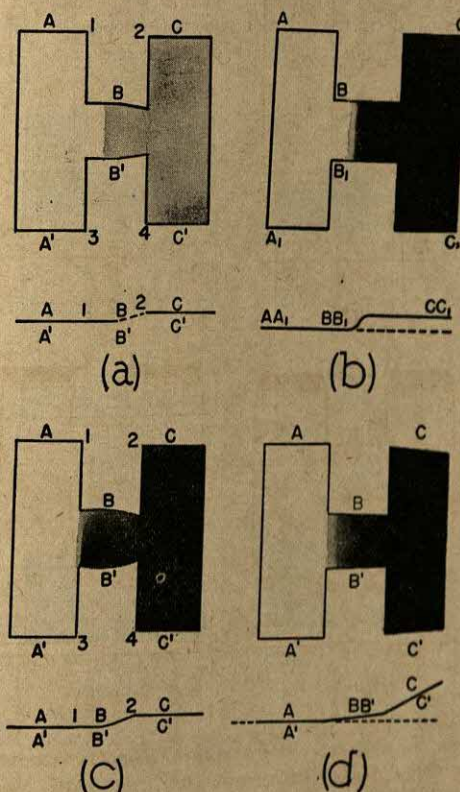


FIG. 5. REPRODUCTION OF THE SLANT AND CURVATURE OF THE TEST-FIGURE For (a) and (b) when a shadow is cast upon the right half; for (c) and (d) when the right bar and bridge are shaded differently.

the middle with extremities forward and center farther away" (Fig. 6a).  $S_2$  reported: "The bottom of unshaded area appeared to be larger than the shaded area. The top appeared to be leaning backward slightly" (Fig. 6b).  $S_3$  stated, "The horizontal center line seemed to be the closest point to the eye with the four outer corners falling away like a roof top" (Fig. 6d),  $S_4$  saw the upper and lower part of the target completely "broken and located in planes of different distance" (Fig. 6d).



Because of these latter results, suspicion arose that the sharpness of the boundary between the shaded and the unshaded areas exerts a decisive influence on the interpretation of the spatial distortion obtained thus far. Tentatively, it was assumed that a sharp border line causes a breaking of the figure and favors the apparent dislocation of the darker part farther away. For this reason an additional experiment was made in which only sharp borders between the shaded and the unshaded areas were applied. The results obtained with two Ss are shown in Fig. 7. Two types of distortion were observed. One was a full confirmation of our assumption (Fig. 7b); the other one deviates in that the target remains in its frontoparallel plane, although the shaded part appears to be smaller than the unshaded one (Fig. 7a). By and large,

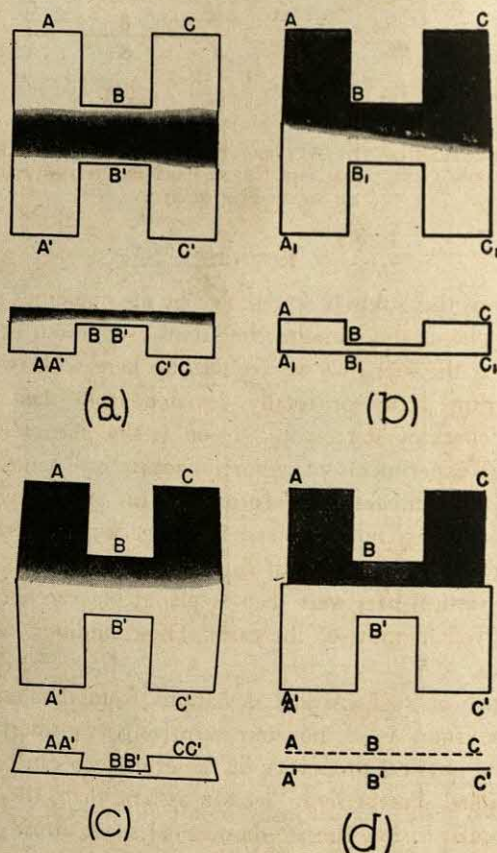


FIG. 6. REPRODUCTION OF THE APPARENT DISTORTION OF THE TEST-FIGURE WHEN THE UPPER HALF IS OVERSHADED

Figs. 6 and 7 justify the conclusion that the sharp boundaries favor an apparent breaking and dislocalization of the differently illuminated parts of the target, while blurred or faint boundaries elicit an apparent curvature of the surface.



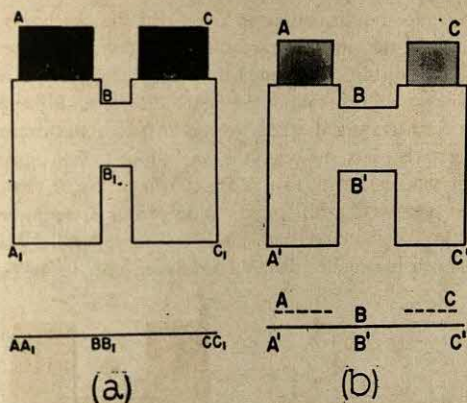


FIG. 7. REPRODUCTION OF THE ALTERATION OF THE TEST-FIGURE WHEN THE SHADOW CAST UPON THE UPPER PART IS DISTINGUISHED BY SHARP BOUNDARIES

### RESULTS

The outcome of this study is represented by the numerous pictures covering the various phases and stimulus conditions used in our experiments. Of special interest is the fact that we are dealing here with two-dimensional surfaces appearing partly or totally deviated from their actual plane. Similar to the effect of stereoscopic vision is the phenomenon of depth produced in our experiments of a purely 'subjective' nature; *i.e.* without any objective (three-dimensional) correlate. This effect may be explained by the individual's "psychological system of space interpretation,"<sup>5</sup> which functions differently for the vertical and the horizontal bars of our target. In all tests the vertical bars were seen as plane; but the horizontal bridge was seen as curved in most of the cases. These findings demand a more detailed analysis.

The occurrence of such marked deviations from the 'normal' or true image with the four Ss is not too surprising, considering the slight anomalies of the physical properties of the eye which can be found in almost any individual. Furthermore, we can assume that "the space-creating powers of binocular vision operate on original space already at hand, and transform it. These phenomena reveal the existence of an internal dynamics of visual space."<sup>6</sup> In the first place, visual perception may be altered by

<sup>5</sup> W. Ehrenstein, *Probleme der ganzheitlichen Wahrnehmungs-Lehre*, 1947.

<sup>6</sup> David Katz, *Gestaltpsychology*, 1950.



aniseikonia, cyclotorsion, and by all types of anisopia.<sup>7</sup> Alterations of space will also occur under the effect of the internal forces and dynamic processes. In our tests the Ss represent a selected group since they were professionally trained and skilled in the reproduction of visual impressions, but even within this group there was great variation in the perception of apparent slant and curvature of the target.

As previously described by Cibis and Haber and demonstrated in the schematic illustration given in Fig. 8, two plane surfaces set in the fronto-

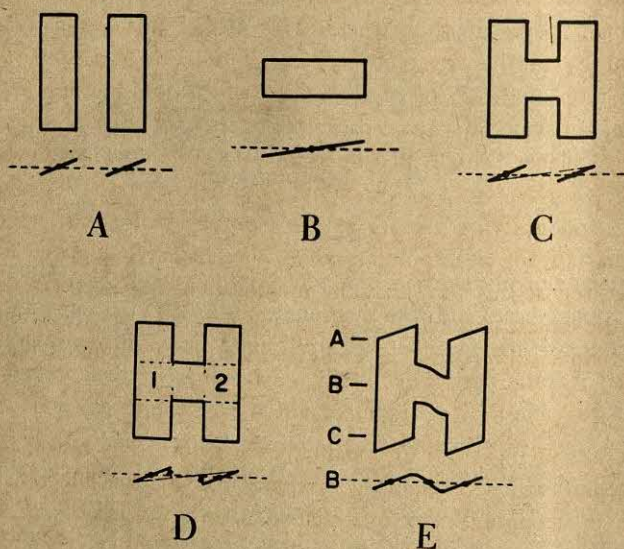


FIG. 8. THE APPEARANCE OF THE PARTS (A) AND (B) OF THE TEST-FIGURE (C) AND (D) AND THEIR ACTUAL APPEARANCE (E) WHEN SEEN THROUGH A FILTER IN FRONT OF THE LEFT EYE

parallel plane are likely to appear tilted about separate pivot points when seen with different eye illuminations (Fig. 8A). According to its retinal picture, a single plane should appear rotated about a pivot point as shown in Fig. 8B. Since our target represents a combination of a and b, the rotation of the vertical and horizontal parts of the target in a geometrical construction (deducted from a and b) would be as demonstrated in Fig. 8C. In this way there seem to be three individual pivot points as vertical components of the target. In the geometrical construction of image of the

<sup>7</sup> W. B. Lancaster, *Arch. Ophthalm.*, 20, 1938, 907.



test-figures, the bridging part should be in a position somewhat apart from the vertical bars. The rectangles 1 and 2 in Fig. 8D, however, pertain to both the vertical and the horizontal part of the target, and are perceptually integrated in the stronger 'fields' produced by the two vertical bars. This results in an apparent counter-rotation of the horizontal parts of the target representing the bridge as shown in Fig. 8E. The apparent clockwise rotation of the image toward the dominating and individually tilted vertical components can only be explained by the 'pregnance' of the perceptually stronger features of the target, and demonstrates that the apparent position of the bridge is determined by the apparent position of the main parts of the target.<sup>8</sup> This spatial adaptation (*räumliche Angleichung*) of the apparent position of the bridge also results in a transformation of the shape of the bridge which does not appear straight or broken, but smoothly curved backward or forward at the junction of the three target planes. This is of importance since the apparent deviation of the bridge's surface from its geometric form demonstrates the role of the rectifying and unifying factors in the perception of space, which phenomena has already been pointed out by Gestalt Psychologists.<sup>9</sup> It should be emphasized at this time that the modification of perception described above is the typical one, but that we cannot expect identical pictures in all cases. Individual differences can easily be identified throughout Fig. 3.

### DISCUSSION

The results of this study are qualitative in nature. The reason for using this procedure will be explained elsewhere; namely, the difficulty one experiences in obtaining exact quantitative data by means of rod or stick measuring devices.<sup>10</sup> Nevertheless, the use of draftsmen as Ss and the reproduction of the observed phenomena in pictures may be questioned.

Recently Gibson,<sup>11</sup> as well as Schildt and Petterson,<sup>12</sup> conducted experiments on space perception using methods of reproduction. The Ss were required either to simulate the visually perceived slant of an apparently tilted surface by a motor response (Gibson), or to reproduce visually perceived complex scenes with material provided for that purpose (Schildt

<sup>8</sup> Katz, *op. cit.*

<sup>9</sup> Max Wertheimer, *Untersuchungen zur Lehre von der Gestalt, Psychol. Forsch.*, 4, 1923, 301-350; Wolfgang Köhler, *Gestalt Psychology*, 1929.

<sup>10</sup> P. A. Cibus and S. J. Gerathewohl, Apparent shift of contours as a function of retinal illumination (in preparation).

<sup>11</sup> Gibson, *op. cit.*, 367.

<sup>12</sup> Cited by Katz, *op. cit.*



and Petterson). In both cases valuable results on the interpretation of spatial relations were obtained.

The analysis of the effects of retinal illumination on the interpretation of space as applied here is not without a quantitative basis, since data were presented earlier which proved the definite variation of the apparent localization and size of contours with varying illumination.<sup>13</sup> From the results previously obtained it was established that the image of a white target decreases in size when the illumination on the retina is decreased. On the other hand, the image of a black target increases in size under the same conditions. Consequently the spatial effect which occurs when parts of the target are shaded off is a result of the contour shift and of the decrement in brightness contrast of the areas concerned. Furthermore, it can be assumed that shade as a cue in depth perception is to some extent based on the common experience of shaded areas appearing to be farther away than the brighter or unshaded ones; this experience is also associated with the apparent contour shift caused by the attenuation of the retinal illumination.<sup>14</sup>

It is not our intention to deny shade as an individual cue in depth perception. This has by no means been proved by our experiments. There is still another point which should be emphasized; namely, the importance of linear perspective or 'size-at-a-given-distance' in depth perception. Decrease in image brightness produces not only a shift of contour, but also a change in size of contour. This results in an alteration of the monocular cue of 'size-at-a-given-distance' in addition to the alteration of stereopsis associated with the contour shift in the horizontal direction.<sup>15</sup> The decrease in size of vertical contours or of the distance between horizontal contours (*i.e.* by shading the bridge) necessarily results in a change of the size-distance ratio of the related reference points significant in monocular depth perception. This fact may be of great significance in the apparent receding of the shaded parts. Regardless of these factors, there are still others which demand more discussion.

There can be no doubt that our Ss were exposed to a perceptual conflict resulting from the apparent distortions of the surface due to the mechanisms determining the properties of the physical image on the one hand,

<sup>13</sup> Cibis, Gerathewohl, and D. Rubinstein, Problems of depth perception in monocular and binocular vision, *USAF SAM Randolph Field, Texas. Special Report*, January, 1953.

<sup>14</sup> Cibis, and Gerathewohl, *op.cit.*

<sup>15</sup> Cibis and Gerathewohl, *op. cit.*



and from the experience of constancy of the figure in space on the other hand. It is also the psychological concept of 'pregnancy,' which seems to be of great importance for the localization of the various parts of the target and their incorporation into a unity. If such simple forms as squares or circles are observed, the contours are the most essential features which determine slant and curvature of surface between them. In case of complex targets, the contour shift may be so complicated and contradictory to common experience that the unity of the visual object can be secured only by compromises in form of integrating the perceptual elements into a meaningful entity. Sander and Iinuma found that the fusion-threshold for stereoscopic vision also depends upon the complexity of the objects presented.<sup>16</sup> In our experiments the fusion of disparate parts takes place in accordance with the law of parsimony resulting in configurations which appear to be reasonable to the observer. For achieving this, the bridge must be rotated visually in just the opposite direction from the rotation brought about by the physical mediae. Only by this act of rectification is it possible to overcome the strength of binocular disparity.

#### CONCLUSIONS

In a series of experiments the appearance of homogeneous and heterogeneous surfaces was investigated as to their apparent slant and curvature. For this purpose a white H-shaped target was used as the test figure. The target was viewed with different retinal illumination in the two eyes and when parts of it were shaded. A variety of physical, physiological, and psychological factors are involved in the interpretation of depth. These factors are capable of producing unequal imagery or illusions of distortion of visual space.

For the sake of a systematic interpretation of our results a differentiation is made between the 'physical image' of the percept, by which is determined mainly the extension, location, timing, and illumination of the image of the object on the retina; and the 'psychological image,' the latter being chiefly determined by the 'individual characteristics' of the perceiver. There is strong evidence that shape, slant, and curvature of a surface not only depend upon binocular disparity, size of the elements, texture density, and linear perspective, but also upon differences in retinal il-

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<sup>16</sup> Friedrich Sander and Ryuon Iinuma, Beiträge zur Psychologie des stereoskopischen Sehens: Die Grenzen der binkoularen Verschmelzung in ihrer Abhängigkeit von der Gestalthöhe der Doppelbilder, *Arch. f. d. ges. Psychol.*, 65, 1928, 191-206.



lumination which produce alterations of the visual space. The results likewise reveal the existence of an internal dynamics acting during the perception of depth.

If an alteration of any of the factors occurs which determine the physical and the psychological image of the percept, the image itself will be affected. Specifically, there is scarcely a case in which any change of these variables in one of the two images fails to reflect an alteration in size or shape of the image. If such an alteration of the physical properties exists unilaterally—either inherent as an anomaly or artificially introduced by lenses or filters—it will bring forth a characteristic distortion of visual space which may in certain cases cause alterations of shape, distortion of space and decomposition of figure.

Normally the physical and the psychological factors in space perception operate in the same direction and, thus, reinforce one another. In this case the effect of depth is enhanced and the perception of space is facilitated. If, on the other hand, the physical imagery and the psychological factors oppose one another, a perceptual conflict arises, which must be solved reasonably through an acceptable compromise. In the latter case the solution is made in favor of the psychological factors in accordance with Gestalt principles.

The results of our investigation suggest that the perception of visual space depends (among other things) upon (1) the location, size and shape of surface contours or their alteration through differences between the retinal illumination of the two eyes (Gibson's hypothesis was confirmed by this finding) and (2) the total or partial brightness (luminance) of the object—as demonstrated in our second series of experiments—determines apparent shape, slant and curvature of a surface. This means that under certain conditions size, shape, and distance are interrelated variables in depth perception and must be so treated. It has been demonstrated, furthermore, that the perception of slant and curvature—and thus the perception of the visual space between contours—correlates with the retinal stimulation and the geometric-optical characteristics of the image on the one hand, but also depends to a high degree upon the psychological characteristics of the observer. It can be concluded from our findings that space perception is not, moreover, a static projection of the environmental phenomena in the mind of the observer; but that the spatial differences and the occurrence of shape, slant, and curvature are subject to a dynamic interpretation of the environment.



## APPARENT SIZE OF AFTER-IMAGES UNDER CONDITIONS OF REDUCTION

By WARD EDWARDS, The Johns Hopkins University

The physical size of any object is the size which it has when measured with a tape measure or some other similar measuring device. The only judgment which must be made by an *O* to measure physical size is one of coincidence of lines (e.g. coincidence of the edges of an after-image with lines of a tape measure, if the physical size of after-images is being measured). The apparent size of any perceived object is the physical size of a comparison object which an *O* asserts to be equal in size to the object being measured. The judgment which must be made by an *O* to measure apparent size is one of equality of sizes of two objects (e.g. equality of size of an after-image and a comparison object of similar shape, if the apparent size of after-images is being measured).

In a previous note Boring and I made the following statements about these two kinds of sizes.<sup>1</sup>

(a) *Size constancy*. The apparent size of an object is proportional to its physical size and is independent of the distance at which it is seen, provided the physical size of the object does not change with distance.

(b) *Euclid's ocular geometry*. The physical size of an after-image is proportional to the distance of the surface on which it is projected, provided the size of the retinal image remains constant.

(c) *Emmert's law of apparent sizes*. The apparent size of an after-image is proportional to the distance of the surface on which it is projected, provided the size of the retinal image remains constant.<sup>2</sup>

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<sup>1</sup> There has been a series of notes and discussions of this subject. They are: E. G. Boring, Size constancy and Emmert's law, this JOURNAL, 53, 1940, 293-295; F. A. Young, The projection of after-images and Emmert's law, *J. Gen. Psychol.*, 39, 1948, 161-166; Boring's interpretation of Emmert's law, this JOURNAL, 63, 1950, 277-280; Ward Edwards, Emmert's law and Euclid's optics, *ibid.*, 607-612; Young, What is Concerning Emmert's law, *ibid.*, 64, 1951, 124-128; Edwards and Boring, What is Emmert's law? *ibid.*, 416-422; Young, Studies of the projected after-image: I. Methodology and the influence of varying stimulation times, *J. Gen. Psychol.*, 46, 1952, 73-86. See also E. Emmert, Grössenverhältnisse der Nachbilder, *Klin. Monatsbl. Augenheilk.*, 19, 1881, 443-450.

<sup>2</sup> Edwards and Boring, *op. cit.*, 417.



We pointed out that these three propositions are so related that if any two of them are true, then the third is necessarily also true. Proposition (a), about size constancy, is approximately true under ordinary conditions of observation. It is not true under conditions of reduction, in which the clues to depth vision are absent.<sup>3</sup> Proposition (b) is a deduction from the geometry of projection of after-images, and should be true under all conditions. Proposition (c), Emmert's law about the apparent size of after-images, should therefore be true under ordinary conditions of observation and should be false under reduction-conditions.

Casual observation of the behavior of after-images shows that proposition (c) is at least approximately true under ordinary conditions. When we project an after-image on a far surface it looks large; on a near surface it looks smaller. No one, however, has studied what happens to after-images under reduction-conditions. In my original note on this subject, I predicted the result of such an experiment: "(Under reduction-conditions) the apparent size of the after-image should remain constant at every distance of the projection-surface. Complete reduction would thus result in the complete failure of Emmert's law."<sup>4</sup> The purpose of this experiment was to test this prediction.

#### METHOD AND PROCEDURE

*Apparatus.* An existing apparatus was modified to meet the demands of this experiment. The modified apparatus had the following parts:

(1) *Left-hand field.* This field could be illuminated at two levels of brightness, very high and low. The high brightness level had a luminance of 803 foot-lamberts. The color temperature of the light source was approximately 2450°K. The low brightness level had a luminance of 0.092 foot-lamberts, and in this case the color temperature of the light source was approximately 1750°K. The distance of the field from *O* could be varied by *E*. A fixation-point (a square of black tape) was attached to the field.

(2) *Comparison square.* This square, located to the right of the left-hand field, was a dimly luminous area, 2-in. sq., made by covering a small light box with an appropriate mask. Its luminance was 0.100 foot-lamberts, and its color temperature was approximately 1850°K. *O* could vary the distance of the light-box from him by turning a wheel. A screen prevented light reflected by the left-hand field from illuminating the outside of the light box.

(3) *Reduction screen.* This screen was located 26.5 in. from *O* and had a rectangular hole, 7 in. wide and 6½ in. high, cut in it. Through the hole *O* could see the left-hand field, the comparison square, and a vertical partition down the center of the apparatus which separated these two areas from each other.

<sup>3</sup> William Lichten and Susan Lurie, A new technique for the study of perceived size, this JOURNAL, 63, 1950, 280-282.

<sup>4</sup> Edwards, *op. cit.*, 612.



(4) *Mask.* This mask could be inserted into the hole in the reduction-screen, so as to block it. The mask had an opening, 1-in. sq., cut in it, with a fixation-point painted on transparent plastic in the middle. When the mask was in place, *O* could see neither the comparison square nor the fixation-point attached to the left-hand field. All he could see was a square of the undifferentiated left-hand field which was exposed by the transparent plastic, with the fixation-point attached to the mask in the middle of it.

(5) *Reduction-tube.* The reduction-tube, fixed to the apparatus, blocked out view of all but the area occupied by the parts of the apparatus discussed above. Attached to the end into which *O* looked was an artificial pupil 2 mm. in diameter. Observation was monocular.

*Procedure.* Ten naïve *Os* were used; each was instructed that this was to be an experiment on the size of after-images. After instruction, each *O* was given a few practice trials. In the experiment itself, each *O* made 30 judgments. Each judgment involved three successive uses of the apparatus. The first was the after-image producing stage. The mask was in place, and the lights on the left-hand field were bright. The left-hand field was as close behind the mask as it could be moved (42.25 in. from *O*). *O*, looking through the reduction-tube, saw a luminous white square with a dot in the middle. He was instructed to fixate the dot. This stage continued for 45 sec.

In the second stage of the apparatus, the after-image developing stage, all lights were off, and *O* was in complete darkness for about 30 sec. This stage occurred immediately after stage one—the after-image producing stage.

In the third stage, the after-image measuring stage, the mask was removed, and *O* saw the left-hand field dimly luminous and the right-hand comparison object also dimly luminous. Both looked orange. *O* was instructed to project his after-image (which usually also looked orange, but of a somewhat different hue and saturation) onto the left-hand field by fixating the fixation-mark attached to it. This fixation-mark was large enough to be seen in spite of the dim illumination and the obscuring effect of the after-image covering it, but was substantially smaller than the after-image itself. The fixation-mark was located above the level of the comparison-square, hence *O* would be discouraged from simply trying so to set the comparison-square that its horizontal edges would look like continuations of the horizontal edges of the after-image.

*O* was also instructed to turn the wheel which moved the comparison-square until the comparison-square appeared to be the same size as the after-image. In spite of the fact that actually it was the distance rather than the size of the comparison-object which *O* was changing, no *O* objected to these instructions. The reduction-conditions were so effective that no *O* had any difficulty judging that in some positions the comparison-object it appeared larger than the after-image while in other positions it appeared smaller.

*O* was allowed to continue to adjust the position of the comparison-object until he was satisfied with his judgment. This usually took between 30-60 sec. After a rest of about a minute, *O* was then instructed to fixate the bright square to develop the next after-image. *O* alternated eyes during the course of the experiment; the first after-image was always developed in the right eye.

There were five independent variables in this experiment:

(1) *Distance from O of the surface (the left-hand field) onto which the after-image was projected.* Five distances were used: 42.25, 54.25, 66.25, 78.25, and 90.25 in.



(2) *Starting place of the comparison-object.* At the beginning of each after-image measuring period, the comparison-square was exposed to *O* either at its maximal (90.75 in.) or minimal (37 in.) distance.

(3) *The eye used for the judgment.* Each *O* started using his right eye, and alternated eyes on successive judgments throughout the experiment.

(4) *Observers.* Ten naïve male undergraduates were used.

(5) *Replications.* There were three identical blocks of ten trials each. Table I gives the arrangement of these variables for one replication.

The method of measurement of the size of the after-images used in this experiment was an unusual one, which depends for its validity on the ability of the *Os* to interpret a distance-change in a stimulus of constant size seen under reduction-conditions as a size-change. One obvious method of testing its validity is to see

TABLE I  
SUMMARY OF CONDITIONS WITHIN ONE REPLICATION

Judgment	Eye	Distance of projection-surface (in.)	Starting position comparison square	Judgment	Eye	Distance of projection-surface (in.)	Starting position comparison square
1	R	66.25	front	6	L	66.25	back
2	L	42.25	back	7	R	42.25	front
3	R	78.25	front	8	L	78.25	back
4	L	54.25	front	9	R	54.25	back
5	R	90.25	back	10	L	90.25	front

whether the *Os* can successfully judge the apparent size of real stimulus-objects with this apparatus. Squares of tin, painted orange, were attached to the projection-surface for use as stimuli in a separate validation experiment. Eight squares of different sizes were used, four at the 90.25-in. projection-distance and four at the 42.25-in. distance. Six new *Os* were chosen for this part of the study. Only the back starting position of the comparison-square was used. The *Os* made four judgments of apparent size of each square. Only the dim illumination of the projection-surface illuminated the squares. The data show clearly that the method of measurement used was adequate to measure apparent size. A plot of the visual angle of the stimulus-objects at either projection-distance against the visual angle of the comparison object at the distance where it was judged equal to each stimulus-object yields essentially a straight line with a slope of  $45^\circ$  passing through the origin—as it should if this method of measurement is to be considered satisfactory. The variability due to individual differences was small.

## RESULTS

Table II gives the results of the main analysis of variance, which excludes eye used for reasons given below. A significant contribution to the variance was made by the starting place of the comparison-object, and this variable interacted significantly with *Os*. Inspection of the original data shows that this interaction arises principally because two *Os* were very



much affected by the starting position, *i.e.* whether the comparison-object started from the front or the back.

The only other significant contribution to the variance comes from the *O*s. The contribution made by projection-distance, with which the experiment was primarily concerned, is insignificant and is, in fact, smaller than the triple interaction and the replication variance (which are the best estimates of unexplained experimental error). To check on the possibility that there might be a close relationship between projection-distance

TABLE II  
ANALYSIS OF RESULTS

Source	Sum of squares	df	Variance
Projection distance	99.27	4	24.82
Starting place of comparison object	987.65	1	987.65*
<i>O</i> s	5645.60	9	627.29*
Projection $\chi$ starting	87.38	4	21.85
Projection $\chi$ <i>O</i> s	814.73	36	22.63
Starting $\chi$ <i>O</i> s	1546.38	9	171.82†
Projection $\chi$ starting $\chi$ <i>O</i> s	948.31	36	26.34
Replications	5236.47	200	26.18
Total	15365.79	299	

\* Significant beyond 5% level using starting  $\chi$  *O*s interaction as the denominator of the *F*-ratio.

† Significant beyond 0.1% level using either the triple interaction or the replications variance as the denominator of the *F*-ratio.

and size of after-image which was obscured by the extra degrees of freedom assignable to deviations from a regression line relating these variables, the correlation coefficient between them was computed. It was  $-0.007$ .

The design of this experiment was confounded, since not all possible combinations of distances, starting positions, and eye appear. There was no reason to believe before the experiment that there would be any difference between the right and left eyes. Originally, in fact, it was intended that the right eye would be used throughout. The after-images were so persistent, however, that it was necessary to alternate eyes to allow time for each retina to rest between one after-image and the next. It would have been possible to allow for eye used in the design, but this would have required more trials, a reduction in the number of projection-distances used, or some kind of incomplete design. Since none of these seemed desirable, the confounded design was used.

Examination of the data does not show any difference between eyes. It is possible to test this statistically in at least an approximate way by pooling the data for the two greater projection-distances, calling them 'far,' and



those for the two smaller projection-distances, calling them 'near.' (For this purpose data obtained at the middle projection distance must be omitted.) When the data are tabulated in this way, confounding is no longer present, and it is possible to perform an analysis of variance to test the significance of variance attributable to eyes. Table III presents the results of this analysis.

It is clear that neither eye used nor any of its interactions with other variables even approaches significance. Otherwise, the results of all  $F$ -

TABLE III  
ANALYSIS OF RESULTS WITH ONLY TWO DISTANCE-CATEGORIES

Source	Sum of squares	df	Variance
Projection distance	14.01	1	14.01
Starting place of comparison object	814.90	1	814.90*
Os	4473.67	9	497.07*
Eye used	10.44	1	10.44
Projection $\chi$ starting	20.46	1	20.46
Projection $\chi$ Os	188.61	9	20.96
Projection $\chi$ eye used	50.29	1	50.29
Starting $\chi$ Os	1155.86	9	128.43†
Starting $\chi$ eye used	54.34	1	54.34
Os $\chi$ eye used	151.16	9	16.80
Pooled higher-order interactions	699.91	37	18.92‡
Replications	4052.67	160	25.33
Total	11686.32	239	

\* Significant beyond 5% level using starting  $\chi$  Os interaction as the denominator of the  $F$ -ratio.

† Significant beyond 0.1% level using either the replication variance or the pooled higher-order interaction variance as the denominator of the  $F$ -ratio.

‡ All the higher-order interactions were calculated. None of them even approached significance when tested against the replication variance, so they are reported here in pooled form.

tests are exactly the same as were found in the main analysis of variance presented in Table II. To the extent to which it is legitimate to lump together data gathered at slightly different projection-distances, then, this analysis justifies the conclusion that eye used made no difference to the results.

## DISCUSSION

This experiment was designed to reveal any effect of variations in projection-distance on the apparent size of after-images under reduction-conditions. If Emmert's law of the apparent size of after-images were applicable to such images viewed under reduction-conditions, the size of the after-images should have increased with increasing projection-distance.



The results clearly indicate that no such effect occurred. Of course, it is not possible to make a statement of the probability that the null hypothesis (*i.e.* the hypothesis that under reduction conditions the projection-distance makes no difference to the apparent size of after-images) is correct—no experiment can ever prove its null hypothesis. It is possible, however, to say that under excellent conditions for refuting the null hypothesis with which this experiment was concerned, no evidence against it could be found.

It is not surprising that Emmert's law fails under reduction-conditions. In fact, it would be most surprising if it did not. All this experiment does, then, is to confirm a simple deduction from well-established logical premises, and consequently to provide empirical support for a thesis which can be adequately defended even without it.



## SECONDARY REINFORCEMENT AND THE DISCRIMINATION HYPOTHESIS

By M. E. BITTERMAN, W. E. FEDDERSEN, and D. W. TYLER,  
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The experiment to be reported is concerned with the relation between the concept of secondary reinforcement and what Mowrer and Jones have called the *discrimination hypothesis*—the assumption that rate of extinction is inversely related to the similarity between conditions of training and extinction.<sup>1</sup> In contemporary learning theory, the effect of change in the afferent consequences of a response upon its resistance to extinction has been considered primarily in terms of the principle of secondary reinforcement—a neutral stimulus acquires reinforcing properties as a function of repeated association with primary reinforcement.<sup>2</sup> From this point of view the extinction of a response must inevitably be retarded by stimuli which have been contiguous with reinforcement during training. It is doubtful, however, that the effect of change in the afferent consequences of response can be fully understood in this way. The presence during extinction of stimuli encountered in training may contribute to the similarity of the two series of events and thus, to some extent at least, sustain response independently of previous association with reinforcement. The validity of this conception may be tested under circumstances in which the transition from training to extinction is more readily discriminated in the presence of a secondarily reinforcing stimulus than in its absence.

*Problem.* Rats are trained to traverse a runway and enter a goal-box. On half of the trials they are rewarded in a box of one color (*e.g.* white), while on the remaining trials they find an empty box of another color (*e.g.* black). Reinforced and non-reinforced trials are randomly alternated. After considerable training of this kind the animals are extinguished, some with the white box and some with the black. From the principle of secondary reinforcement it must be predicted that extinction will be less rapid

\* Accepted for publication May 14, 1952. This study was supported in part by a grant from the Research Council of the University of Texas.

<sup>1</sup> O. H. Mowrer and Helen Jones, Habit strength as a function of the pattern of reinforcement, *J. Exper. Psychol.*, 35, 1945, 293-311.

<sup>2</sup> C. L. Hull, *Principles of Behavior*, 1943, 84-101.



with the white box, but the discrimination hypothesis suggests the reverse outcome when we take into account the afferent consequences of non-reinforced as well as reinforced responses. For animals extinguished with black, the early trials represent no discriminable departure from the training conditions—non-reinforcement in the black box having been an integral part of the training series—and eventual discrimination of the change in experimental conditions must be based on the relative frequency of black-non-reinforcement and white-reinforcement experiences. For the animals extinguished with the white box, however, the consequences of the first response represent a marked departure from the conditions of training. In this experiment, therefore, when discriminability is estimated from the congruence of the entire series of training experiences (serial patterning) with the events of extinction,<sup>3</sup> the discrimination hypothesis leads to a prediction which contradicts that derived from the principle of secondary reinforcement. It is conceivable, of course, that two opposed effects might operate in such an experiment—a preference for the reinforced color which would favor one group,<sup>4</sup> and a patterning effect which would favor the other. For purposes of comparison, therefore, a control group was introduced for which goal-boxes of the same color were used both on reinforced and non-reinforced training trials. Under these conditions the two effects may be regarded as operating in the same direction.

*Subjects.* Forty-eight experimentally naïve Albino rats were studied. They ranged in age from 3–4 mo. at the beginning of the experiment.

*Apparatus.* The apparatus employed may perhaps best be described as a combination elevated runway and single-window jumping stand. The runway was 3.75 in. wide and 7.66 ft. long. At its distal end was a goal-box containing a 6 × 6 in. window. The animal gained access to the box by jumping a 9-in. gap from the end of the runway to an unfastened card situated in the window. The purpose of this arrangement was to prevent the animal from seeing the contents of the goal-box until the terminal response was made. The sides and top of the goal-box were funnelled to minimize abortive jumping. The entire apparatus was painted mid-gray except the card in the window of the goal-box (which was covered with ½-in. vertical black and white stripes) and the interior of the goal-box (which, on a given trial, was either mid-gray, black, or white). The experiment was conducted in the homogeneous Dome Room of the small animal laboratory.

*Preliminary adjustment.* The animals were placed on a 24-hr. feeding schedule

<sup>3</sup>D. W. Tyler, E. C. Wortz, and M. E. Bitterman, The effect of random and alternating partial reinforcement on resistance to extinction in the rat, this JOURNAL, 66, 1953, 57-65.

<sup>4</sup>I. J. Saltzman, Maze learning in the absence of primary reinforcement: A study of secondary reinforcement, J. Comp. Physiol. Psychol., 42, 1949, 161-173.



which gradually reduced them (over a period of several weeks) to 80% of satiated bodily weight. By gearing the amount fed after each day's trials to weight-loss in the preceding 24-hr. interval, each animal could be brought very close to the 80-% level at the start of the next day's trials. Preliminary training consisted of feeding the animals in the gray goal-box for brief periods and teaching them to jump gradually increasing distances (to a maximum of 9 in.) from the end of the runway, first to the open window and then to the striated card. Each jump was rewarded with a few seconds of feeding from a cup of wet mash in the goal-box.

*Experimental training.* Each animal was given 10 trials per day for a period of 10 days. Each trial consisted of a run from the beginning of the runway to its end and a jump from there to the striped card in the window of the goal-box. The animal remained in the goal-box for 10 sec. and was then transferred to a mesh waiting cage for a period of 20 sec. At the conclusion of this period the next trial was begun or (after 10 trials) the animal was returned to the home cage and fed. On each trial the time which elapsed between placing the animal on the runway and its jump to the goal-box was measured with a stopwatch. If on any training trial the animal did not reach the goal-box in a period of 90 sec. it was manually guided in the direction of the goal and encouraged to jump from the end of the runway. All animals were reinforced (10 sec. of feeding with wet mash) on 5 of the 10 daily trials. Reinforcement and non-reinforcement were administered in accordance with selected Gellerman orders.<sup>5</sup>

The animals were divided into two principal groups whose training differed in only one respect. For the 23 animals of Group I the interior of the goal-box was the same color (either white or black) on both reinforced and non-reinforced trials; white was used for 11 of the animals and the remaining 12 were trained with black. For the 25 animals of Group II the interior of the goal-box was one color on reinforced trials and the opposite color on non-reinforced trials; of these animals 12 were reinforced in a white box and 13 in a black box. To ensure the complete absence of food in the goal-box on non-reinforced trials separate goal-boxes were used on reinforced and non-reinforced trials. The procedure thus necessitated four interchangeable boxes—two with white interiors and two with black interiors.

*Extinction.* After the 10-day training period a 15-day extinction period ensued. Ten non-reinforced trials per day were given; as on non-reinforced trials of the training series no food was present in the goal-box in which the animals were confined for 10 sec. The 20-sec. interval between trials was again spent in the waiting cage. Runs were timed as before. If an animal did not reach the goal-box in 90 sec. on any given trial it was removed from the runway and placed in the waiting cage for the usual 20-sec. period. The criterion of extinction was two successive incomplete trials of this kind. Work with an animal was discontinued after it met this criterion.

Each of the two principal groups was divided into two parts which were treated differently during extinction. Groups I-S and II-S—the secondary reinforcement groups—were extinguished with the goal-box color which had been associated with reinforcement during training, while Groups I-N and II-N were extinguished with the goal-box color never previously associated with reinforcement. These groups were

<sup>5</sup> L. W. Gellerman, Chance orders for alternating stimuli in visual discrimination experiments, *J. Genet. Psychol.*, 42, 1933, 356-360.



so selected that in each case approximately half the animals were extinguished with white and half with black.

*Results.* The course of learning in the two major groups was quite similar, although the performance of Group I was somewhat superior to that of Group II in the early stages of training. The mean log time per trial for the 10 days of training was 0.966 for Group I and 1.022 for Group II, the difference falling short of significance at the 5% level ( $t = 1.94$ ). It seems, therefore, that the additional source of secondary reinforcement available on non-reinforced trials to the animals of Group I did not markedly influence rate of learning. The presence or absence on non-reinforced trials of the color associated with reinforcement did, however, have an observable effect on behavior. All animals showed signs of frustration on non-reinforced trials—turning abruptly away from the distal end of the goal-box where food was found on reinforced trials, exploring the entrance to the goal-box, climbing to the top of the enclosure, and so forth. After several days of training this behavior would appear in the animals of Group II immediately upon their entrance to the goal-box, while in the animals of Group I this behavior did not ordinarily appear until the distal end of the goal-box had been investigated. The animals of Group II were apparently discriminating between positive and negative colors.

In Fig. 1 the performance of Groups I-S and I-N are plotted in terms of mean log time for the last 10 training trials and the first 30 extinction trials. Comparable plots for Groups II-S and II-N are shown in Fig. 2. The curves for I-S and I-N are quite similar during training and diverge during extinction in the manner predicted from the concept of secondary reinforcement. The difference between the two groups did not, however, attain significance during this period; for the first three days of extinction the mean log time per trial for Group I-N was 1.257 and the corresponding mean for Group I-S was 1.133 ( $t = 1.66$ ,  $p > .05$ ). The curves for Groups II-S and II-N, which are similar during the training period, diverge during extinction in a manner *opposite* to that predicted from the concept of secondary reinforcement. The difference between the two groups was significant beyond the 1% level for each day of extinction considered separately and for the three days combined. The mean log time per trial for the first 30 trials of extinction was 1.434 for Group II-S and 1.150 for Group II-N ( $t = 6.72$ ).

In Fig. 3 the course of extinction in all four groups is plotted in terms of mean log time per day for the 15-day extinction series. In these curves



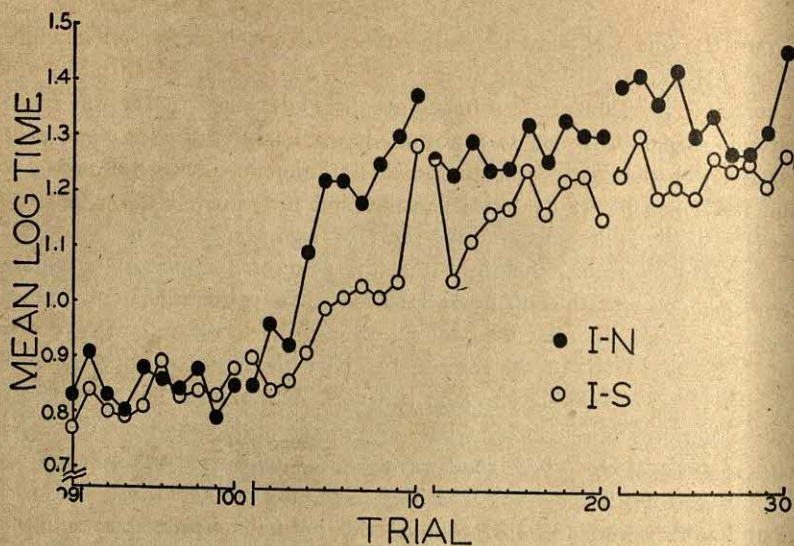


FIG. 1. THE PERFORMANCE OF GROUPS I-N AND I-S ON THE LAST DAY OF TRAINING AND THE FIRST THREE DAYS OF EXTINCTION

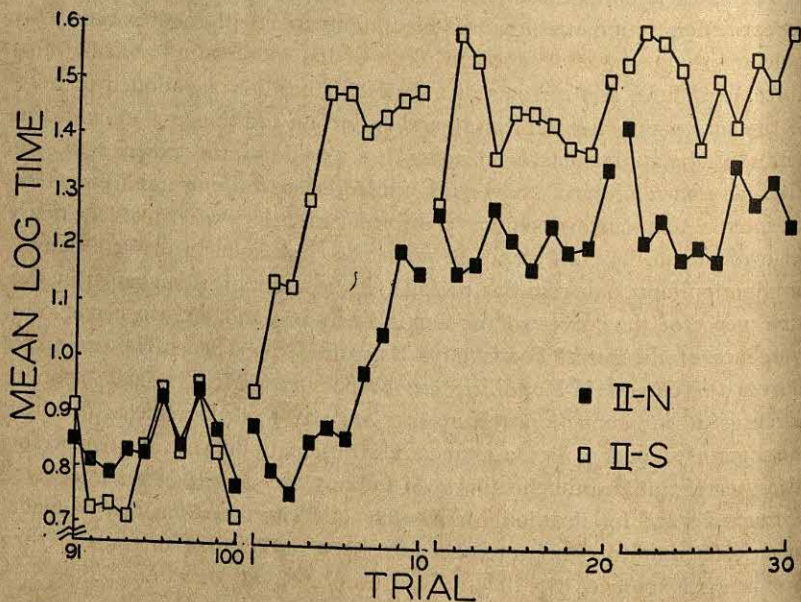


FIG. 2. THE PERFORMANCE OF GROUPS II-N AND II-S ON THE LAST DAY OF TRAINING AND THE FIRST THREE DAYS OF EXTINCTION



Groups I-S and I-N show a significant secondary reinforcing effect while Groups II-S and II-N again show a significant reversal. Mean scores are summarized in Table I. It should be noted, however, that the difference between Groups II-S and II-N is initially maximal and then declines progressively in the course of extinction. The difference between Groups I-S and I-N is small to begin with and increases progressively during extinc-

TABLE I  
MEAN LOG TIME PER TRIAL FOR THE FOUR GROUPS  
DURING THE 15-DAY EXTINCTION PERIOD

Group	Time	Diff.	t	Sig.
I-N	1.602	0.170	2.12	P < .05
I-S	1.432			
II-N	1.462	-0.116	2.08	P < .05
II-S	1.578			

tion. The curves for Groups I-S and II-N are in all respects quite similar. The curves for Groups I-N and II-S, while similar in mean level, show a distinct cross-over in the course of extinction.

The most important result of this experiment is the greater resistance to extinction found in Group II-N as compared with Group II-S. This outcome cannot be understood in terms of the concept of secondary reinforcement. It is, however, predictable from the discrimination hypothesis if the similarity between training and extinction conditions is evaluated in terms of the congruence of the entire series of training experiences—including non-reinforced as well as reinforced trials—with the events of extinction. Although secondary reinforcement *per se* was not very much in evidence in the early stages of extinction, it seemed to play an increasingly important rôle from day to day. This assumption is suggested by the divergence of the curves for Groups I-S and I-N and the concurrent convergence of the curves for Groups II-S and II-N. The cross-over in the curves for Groups I-N and II-S can be understood in the same manner. Even if it is assumed, however, that secondary reinforcement played a negligible rôle early in the extinction series, it is difficult to account for the greater initial difference between Groups II-S and II-N than between Groups I-S and I-N. Although certain general interpretations may be made with fair confidence from a concept of serial patterning, findings of this sort suggest the need for independent evaluations of discriminability.

In view of the brief interval between trials which was employed in the present experiment, the possibility should be considered that all of the results can be ac-



counted for if the principle of secondary reinforcement is supplemented with the principles of stimulus-generalization and stimulus-compounding. The massing of training is critical because traces of the afferent consequences of a response may under these circumstances be carried over to the start of the next trial. Consider the case of an animal which is both reinforced and non-reinforced in the white box. If the alley stimuli are designated with the letter *A*, the trace of primary reinforcement with *R*, the traces of stimuli arising from frustration with *F*, and the traces of white and black with *W* and *B*, then half the reinforced responses of this animal are made

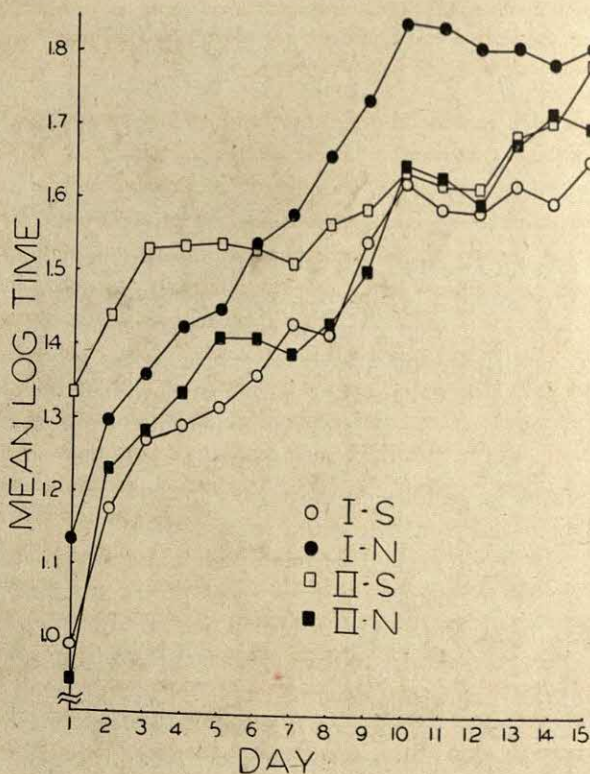


FIG. 3. THE COURSE OF EXTINCTION IN THE FOUR GROUPS

to the compound  $S_{A,W,R}$  and the other half to the compound  $S_{A,W,F}$ . This animal should extinguish less rapidly with the white box than with the black, since the compound  $S_{A,W,F}$  has been reinforced in training while the compound  $S_{A,B,F}$  has not. For an animal reinforced in the white box and non-reinforced in the black box—*i.e.* reinforced for responses to compounds  $S_{A,W,R}$  and  $S_{A,B,F}$ —extinction with the black box should be less rapid than with the white since the compound  $S_{A,B,F}$  has been reinforced in training while the compound  $S_{A,W,F}$  has not.



This interpretation is faced with a number of difficulties. In the first place it conveniently ignores the operation of secondary reinforcement—the very concept which it is designed to save. If both concepts are taken seriously it must be recognized that both operate to produce more rapid extinction in Group I-N than in Group I-S. In the case of Groups II-N and II-S, however, the two factors work in opposite directions. Nevertheless, in the first stage of extinction, at least, there was a significant difference between II-N and II-S but not between I-N and I-S. Even if some acceptable reason is found for postponing the hypothetical operation of secondary reinforcement, the greater N-S difference in the case of Group II poses a problem. Furthermore, the explanation of differential resistance to extinction in terms of generalization-decrements based on the carry-over of afferent consequences of response has been quite thoroughly discredited in recent experiments.<sup>6</sup>

It might be well to note the relation between the results here reported and those obtained in another study of the development of secondary reinforcement in the course of training on a runway.<sup>7</sup> Under conditions corresponding to those of Group I and also under conditions corresponding to those of Group II, Saltzman has demonstrated the operation of secondary reinforcement with a test involving the competition of two end-boxes in a simple T-maze. Saltzman's experiment differed in a number of ways from that here reported, but the crucial difference probably lies in the kind of test employed. It should also be noted that the present results vary in some respects from those obtained in preliminary explorations, briefly described elsewhere,<sup>8</sup> which were performed with a somewhat cruder apparatus and a less rigorous control of motivation. In one of these exercises a comparison of the same kind as that between Groups II-S and II-N failed to yield a reliable difference; the difference obtained was in the same direction but it fell just short of the 5-% level of confidence. A more serious discrepancy appeared in a preliminary experiment which involved a comparison like that between Groups I-S and I-N—a difference significant at just about the 5-% level, but in a direction opposite to that here reported, was obtained. The earlier result in all likelihood represents a random fluctuation, since there is no reasonable basis for expecting it. A series of experiments under conditions of Group I would probably indicate that the true difference is not large.

<sup>6</sup> Janet Crum, W. L. Brown, and Bitterman, The effect of partial and delayed reinforcement on resistance to extinction, this JOURNAL, 64, 1951, 228-237; E. D. Longenecker, John Krauskopf, and Bitterman, Extinction following alternating and random partial reinforcement, this JOURNAL, 65, 1952, 580-587; Tyler, Wortz, and Bitterman, *op. cit.*, 57-65.

<sup>7</sup> Saltzman, *op. cit.*, 161-173.

<sup>8</sup> E. F. MacCaslin, W. E. Feddersen, and Bitterman, Secondary reinforcement and partial reinforcement, *Amer. Psychol.*, 7, 1952, 274, (abstract).



## SUMMARY

Rats were trained to traverse a runway and enter a goal-box (black or white) under conditions of random 50% reinforcement. Group I was reinforced in a goal-box of one color and non-reinforced in a goal-box of the *same* color. Group II was reinforced in a goal-box of one color and non-reinforced in a goal-box of the *opposite* color. Then the animals were extinguished, half of each group with the reinforced color (Subgroup S) and half with the color not previously associated with reinforcement (Subgroup N). Group I-S extinguished less rapidly than I-N, and Group II-N extinguished less rapidly than II-S. These results are discussed in relation to the discrimination hypothesis, secondary reinforcement, and stimulus-generalization.



## DISCRIMINATIVE THRESHOLDS OF SALT FOR NORMAL AND ADRENALECTOMIZED RATS

By A. E. HARRIMAN, Trinity University, and R. B. MACLEOD,  
Cornell University

In 1932, Loeb showed that Addison's disease is invariably accompanied by a significant decline in plasma sodium.<sup>1</sup> Subsequent studies have indicated that adrenalectomy has a similar effect, and that the deficiency symptoms of adrenalectomized animals and of patients with Addison's disease are temporarily, although not permanently, alleviated by the addition of sodium compounds to the diet.<sup>2</sup> Cannon, in developing his doctrine of homeostasis, laid great stress on the importance of sodium metabolism for the health of the organism, and devoted considerable attention to the physiological process whereby constancy of salt content is maintained.<sup>3</sup> We are indebted to Richter, however, for a series of challenging researches designed to reveal the behavioral mechanisms involved in salt homeostasis. His theory represents a further extension in the behavioral direction of the general principle of physiological homeostasis. The present investigation was designed to test the adequacy of some parts of this theory.

According to Richter, the failure of sodium metabolism in complete adrenalectomy involves chemical changes in the organism which result in an accentuation of the 'specific appetite' for salt.<sup>4</sup> Furthermore, this increase in appetite for salt is accompanied by increased sensitivity of the taste mechanism.<sup>5</sup> For 12 normal rats the lowest level of preference for a salt solution as compared with distilled water occurred at an average salt concentration of 0.055%, with a range from 0.035 to 0.080%. Four adrenalectomized rats, on the other hand, began to make the discrimination at an average salt concentration of 0.0037%. This finding, if confirmed, is of the utmost

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\* Accepted for publication September 16, 1952. The experiments were conducted by A. E. Harriman, John Wallace Dallenbach Memorial Fellow in Experimental Psychology at Cornell University.

<sup>1</sup> Robert Loeb, Chemical changes in the blood in Addison's disease, *Science*, 76, 1932, 420-421.

<sup>2</sup> Loeb, Effect of sodium chloride in treatment of a patient with Addison's disease, *Proc. Soc. Exper. Biol. Med.*, 30, 1933, 808-812.

<sup>3</sup> W. B. Cannon, *The Wisdom of the Body*, 1939, 91-97.

<sup>4</sup> C. P. Richter, Increased salt appetite in adrenalectomized rats, *Amer. J. Physiol.*, 115, 1936, 155-161.

<sup>5</sup> Richter, Salt taste thresholds of normal and adrenalectomized rats, *Endocrinology*, 24, 1939, 367-371.



importance for the theory of appetite as well as for the theory of receptor sensitivity.

Certain questions are raised, however, by Richter's procedure and by the results of some subsequent investigations. (1) Richter did not demonstrate that adrenal-cortical tissue may not have been present in his adrenalectomized animals in sufficient quantity to negate the expected effect of the operation, nor did he attempt to exhaust the adrenal-cortical hormone remaining in the body following surgery. (2) During the first post-operative day Richter subjected his adrenalectomized animals to a choice between distilled water and a salt solution. The concentration of the solution was increased daily by small and standard steps. The point at which the animals distinguished one fluid from the other was accepted as the threshold. This seems to imply the assumption that the condition of salt need in the adrenalectomized rat is a constant factor rather than part of a progressively deteriorative syndrome. (3) By the ninth post-operative day the animals discriminated one fluid from the other. By the ninth day after a successful operation most of the remaining hormone will have been lost from the body and a serious deficiency will have been engendered if, indeed, the animal has not died.<sup>6</sup> During this period, however, the animal has had an opportunity to establish an association between the ingestion of a fluid and the reduction of tissue need. Although Richter discounts the possible benefit which may be derived from the ingestion of low concentrations of salt, the question may still be an open one. In one study, newly weaned rats were placed on a diet containing only 0.002% sodium.<sup>7</sup> These animals lived for a period of from 18 to 20 weeks. Thus Richter's results do not fully exclude a 'learning' hypothesis; nor, for the same reasons, do those of Bare, who substantially repeated Richter's procedure and confirmed Richter's results.<sup>8</sup> It may be that the difference in discriminatory performance between the normal and the adrenalectomized rat rests not on a lowering of sensory threshold but rather on an increase in the motivation to discriminate. It is possible that the sensory capacity of the normal animal is no different from that of the adrenalectomized animal. (4) Using an electrophysiological technique, Pfaffmann and Bare made threshold determinations by recording potentials from the glosso-pharyngeal nerve.<sup>9</sup> As thus determined, there was no difference in sensitivity between normal and adrenalectomized rats. To reconcile this finding with those of Richter, Pfaffmann and Bare suggest that there are two independent and coexisting thresholds of taste for salt: an absolute physiological threshold and a variable preference threshold. In the normal animal, the later is far above the absolute threshold. Following adrenalectomy, the preference threshold more nearly coincides with the absolute threshold. The nature of the mechanism responsible for this lowering of the preference threshold after adrenalectomy remains for them an open question.

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<sup>6</sup> Robert Gaunt, Survival period of bilaterally adrenalectomized rats, *Proc. Soc. Exper. Biol. and Med.*, 29, 1932, 823-825.

<sup>7</sup> Elsa Orent-Keiles, Aaron Robinson, and E. V. McCollum, The effects of sodium deprivation on the animal organism, *Amer. J. Physiol.*, 119, 1937, 651-661. Referred to in E. J. Farris and J. Q. Griffith (ed.), *The Rat in Laboratory Investigation*, 2nd, ed., 1949, chap. 5.

<sup>8</sup> J. K. Bare, The specific hunger for sodium chloride in normal and adrenalectomized white rats, *J. Comp. Physiol. Psychol.*, 42, 1949, 242-253.

<sup>9</sup> Carl Pfaffmann and Bare, Gustatory nerve discharges in normal and adrenalectomized rats, *J. Comp. Physiol. Psychol.*, 43, 1950, 320-324.



## PROBLEM

The distinction between an absolute and a preferential threshold may be a useful distinction, but it leaves certain essential questions unanswered. Threshold determinations in classical psychophysics have always assumed standard conditions of judgment, including conditions of motivation. When we speak of an absolute threshold we refer to the (possibly hypothetical) physiological limit of sensitivity determined under optimum conditions. No threshold determination is fully meaningful unless the conditions of the judgment are completely specified. A 'preferential' as distinguished from an 'absolute' threshold can be made meaningful only if we specify the conditions of the judgment—in this case, the conditions of motivation—that differentiate it from the conditions that might permit the determination of an absolute threshold. Any threshold determination, thus specified, may be considered valid. Without such a specification, it is of little value.

The present problem thus resolves itself into that of determining the threshold. Three questions emerge: (1) Does the threshold for salt discrimination, as measured by the method of free-selection, represent the limit of salt-sensitivity for the normal rat? Or, will the intensification of motivation lower the threshold? (2) Does adrenalectomy lower the absolute threshold for salt? (3) If there is no significant difference in the absolute threshold for salt between normal and adrenalectomized rats, how can the difference in the preferential threshold be explained? The following experiments were designed to answer the first two of these questions. The answer to the third question must await further study.

## EXPERIMENT 1

Experiment 1 was undertaken to determine whether the normal rat's discriminative threshold for salt, as measured by the method of free choice, is the animal's limit of sensitivity to salt.

*Procedure.* Twelve experimentally naïve rats, approximately 150 days old and of the Wistar or the Sprague-Dawley strains, were used. During a 5-day period of familiarization, they were deprived of water for 24 hr. They were then introduced, one at a time, into a metal discrimination-box. They were permitted to drink to satiation from either or both of the brass drinking tubes attached to nearby inverted bottles containing distilled water. On the sixth day, after 12 hr. of water-deprivation, a salt concentration of 2% was substituted for distilled water in one of the two bottles. Ingestion of the salt concentration for a period of 10 sec. was punished by electric shock lasting approximately 0.5 sec. The shock was delivered through the drinking tube. The punished rat was then permitted to drink from the other bottle



for 10 sec. Whenever a shocked rat continued to drink after an initial shock, the shocks were continued for the same interval, every 5 sec., for a period of 30 sec. An initial choice of distilled water was rewarded by a 15-sec. period of uninterrupted access to the bottle. Upon termination of one of the drinking periods, the rat was removed from the discrimination-box. After a period of approximately 10 sec., the rat was returned to the box.

The attempt was made to give each rat 10 trials in series. Each series was separated from the next by a 12-hr. interval. At the end of the second series in a 24-hr. period, each rat was given an opportunity to satiate itself from the distilled water bottle. The water was made accessible only in the experimental situation. Food—a commercial Purina dog chow, containing 1% salt—was available in unlimited quantities in the living cages.

After the last trial in a series for a given rat, the drinking tubes were rinsed and wiped. At the end of an experimental period, all bottles and tubes were thoroughly cleaned. The tubes were randomly interchanged among the bottles throughout the course of the experiment.

If a rat received no more than one shock in each of two successive series of trials, discrimination between the salt solution and distilled water, at a particular level, was considered to be achieved. Furthermore, if, after 20 series, a rat had not met the criterion at a particular level, it was assumed to have reached its limit and was subjected to no further tests.

*Results.* All of the rats learned to discriminate between distilled water and a 2% salt solution after approximately 20 trials. Four rats were then used as controls. Each was run just as though one of the bottles contained salt, and was punished if it chose a bottle arbitrarily designated as 'salt solution.' These four rats responded in a chance fashion to the bottles in the course of 10 series of trials. Moreover, the rats frequently exhibited deviant behavior in the box, such as by exploring, washing, crouching, and, when picked up by the experimenter, squealing. Therefore, it was assumed that, on the basis of reactions of the control group, the original group had been responding negatively to the salt concentration.

The lowest concentration of salt discriminated from distilled water by each of the rats in the experimental group varied from 0.002% to 0.000025%.<sup>10</sup> This range is far below that reported by Richter, and even

<sup>10</sup> In a recent study under conditions fairly comparable to those of this experiment, Carr (W. J. Carr, The effect of adrenalectomy upon the NaCl taste threshold in rat, *J. Comp. Physiol. Psychol.*, 45, 1952, 377-380) obtained a threshold of 0.009% with normal rats and 0.012% with adrenalectomized rats. The mean thresholds obtained with each of these groups are above those obtained by Richter with adrenalectomized rats. Carr's procedure, however, involved fewer and larger steps in the reduction from one concentration to the next—a maximum of two successive reductions were employed, with no more than 60 trials at any given concentration as compared trials at a given concentration prior to the elimination of an animal. The experience of the present investigators indicates that, for the finest discriminations, the reduction steps must be small and adequate practice at each step must be permitted.



TABLE I

NUMBER OF RESPONSES PRECEDING ACHIEVEMENT OF THE CRITERION AT EVERY PERCENTAGE OF SALT CONCENTRATION USED

The maximal number of trials in every series is 10. Responses are not given in multiples of 10, however, because a rat frequently refused to respond after making a wrong choice.

%	Rats							
	1	2	3	4	5	6	7	8
2.0	23	25	20	10	19	20	20	10
1.5	20	10	37	10	20	0	30	10
1.0	10	20	36	10	10	0	10	10
.9	20	10	54	20	0	0	47	10
.8	33	0	20	10	39	10	53	10
.7	20	0	19	0	20	0	29	0
.6	10	0	20	0	0	0	19	10
.5	0	10	30	0	0	0	0	10
.4	26	20	19	0	10	0	0	10
.3	13	28	10	0	10	20	0	0
.2	34	35	50	10	10	20	0	0
.1	72	40	20	0	68	10	0	0
.09	0	23	0	30	20	30	17	0
.08	23	117	10	0	20	10	0	0
.07	18	39	0	0	10	0	0	0
.06*	17	27	0	10	0	0	31	0
.05	10	20	0	9	30	0	50	0
.04	10	20	0	3	10	50	42	0
.03	10	30	0	34	70	0	49	0
.02†	10	10	0	10	71	10	87	20
.01‡	10	10	10	20	67	20	10	30
.009	77	0	50	0	10	0	10	20
.008	66	0	20	0	19	39	0	10
.007	23	30	20	18	28	10	50	10
.006	42	20	0	20	10	10	10	10
.005	53	60	30	160	0	10	0	90
.004((	27	20	10	97	0	40	0	10
.003	85	19	70	27	0	30	0	70
.002	178	10	10	10	0	0	0	68
.001	(20 ser.)	20	0	0	0	53(((	84(((	20
.0009		30	0	29	10	93	20	10
.00075		30	20	59	0	82	10	89
.0005		150	60	30	30	174	10	34
.00025		(20 ser.)	10	79	79	136	76	114
.0001			(20 ser.)	20	143	84	38	35
.00009				(20 ser.)	57	157	20	26
.000075					(20 ser.)	62	20	165
.00005						(20 ser.)	20	82
.000025							(20 ser.)	157
.00001								(20 ser.)

\* Average threshold reported by Bare (0.06%) and by Richter (0.055%) for normal rats.

† Average threshold reported by Bare (0.016%) for adrenalectomized rats.

‡ Average threshold reported by Carr (0.012%) for adrenalectomized rats.

|| Combined thresholds reported by Pfaffman and Bare for normal (0.008%) and adrenalectomized (0.01%) rats, and by Carr (0.009%) for normal rats.

(( Average threshold reported by Richter (0.0037%) for adrenalectomized rats.

(((( Rats 6 and 7 were used as supplementary controls at 0.001% concentration.



farther below the preferential threshold obtained by Bare with adrenalectomized rats. (See Table 1.) Furthermore, the thresholds obtained in this experiment are uniformly far lower than those obtained by Pfaffman and Bare with the electrophysiological technique. Pfaffman suggests that the higher thresholds produced by his method may have been due to his use of tap water rather than of distilled water in the mixing of salt concentrations.<sup>11</sup> Therefore, it is possible that a masking effect may have been obtained. The use of distilled water might have yielded thresholds comparable to those here reported.

## EXPERIMENT 2

While Experiment 1 demonstrates that, under appropriate conditions of motivation and training, the normal rat's discriminative threshold for salt can be reduced to a level even lower than that of the adrenalectomized rat tested by the method of free-choice, it does not exclude the possibility that adrenalectomy may lead directly to an increase in salt sensitivity. A further experiment is consequently required.

*Procedure.* The eight rats, the thresholds of which had been determined in Experiment 1, were retained to the point of the last successful discrimination. One hundred trials were run at this concentration, and the previously determined thresholds were verified by tests at a lower concentration. Each rat was then given two more series of trials at the concentration of last success, following which it was adrenalectomized and tested daily for 10 days.

*Results.* Seven of the eight animals survived the operation. Tested on the first post-operative day at the concentration of final success, each of these demonstrated that the discrimination habit had not been lost. Twelve hours later each rat was given the choice between distilled water and a salt concentration just below its previously determined threshold. During the 10-day testing period not one of these rats achieved a discrimination below the threshold. One additional rat died during the testing period, and all of the remaining six manifested the standard deficiency symptoms. The detailed procedure was the same as in Experiment. 1

## DISCUSSION

(1) The present experiments seem to establish two important facts, namely: (a) that through a judicious use of reward and punishment a normal rat can be trained to make a finer discrimination between salt-containing and salt-free water than has as yet been obtained with normal

<sup>11</sup> Pfaffmann, Personal communication, 1951.



or adrenalectomized rats in a free-choice situation or through electrophysiological measures of the responses of the glosso-pharyngeal fibers; and (b) that the discriminative threshold as thus measured is not lowered by adrenalectomy. It cannot be claimed, of course, that still better methods may not eventually yield still lower thresholds. It seems to be fairly clear, however, that the dramatic change in the rat's ability, following adrenalectomy, to detect salt does not necessarily imply a lowering of the absolute threshold for salt.

(2) The thesis that salt homeostasis is maintained as a result of an automatic increase in salt sensitivity consequent upon salt deficiency is called into question. The broad fact of salt homeostasis is not challenged; but the nature of the behavioral support for salt homeostasis continues to be a question for research.

(3) The 'preferential,' as opposed to the 'absolute' threshold is clearly a function of the conditions of preference, *i.e.* of choice. There are as many preferential thresholds as there are conditions that motivate choice. One might argue that even the absolute threshold is really a preferential threshold under optimum conditions of stimulation and motivation. The adrenalectomized rat, with its consequent increase in sodium 'need,' shows a marked decrease in its preferential threshold for salt. Why? Neither the present experiments nor any reported in the literature provide an adequate answer. It is clearly not a matter of increased receptor sensitivity. The problem demands further research into the relation between motivation and learning.



## BILATERAL ASPECTS OF THE TRIGONOMETRIC RELATIONSHIP OF PRECISION AND ANGLE OF LINEAR PURSUIT-MOVEMENTS

By GEORGE E. BRIGGS and W. J. BROGDEN, University of Wisconsin

In two previous reports Corrigan and Brogden have presented data on the precision of linear pursuit movements as a function of the angle of such movements.<sup>1</sup> More recently Brogden has reported the effect of practice on this relation.<sup>2</sup> In all these studies the trigonometric equation  $y = a + b \cos 2x + c \sin 2x$  represents the functional relation between angle and precision most adequately. In this equation  $y$  refers to the precision of right arm movement in terms of the mean frequently of stylus contact with the edges of a linear track (an error score);  $x$  refers to the angle from the body at which the movement is made;  $a$  is a constant which determines the baseline of the curve; and  $b$  and  $c$  are constants from which the amplitude,  $d = (b^2 + c^2)^{1/2}$ , and the phase angle,  $\cos 2e = c/d$ , are determined. When fitted to the data this function generates a regular curve of sine-wave form that completes two full cycles for the angles  $0^\circ$  through  $360^\circ$ . The angles  $180^\circ$  through  $360^\circ$  duplicate the form and displacements of the function that are evidenced for the angles  $0^\circ$  through  $180^\circ$ .

In the prior studies performance with the right hand and arm only was used. The present study was designed to determine the functional relation between precision and angle of linear pursuit movements of the left hand and arm, and the nature of bilateral transfer.

### METHOD AND PROCEDURE

*Apparatus.* The apparatus used in the present experiment was the same as that previously reported.<sup>3</sup> It consists of a track 0.4 cm. wide formed by two brass plates

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<sup>1</sup> R. E. Corrigan and W. J. Brogden, The effect of angle upon precision of linear pursuit movements, this JOURNAL, 61, 1948, 502-510; The trigonometric relationship of precision and angle of linear pursuit movements, *ibid.*, 62, 1949, 90-98.

<sup>2</sup> Brogden, The trigonometric relationship of precision and angle of linear pursuit movements as a function of amount of practice, *ibid.*, 66, 1953, 45-56.

<sup>3</sup> Corrigan and Brogden, *op. cit.*, 90 f., and Brogden, *op. cit.*



resting on a piece of glass that *S* traverses with a metal tipped stylus. The velocity of the movement of the stylus is controlled by instructions given to *S* to match the rate of his pursuit-movement to that of a small cylindrical target that travels beneath the glass plate of the track at a constant velocity of 3.0 cm. per sec. Control apparatus provides automatically for starting the target, stopping it at the end of the track (35.0 cm. from the start), and returning it to the starting position where its direction is again reversed for the start of a new trial. The circular platform on which the track is mounted may be rotated about its center by means of a bearing in the vertical plane. The angle  $0^\circ$  is represented by the track normal to, and the target parallel to, the frontal plane of *S*.<sup>4</sup> A disk attached to the bearing of the central vertical axis is notched every  $15^\circ$  to permit rapid and secure selection of the appropriate angle. The stylus and each side of the track constitute an open switch of a Potter Electronic Counter, Model 67. Each contact of the stylus with the track side is registered cumulatively on the counter.

*Subjects.* A total of 48 men were used as *Ss* in this experiment. All were volunteers from elementary classes in psychology. The criterion of selection was that they be right handed. None had had experience with the task prior to the experiment.

*Procedure.* The initial step was to seat *S* in a tank driver's seat and to fasten about his trunk a harness that kept both the right and left shoulders in a relatively fixed position. The height of the seat and its closeness to the track platform were so adjusted to body stature that *S* was both comfortable and able to reach the end of the track with the stylus when the arm was fully extended. The standardized instructions, reported verbatim in a previous paper, were modified to satisfy the conditions of the present experiment.<sup>5</sup> These modifications refer to the arm and hand used for making linear pursuit movements. Eight angles,  $0^\circ$ ,  $30^\circ$ ,  $45^\circ$ ,  $60^\circ$ ,  $90^\circ$ ,  $120^\circ$ ,  $135^\circ$ , and  $150^\circ$ , were selected for the present study with the expectation that adequate data would be available with which to fit the trigonometric equation. Ten practice trials were given with the track at the initial angular position of the sequence of angles to which *S* had been randomly assigned. The experimental design provided for initial practice on each of the eight angles by an equal number of *Ss*. When *S* had completed the 10 trials of practice and thoroughly understood the task, the experiment was begun. Ten trials were given at each of the eight angles with a 30-sec. period of rest between angles. Every trial lasted approximately 13 sec. and the interval between trials was approximately 5 sec. The total time for one day's session was therefore on the order of 35 min. Each *S* returned 23 hr. later and the same procedure was repeated with the angles appearing in the same sequence as on the previous day. The only change in the experimental situation was that *S* tracked with the hand not used in the first day's performance.

Two groups of *Ss* make up the overall design of the experiment. Group I practiced with the right hand on Day 1 and with the left on Day 2. Group II practiced with the left hand on Day 1 and with the right on Day 2. A randomized Latin square was generated from a randomly selected  $8 \times 8$  square, and it was used for each of the

<sup>4</sup> Angle is designated by Cartesian coördinates. At that angle of  $0^\circ$  the tracking movement is straight away from the frontal plane of *S* while at  $90^\circ$  the movement is from right to left.

<sup>5</sup> Corrigan and Brogden, *op. cit.*, 502 f.



two groups for each of the two experimental sessions. Thus every *S* went through the same sequence of angles on Day 2 as he had on Day 1, but the *Ss* of Group I used the right hand on Day 1 and the left on Day 2, whereas for Group II, the left hand was used on Day 1 and the right on Day 2. The assignment of *Ss* to the rows (sequences) of the Latin square on the first day was random. Since *n* for each group is 24, there are 3 replications of each Latin square.

## RESULTS

Since the distributions of the error scores were found to be skewed, the same transformation used on the data of previous studies was applied.<sup>6</sup> The logarithm (base *e*) was obtained for the sum of the raw scores + 5 for each *S* on every trial. Means were computed for each 10 trial block for each angle on each day for every *S*. The analyses of variance performed on these latter data follow the procedure described by Grant.<sup>7</sup> Table I summarizes the results obtained from each of the four analyses. For each anova the error term (Source 6) is free of all variation due to sequence,

TABLE I  
RESULTS FROM ANALYSES OF VARIANCE

	Source	df.	Day 1			Day 2		
			Sums of squares	Mean square	F	Sums of squares	Mean square	F
Group I	(1) Angle	7	455.03	65.00	24.71*	328.01	46.86	19.28*
	(2) Sequence	7	117.82	16.83	6.40*	99.78	14.25	5.86*
	(3) Ordinal position	7	71.13	10.16	3.86*	18.24	2.61	1.07
	(4) Individual differences within rows	16	331.01	20.69	7.87*	594.50	37.16	15.29*
	(5) Square uniqueness	42	116.62	2.78	1.06	100.07	2.38	0.98
	(6) Error	112	204.00	2.63		272.05	2.43	
Group II	(1) Angle	7	375.03	53.58	24.03*	366.72	52.39	26.87*
	(2) Sequence	7	254.96	36.42	16.33*	63.22	9.03	4.63*
	(3) Ordinal position	7	56.41	8.06	3.61*	21.88	3.13	1.61
	(4) Individual differences within rows	16	779.34	48.71	21.84*	427.34	26.71	13.70*
	(5) Square uniqueness	42	75.43	1.80	0.81	87.04	2.07	1.06
	(6) Error	112	249.27	2.23		218.85	1.95	

\* Significant at the 1-% level of confidence.

ordinal position, angle, *Ss*, and square uniqueness. Thus it is a relatively pure error estimate. This term can be used in the computation of *F*-ratios only if variation due to square uniqueness (Source 5) is not significant. Since this is so for all four Latin squares, the residual error (Source 6) was used to determine the *F*-ratios in Table I.

In all four analyses, the *F*-ratio for angles *vs* error is significant at the 1-% level of confidence. Significant differences in performance at the vari-

<sup>6</sup> Corrigan and Brogden, *op. cit.*, 90 f., and Brogden, *op. cit.*

<sup>7</sup> D. A. Grant, The Latin square principle in the design and analysis of psychological experiments, *Psychol. Bull.*, 45, 1948, 427-442.



ous angles occur therefore, when either the right or the left hand is used.

Sequence as a main effect (Source 2) refers to the particular sequences of angles randomly selected for the Latin square utilized in this study. From Table I it can be seen that in all four anova this variable is significant at the 1-% level. Individual differences within rows (Source 4) provides, however, another error-term to test the sequence mean square. If an *F*-ratio for sequence *vs* individual *Ss* within sequence were significant, it would indicate significant variation in the sequences beyond that attributable to individual differences between *Ss*. No mean square for sequence (Source 2) is, however, greater than the relevant mean square for individual differences within rows (Source 4). The significance found for sequence *vs* residual error may, therefore, be attributed to the individual differences between *Ss* and not to differences between sequences.

Ordinal position of angles is significant for both Groups I and II for Day I only. A plot of the means for each of the three positions for each group on Day 1 shows a progressive increase in precision of performance as ordinal position increases. These results are comparable to those of the earlier studies in which a significant practice effect was found for the first day of the experiment, but none for the second day.<sup>8</sup>

The results from the analyses of variance make possible an examination of the data in terms of the primary purposes of the experiment. These are the relationship between precision and angle of linear pursuit movements of the left hand and arm, and the nature of bilateral transfer. Group means of precision of performance were computed for each angle for each group for each of the two experimental sessions. The trigonometric equation,  $y = a + b \cos 2x + c \sin 2x$ , was fitted by the method of least squares to each of these four sets of data. The regression equations are as follows:

Group 1, Day 1, right:	$y = 1.97 - 0.0733 \cos 2x + 0.1813 \sin 2x;$
Group I, Day 2, left:	$y = 2.11 - 0.0100 \cos 2x + 0.1610 \sin 2x;$
Group II, Day 1, left:	$y = 2.14 - 0.0467 \cos 2x + 0.1690 \sin 2x;$
Group II, Day 2, right:	$y = 1.92 - 0.0567 \cos 2x + 0.1645 \sin 2x.$

Fig. 1 presents a plot of the empirical data and of the regression equations.

The trigonometric equation provides a satisfactory quantitative description of the relation between precision and angle of linear pursuit-movements of the left hand and arm as well as for the right. There are several noteworthy differences, however, between the functions for the left and

<sup>8</sup> Corrigan and Brogden, *op. cit.*, 502 f. and 90 f.



right hand and arm. First, that for the left arm is an inversion of the function for the right arm. The first half cycle for the right is positive (large error scores) and for the left hand is negative (small error scores).

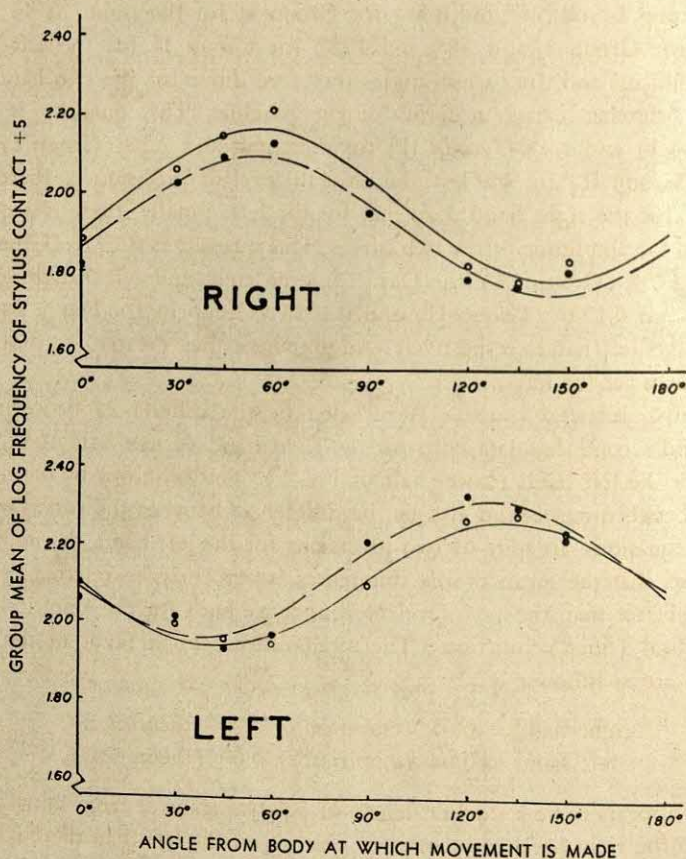


FIG. 1. BILATERAL TRANSFER AND THE TRIGONOMETRIC RELATIONSHIP OF PRECISION AND ANGLE OF LINEAR PURSUIT MOVEMENTS OF THE RIGHT AND THE LEFT HAND AND ARM

The open circles are the means for Group I and the filled circles are the means for Group II. Each curve is the least squares fit to the empirical data of the trigonometric equation,  $y = a + b \cos 2x + c \sin 2x$ . The solid lines are for Group I and the broken-lines for Group II.

The second half cycle shows a similar inverse relation between the two hands: negative for the right (small error scores) and positive for the left (large error scores). The inverse relation between the two curves is not,



however, perfect. The phase angle,  $2e$ , differs for the two. It is  $-1^\circ$  and  $-7^\circ$  for the left hand curves of Groups I and II respectively, and  $11^\circ$  and  $9^\circ$  for the right hand curves respectively. Thus maximal displacement occurs at different angles for the right and left hands; at  $56^\circ$  and  $146^\circ$  for Group I, and  $54^\circ$  and  $144^\circ$  for Group II for the right; at  $44^\circ$  and  $134^\circ$  for Group I and  $38^\circ$  and  $128^\circ$  for Group II for the left. The most difficult and the easiest angles therefore differ for the two hands. A third difference exists in terms of the baseline. This quantity is 1.97 (Group I) and 1.92 (Group II) for the right, and 2.11 (Group I) and 2.14 (Group II) for the left. The overall level of precision is, therefore, higher for the right hand than that for the left. Finally, there is a difference in the amplitude of the two curves. This quantity is 0.1956 (Group I) and 0.1753 (Group II) for Day 1 for the right and left hands respectively, and 0.1740 (Group II) and 0.1613 (Group I) for Day 2 for the right and left hands respectively. Amplitude is thus greater for the right hand than for the left.

Positive bilateral transfer is revealed by the differences between the first and second day data both for the right hand (upper half of Fig. 1) and for the left hand (lower half of Fig. 1). This is shown by the differences between means and also by the differences between the curves of the fitted equations. In spite of two inversions for the left hand, the null hypothesis that the mean of the differences is zero may be rejected by a  $t$ -test at better than the 1-% level of confidence both for the empirical and theoretical (fitted) functions. The mean differences in favor of the Day 2 data are as follows:

$$\begin{aligned}\text{right hand} &= 0.05 \text{ (empirical)}; 0.05 \text{ (theoretical).} \\ \text{left hand} &= 0.04 \text{ (empirical)}; 0.03 \text{ (theoretical).}\end{aligned}$$

There appears to be a smaller degree of positive transfer from initial practice on the right hand to subsequent practice on the left than there is from initial practice on the left to subsequent practice on the right hand.

## DISCUSSION

The results of the present experiment are in conformity with information about other bilateral relations, and with the results of studies of bilateral transfer of motor skills, and prior studies on the precision and angle of linear pursuit movements. The inversion of the curve of precision and angle of linear pursuit movements of the left hand and arm from that for the right hand and arm is what might be expected in view of the inversion



of anatomical and structural relations between the two sides of the body. The places of attachment and the angles through which the members of the arms and hands move at the joints show such an inverse relation. The inversion of the relation between precision and angle of linear pursuit movements for the right and left hands is not perfect, however. There is lack of perfect asymmetry in phase angle of the curves, as well as in amplitude. These characteristics of the curve represent differences in the relative difficulty of the different angles for the two hands. Not only are there differences in the angles that are the most and least difficult for the two hands, but there appears to be a greater difference in difficulty between the most and least difficult angles for the right than for the left hand. The difference in the baseline for the two curves in favor of a greater precision for the right hand reflects the usual dominance of the right over the left side of the body. Since only right handed Ss were used, this difference was to be expected. The trigonometric function,  $y = a + b \cos 2x + c \sin 2x$ , can be used to express quantitatively the relation between precision and angle of linear pursuit movements of the left hand and arm.

The equations fitted to the right hand data for Groups I and II are comparable to the equations fitted to similar data of the earlier studies on precision and angle of linear pursuit movements of the right hand and arm.<sup>9</sup> No direct comparisons may be made with the equations of these previous studies since the experimental conditions are different. However, only minor variations have occurred between the constants of these several regression equations and between the phase angle,  $2e$ , and the amplitude,  $d$ .

Bilateral transfer of motor skills has universally been found to be positive and our results thus agree with the results of other studies of such transfer of motor skills.<sup>10</sup> It is not possible to compare the amount of bilateral transfer obtained in this study with the amounts obtained in other studies. However, the amount of transfer is appreciable in terms of the gains made through direct practice.<sup>11</sup>

<sup>9</sup> Corrigan and Brogden, *op. cit.*, 502 f. and 90 f., and Brogden, *op. cit.*

<sup>10</sup> C. W. Bray, Transfer of learning, *J. Exper. Psychol.*, 11, 1928, 443-467; T. W. Cook, Studies in cross education: III. Kinaesthetic learning of an irregular pattern, *J. Exper. Psychol.*, 17, 1934, 749-762.

<sup>11</sup> Brogden, *op. cit.*



## A COMPARISON OF THE INTAKE OF GLUCOSE AND SACCHARIN SOLUTIONS UNDER CON- DITIONS OF CALORIC NEED

By JAMES W. CARPER, Yale University and FORBES POLLIARD,  
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The present experiment was designed to test the effect of a specific caloric hunger on the intake of glucose and saccharin solutions, both of which are preferred to water by animals on an *ad lib* diet.<sup>1</sup> Glucose, however, furnishes calories while saccharin has no nutritive value. Hausmann has shown that when animals drink sugar solutions they reduce their food intake, keeping total caloric intake constant.<sup>2</sup> Since the drinking of saccharin solutions did not affect food intake, Hausmann concluded that the animals are not 'deceived' by the taste of saccharin.

There is evidence, however, which indicates that hunger influences the reward value of saccharin for rats. Sheffield and Roby show that hungry animals learn to respond to a stimulus-pattern related to the availability of saccharin while satiated animals do not.<sup>3</sup> Since saccharin is supposed to have no effect on hunger, we might assume that saccharin acts as a secondary reinforcement because it has taste qualities which are similar to the sugars. If saccharin is a secondary reinforcement, then introduction of hunger should affect responding for sugar and saccharin similarly. This is not the case, however, as shown by a study of the bar-pressing response with rats.<sup>4</sup> Increasing both the specific caloric hunger and general hunger decreased the rate of responding for a saccharin reinforcement, whereas the opposite was true for glucose.

The purpose of this experiment is to test whether caloric hunger differentially effects the drinking of glucose and saccharin solutions in a free

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<sup>1</sup> M. F. Hausmann, The behavior of albino rats in choosing foods: II. Differentiation between sugar and saccharin, *J. Comp. Psychol.*, 15, 1933, 419-428.

<sup>2</sup> Hausmann, *op. cit.*, 427.

<sup>3</sup> F. D. Sheffield and T. B. Roby, Reward value of a non-nutritive sweet taste, *J. Comp. & Physiol. Psychol.*, 43, 1950, 471-481.

<sup>4</sup> J. W. Carper, A comparison of the reinforcing value of nutritive and non-nutritive substance under conditions of specific and general hunger, this JOURNAL.



drinking situation. Also, if the introduction of caloric hunger increases the intake of saccharin solutions, will such an increase dissipate when the animals 'learn' that saccharin does not reduce hunger? A third purpose is to find whether animals compensate for caloric deficiency by increasing their intake of glucose—as suggested by the Hausmann study.

*Method and procedure.* (a) *Subjects.* The Ss were 12 female rats of the Lashley strain that were raised in the colony of the Department of Psychology of Johns Hopkins University. The animals were approximately 140 days old at the beginning of the experiment. They were divided into two groups matched according to weight. In the experimental sessions, six animals received saccharin solutions and six glucose solutions.

(b) *Diets.* The special diets used to control caloric need were the same as reported in an earlier study.<sup>5</sup> The adequate and inadequate diets were the same in all respects except for the fact that the inadequate had approximately one-half the caloric value of the adequate.

(c) *Procedure.* Before the beginning of the experiment the animals were adapted to the cages and to their new diet for one week. The following experimental conditions were thereafter instituted for both groups:

- Day 1-6: Water and adequate diet
- Day 7-12: Solutions and adequate diet
- Day 13-18: Water and inadequate diet
- Day 19-36: Solutions and inadequate diet
- Day 37-42: Water and inadequate diet
- Day 43-60: Solutions and adequate diet

Throughout the entire experiment the animals were fed one-half their daily ration twice daily. From Days 1 through 48 they received 10 gm. of diet per day. From Days 49 through 54 the amount was increased 1 gm. per day to hasten the removal of caloric need created by the inadequate diet.

Maximally preferred concentrations of solutions were offered the two groups: 0.13% saccharin and 4.0% glucose. During the periods when the solutions were offered, plain tap water was not available to the animals.

*Results.* The bottom half of Fig. 1 shows the average daily fluid intake of the saccharin and glucose groups for 6-day periods. The upper part of Fig. 1 shows the average weights of the animals for corresponding periods. Comparing Days 1-6, 13-18, and 37-42, we see that the water intake was the same when animals were calorically hungry and when they were satiated. During Days 7-12 when animals were on adequate diet and were offered saccharin or glucose, fluid intake increased significantly (for glucose  $p = > 0.01$  and for saccharin  $p = 0.05$ ) as one would expect.

When we compare Days 7-12 with 19-36 we see that the inadequate

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<sup>5</sup> *Ibid.*, 271.



diet produces a marked increase in the intake of both glucose and saccharin solutions. The intake of saccharin solution, however, does not increase as much as the intake of glucose solution. Over the eighteen day period (Days 19-36) there is a tendency for the intake of glucose to decrease, presumably because the animals are reducing their need by intake of

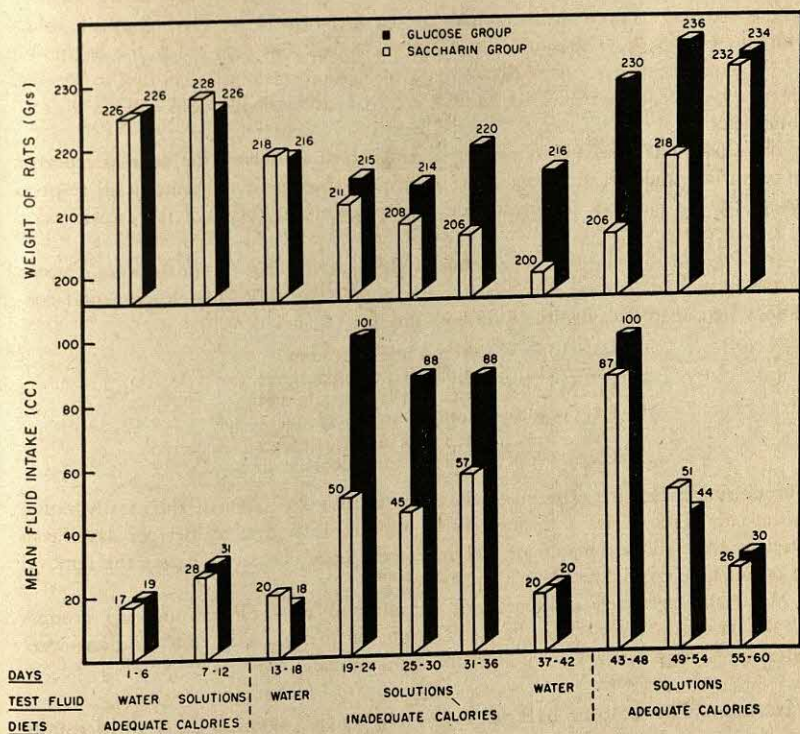


FIG. 1. RESULTS GRAPHICALLY DEPICTED

The lower part of the figure shows the average daily intake of water and saccharin and glucose solutions for the various 6-day periods. The labels on the abscissa indicate whether the animals were given water or solutions and whether they were deprived of calories. The upper part of the figure shows the corresponding weight changes.

glucose. In fact, these animals show weight gains although diet intake remains constant. On the other hand, the intake of the saccharin group shows an increase in the same period. In this case, the animals are getting no calories from saccharin and their deficiency is becoming greater as judged by continual loss of weight.

During Days 43-60 the animals are on adequate diet and their caloric



deficiencies are being corrected. During this period the consumption of glucose and saccharin solutions decreases, and correspondingly weights return to normal. By the time the weights are the same as they were at the beginning of the experiment, the intake of glucose and saccharin is approximately the same as it was in the beginning of the experiment (Days 7-12 vs. 55-60).

*Discussion.* The results of this experiment indicate that the intake of both saccharin and glucose is related to degree of caloric deficiency. The increased intake of glucose solutions by deficient animals is in line with Hausmann's findings.<sup>6</sup> The increased intake of saccharin, however, would not be predicted. These results indicate that the increased intake as a function of caloric hunger is not a simple matter of homeostasis since saccharin has no food value. Furthermore, these data do not support the notion that saccharin is a secondary reinforcement. If this were the case, we would expect that during the 42 days in which the animals received saccharin solutions, their intake would decrease through a process of extinction. By comparing Days 19-24 with 43-48 we see, however, that intake increases almost twofold. Also, during the last period of the experiment animals consumed approximately as much of the saccharin solution as during the first period in which solutions were presented.

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<sup>6</sup> Hausmann, *op. cit.*, 426.



# APPARATUS

## A DEVICE FOR PRESENTING KNOWLEDGE OF RESULTS AS A VARIABLE FUNCTION OF THE MAGNITUDE OF THE RESPONSE

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Almost all investigations relating learning to knowledge of results have manipulated some variant of (a) the time elapsed between the response and some knowledge of the response provided *S* by *E*, or (b) the completeness or comprehensiveness of expression of relative response proficiency.

The number of areas investigated and the status of knowledge of results as a training variable can be extended by suitable design of apparatus having the following general properties: (1) it must present a task to be mastered; (2) it must provide a measure of the response; and (3) it must require that the task-situation be such that the learning of the desired response is contingent upon the knowledge of results which the apparatus can provide at *E*'s discretion.

The information provided *S* by psychomotor apparatus is always some function of the response. One desired aspect of such apparatus is to provide the potentiality of a large number of such functions. Usually, the larger the number of functions and the greater their individual complexities, the greater is the cost of the apparatus. *E*, for the present status of research, however, is flexible enough to provide the necessary informational programming. When *E* is inserted within an information loop, knowledge of results can be considered as independent of the response.

The apparatus described here makes use of a motor response already learned when *S* begins practice. The task consists in learning by means of display signals what magnitude of the response is appropriate. The signals are, of course, monitored by *E* and are *some* specified function of the measured response.

*The manual lever.* Three different terms are frequently used in conjunction with the lever. These are: (1) true-score—*E*'s reading of the response measure; (2) re-

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\* This apparatus was devised as part of the United States Air Force Human Resources Research and Development Program, San Antonio, Texas.



ported-score—the score displayed to *S* after each response; and (3) goal-score—the numerical score which *S* is asked to achieve on each trial.

The apparatus consists of three main parts: (a) *S*'s display panel; (b) the lever; and (c) *E*'s panel. Cutaways of the device are presented in Fig. 1.

*S*'s display panel. *S* is seated in a chair before the apparatus in such a position that his right hand can be placed upon a lever grip when the arm is nearly fully extended. A vertical wooden screen prevents *S* from seeing the lever. A one-way vision window is mounted in a vertical position about 8–9 in. away from *S* at eye-level. When scores are not being reported to *S* this window is dark and nothing beyond can be seen. Whenever *E* elects to present *S* with a score, a lamp between the window and a score-scale is lighted to expose the scale and a pointer. The position of the pointer along the horizontally mounted scale represents the reported-score or the score which is given to *S*. The reported-score scale ranges from zero on the left to 100 on the right and is marked off in units-places with major divisions indicated at multiples of 10 and 5.

*The lever.* In its normal resting position the lever is nearly vertically suspended from a shaft, passing from the lever side of the apparatus to *E*'s panel.<sup>1</sup> The lever is so attached to the shaft that displacement of the lever causes the shaft to rotate. The lever is some 21 in. in length. Located along the lever is a movable grip which can be fixed at any distance from the center of lever rotation. When the lever is pulled towards the body, the lever is limited to a maximal arc of approximately 65°. Attached to the lever by a system of cables and one fixed and one movable pulley is a weight which exerts a force against lever displacement. After the lever has been displaced and released, the weight acts to return the lever abruptly to its resting position. To counteract this effect, a hydraulic system was installed to dampen the lever's return to the resting point.

This hydraulic system was necessary to prevent damaging the apparatus after *S* released the lever. The damping action acts only on the return travel of the lever after *S* has completed a response. A hydraulic cylinder was so placed that motion of the lever extended it. A one-way check valve and variable restrictor valve were so placed between the upper and lower chambers of the cylinder that the motion is free during extension and damped during the return. The adjustability of the damping force was necessary to set equal rates of return for various loads or weights on the lever.

*E*'s panel. The lever is securely fastened to the horizontal shaft running from the lever side of the apparatus to *E*'s panel. Rotation of the shaft is, of course, linearly related to the displacement of the lever. A pointer, attached to the shaft at *E*'s panel, rotates with the shaft and lever. A second pointer, or knockup, is so mounted that movement of the first pointer causes the second to move along with the first. When the lever is released the knockup remains at the point of maximal travel whereas the first pointer returns to zero, following the motion of the shaft and lever.

The entire system is linear, in that movement of the pointer along the true-score scale is directly related to displacement of the lever. True-score information is provided to *E* by the two pointers activated by displacement of the lever. The two pointers move along a true-score scale graduated into 100 units. A vernier is also

<sup>1</sup> This device was suggested by a lever task originally devised by Dr. Robert M. Gagne.



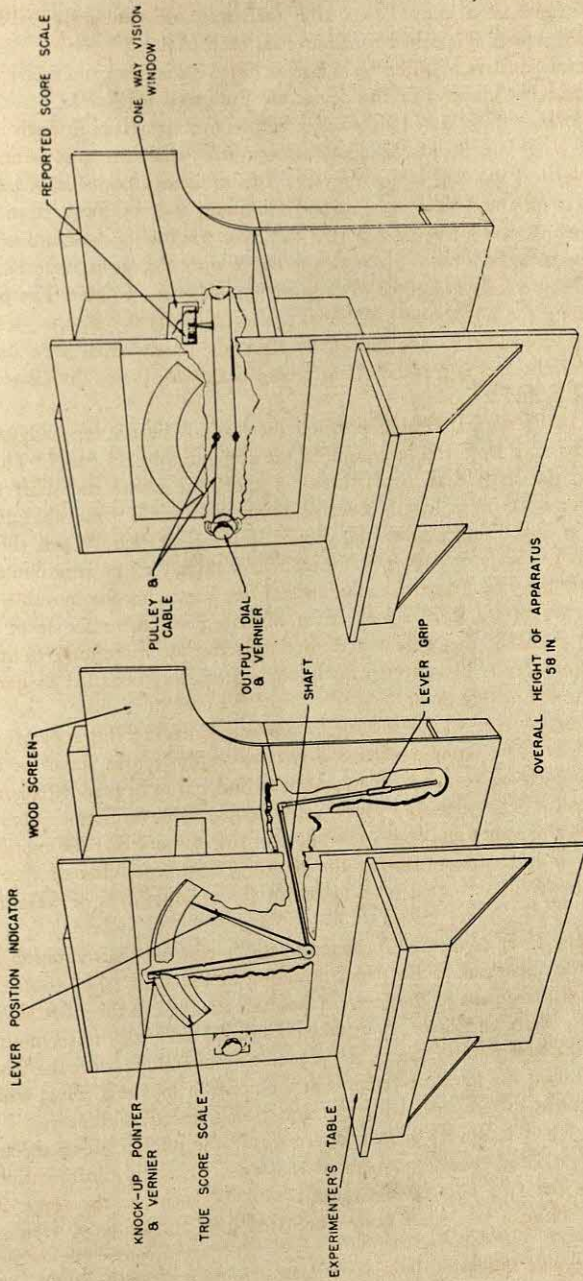


FIG. 1. TWO CUTAWAYS OF THE APPARATUS SHOWING THE MANUAL LEVER DEVICES



mounted along the knockup pointer, providing for the reading of the response measure in tenths of units. After recording the true-score, *E* performs two additional functions. The first is to transform the true-score to a reported-score and the second is to pass along the reported-score to *S*. *E* performs his first function by transforming the true-score according to some predetermined formula and the second by setting the transformed score into the output channel. Transmission of the reported-score is accomplished with a dial-scale arrangement which mechanically displaces *S*'s pointer along the reported-score scale. After the reported-score is set into the display, the display panel is lighted, exposing to *S* a score some function of the response previously made. The linkage between *E*'s output dial and *S*'s reported-score pointer is pulley and cable. *E*'s output dial is also provided with a vernier such that the reported-score can be transmitted in tenths of units.

Two clocks are provided for *E* to time durations of practice and rest or other events. At present these clocks are independent of the lever response and are not a part of the lever device.

*Calibration.* The pulley and weight system is so arranged that every pound of weight at the end of the system causes two pounds of pull at a radial distance of one foot to resist twisting of the shaft by *S*'s lever. With lever length set at 14 in. the resistance *S* must overcome is slightly more than twice the dead weight hung at the end of the cable and pulley system. A set of calibrated weights permits adjustment of the dead weight to the nearest half pound. (To a maximum of 30 lb. or over 60 lb. at *S*'s grip with the 14-in. radius.)

The resistance to lever-motion is kept constant regardless of lever-position by having the cable from the pulley and weight system wrap about an arc of constant radius (1 ft.). Space limitations of the device made the constancy of the weight's effect diminish slightly at true-score readings of 70.0 and above.

An end-stop on the lever limits the travel of the lever to approximately  $65^\circ$  (the upper limit is adjustable over a small range). With maximal lever travel 87.5 scale-units are encompassed by *E*'s pointers. True-score units of 1.34 equal  $1^\circ$  of lever-travel ( $0.746^\circ$  equals 1 true-score unit). For every scale-unit there is 0.182 in. of arc (hand) displacement at the lever-grip (with 14 in. from center of rotation to center of hand-grip).

Potential accuracy of measuring the position of the lever is consequently rather precise—the limit is  $0.075^\circ$ . The apparatus is rugged and, once calibrated, reliabilities need not be a source of constant anxiety. The emphasis on direct mechanical drive appears to be the simplifying feature.

*Physical variables.* A number of physical variables can be readily introduced for study. Some of these are: (1) Length of lever arm (displacement angle constant). (2) Displacement angle required (length of lever arm constant). (3) Force required to overcome the lever system: (a) linear over lever displacement; and (b) proportional to lever displacement, which may be accomplished by: (i) substitution for the weights of a carefully wound spring with the desired characteristics, and (ii) substitution for the constant radius arc of a linkage which would decrease the moment arm as the lever was angularly displaced.

*Psychological variables.* For one set of experiments *S* may be instructed as follows: "Your task on each of a number of trials will be to obtain a score of 45 (goal-score) in this window. The further you pull this lever the higher your score



as shown in the window after you finish your pull." A sample of possible learning investigations illustrating the versatility of the lever system would be to vary: (1) Target size— $E$  reports 45.0 for all settings within specified tolerances; (2) Variable error of information—the several treatments are distinguished by the magnitude and frequency of random error added to the true-score; (3) Constant error of information added periodically—on alternate blocks of trials an error constant is added to the true-score and  $S$  is required to learn to shift the magnitude of responses in order to hold constant the goal of 45; and (4) Rate of change in reported-score as a function of response magnitude—groups are distinguished on the basis of the kind of function to be investigated, *i.e.* linear and curvilinear, and subgroups are defined by the magnitude of the parameters of the equations.



## NOTES AND DISCUSSIONS

### APPARENT SIZE IN STEREOSCOPIC MOVIES

Psychologists and others interested in the problem of visual perception must think as Southall did in the early twenties when he remarked that "Stereoscopic pictures recently exhibited . . . excited much interest and curiosity, as though they were something very novel and marvelous."<sup>1</sup> As everyone knows, the movie industry at present is very active in its efforts to get the third dimension into a medium which has been 'flat' so long. In the process of doing this the public's curiosity is again aroused by a 'new twist' in a popular form of entertainment. Lest the 'newness' be taken seriously, it should be known that stereoscopic movies are not a recent accomplishment at all. They were achieved in the laboratory by Pi Suñer in 1914, and were exhibited to an audience in New York City as early as the winter of 1922.<sup>2</sup> The techniques employed were cumbersome but they achieved the desired effect.

The principle involved in stereoscopic movies is well-known; yet many visual phenomena related to them are not clearly understood.<sup>3</sup> The purpose of this note is to discuss the reduction of apparent size induced by viewing such movies.

When one of these modern three-dimensional movies was seen by the writer,<sup>4</sup> he noticed that the picture and the actors in it were smaller than in the usual two-dimensional movies. By repeatedly removing and replacing the polaroid glasses, the reduction in apparent size could be observed easily.

By a happy coincidence students in the introductory course in psychology were studying problems in visual perception when the movie was shown at the local theater. This provided an opportunity to obtain information

<sup>1</sup> J. J. C. Southall, Editor, English trans., Helmholtz, *Physiological Optics*, 3, 1923, 356, footnote 2.

<sup>2</sup> August Pi Suñer, *El relleu cinematografic, Treballs de la soc. de Biologia de Barcelona*, 2, 1914 1ff.; Southall *op. cit.*, 358, footnote 1. Cf. also Pi Suñer, The third dimension in the projection of motion pictures, this JOURNAL, 60, 1947, 116-118.

<sup>3</sup> Reference here is to those movies which present disparate images, one to each eye, by means of some type of selective optical device.

<sup>4</sup> "B'Wana Devil," produced by Arch Oboler and distributed by United Artists. Its technique involves the polarization of light.



from the students about the size-effect and other related aspects. The following questionnaire, therefore, was prepared and answered by 55 members of the class.<sup>5</sup>

## QUESTIONNAIRE

- (1) Did you observe the intended depth-effect in the movie? (All replied 'Yes.')
- (2) As you observed the movie did the size of the picture seem smaller, larger, or no different in comparison to the size of the picture in the usual two-dimensional movie?
- (3) In general, did the over-all size of the people in the movie seem larger, smaller, or no different in comparison to the size of people in the usual two-dimensional movie?
- (4) During the movie did you at any time have any of the following experiences? Check the appropriate item or items. (Five checked none; 50 checked one or more.)
 

(a) Watering of the eyes (14 checks) (b) Double vision (31) (c) Pain in the back of the head (2) (d) Strain in and around the area of the eyes (43) (e) Itching of the eyes (4) (f) General irritation of the eyes (13)	(g) Difficulty of keeping the picture in focus (32) (h) General nausea (2) (i) General headache (11) (j) Localized headache (2) (k) Dizziness (3)
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- (5) After leaving the movie, did you experience any after-effects of any kind? If so, describe them carefully and state how long such effects lasted.
- (6) Describe any particular circumstances in the movie which seemed to induce unpleasant or confusing effects. Try to be as specific as possible.
- (7) Assuming such things as acting ability, plot, etc., equal, which kind of movie do you prefer? (Six had no preference, 32 preferred two-dimensional, and 17 tri-dimensional.)

Please add any comments below which might supplement and clarify the answers given to the questions.

To the second question, 24 students answered 'smaller,' 29 'no different' and 2 'larger.' To the third question, concerning the size of the actors, 31 answered 'smaller,' 22 'no different' and 2 'larger.' It is the writer's belief that the proportion of 'smaller' answers would have been even greater had the questions been asked as the movie was being observed. In any event, if one grants there is a tendency to see stereoscopic movies as reduced in size, then the question arises: What is the explanation of this effect?

The effect is not due to any difference in actual sizes of projected images in the two types of movies. These magnitudes were equal at least for this

<sup>5</sup> Most students filled out the questionnaire one or two days after seeing the movie. In a few cases the interval was longer, five or six days. The number of students responding to the various alternatives are given in the appropriate places in the questionnaire.



theater. It is not, furthermore, explained by the alteration of the values of apparent depth as a function of binocular disparity alone. Such an explanation is relevant only to the apparent sizes of objects in the picture *relative to one another*.<sup>6</sup>

A possible explanation involves the factors of anomalous convergence and accommodation. It is not a new observation, of course, that distortions of size are frequently observed when objects are viewed through stereoscopic instruments. When the separation of the lenses exceeds normal interpupillary distance, a 'model' of reduced size is often observed. The cause of this reduction has never been clearly explained, certainly not with regard to stereoscopic movies. Consider for a moment the viewing conditions for the movie. The disparities presented in many cases (particularly close-ups) exceed those experienced in normal vision. Moreover, and this is important, the usual invariable relation between magnitude of disparity and amount of accommodation and convergence is seriously violated due to the distance of the screen. Disparities of the order of magnitude present in the movie would never be experienced in normal vision, say, at a viewing distance of 100 or 200 ft. More likely the appropriate distance would be 10 to 20 ft. at the most. Under such viewing conditions it seems reasonable to expect changes of convergence and accommodation which are correlated with a viewing distance much smaller than the actual distance, viz., the distance of the screen.<sup>7</sup> From what is known of the effect of accommodation and convergence upon apparent size and in so far as the induced changes are effective in this situation a reduction in apparent size is to be expected. This expectation is confirmed by the responses of roughly half of the students. The reduction in apparent distance can only be inferred from the responses to various 'dramatic' incidents in the movie, such as 'ducking' when a spear is thrown in the direction of the audience.

The situation described is a good example of conflict in visual perception. The viewing distance calls for appropriate adjustments of accommodation and convergence. The disparity factor calls for adjustment of the accommodation convergence mechanism which is related to a much nearer viewing distance. In the presence of such conflicting indications one should expect visual symptoms to be reported. They were. Three of the 11 symptoms listed in Question 4 were checked with a disproportionately high

<sup>6</sup> See H. A. Carr, *An Introduction to Space Perception*, 1935, 363-365.

<sup>7</sup> One is reminded here of paresis micropsia, a disorder of the convergence mechanism wherein the effort of convergence is considerably out of proportion to the actual convergence obtained. In such cases, the apparent sizes of objects are much reduced.



frequency. Forty-three of the 55 students reported the experience of strain in and around the eyes, 32 reported difficulty keeping the picture in focus, and 31 reported recurring double images. Such symptoms are precisely those which should be related to anomalous convergence and accommodation. While their occurrence is only suggestive of the validity of the explanation ventured above, the frequency with which these symptoms were reported definitely refutes the claim made by the producers; namely, that the viewing of the movie is not in the least harmful but, on the contrary, enables the viewer to leave the theater with eyes rested and refreshed!

The size-effect discussed above raises a paradoxical problem with respect to three-dimensional movies in general. Today there are basically two types in use: one type can be classed as 'illusory'; the depth effect obtained is dependent, not upon stereoscopic vision, but in part upon an increase in the size of the projected picture. An example of this type is commercially known as 'Cinerama.' The other type, with which this note is concerned, involves true stereoscopic vision. The paradox is that the former type is in part dependent upon an increase in size of the perceptual field for its depth effect, while the latter, in obtaining this effect, *reduces* perceptual size, the one factor upon which the 'illusory' type of movie depends. Both types of movies achieve visual depth. Which one does so more successfully is debatable. In view of the considerations discussed here, certain objectionable features can be ascribed to true stereoscopic movies. This may be the basis for the fact that in response to Question 7, 32 of the 55 students stated that they preferred ordinary two-dimensional movies. Seventeen preferred the three-dimensional kind and six expressed no preference.

It will be interesting to follow the commercial development of these modern types of three-dimensional movies. The visual phenomena and problems related to them should be of particular concern to the psychologist interested in visual perception.

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### ON THE EFFECT OF AN IRRELEVANT RELATION

In a recent issue of this JOURNAL, Elam and Bitterman described a series of experiments on the effect of an irrelevant relation in discriminative learning.<sup>1</sup> Two groups of rats were trained on a thickness-discrimina-

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<sup>1</sup> C. B. Elam and M. E. Bitterman, The effect of an irrelevant relation on discriminative learning, this JOURNAL, 66, 1953.



tion with horizontally and vertically striped cards. For one group the stripes on each pair of cards differed only in thickness, while for the second group each pair differed both in thickness (relevant) and direction (irrelevant) of stripes. The performance of the two groups did not differ significantly. Given this set for thickness, however, subsequent learning of the directional discrimination was retarded by the presence of the thickness-relation. Elam and Bitterman suggested that "retardation in the first part of such an experiment" (prior to the experimental establishment of set) "could be demonstrated by the selection of a more striking irrelevant relationship." The results of the experiment to be reported confirm this prediction.

Twenty-four naïve Albino rats, ranging in age from 3 to 4 mo., were studied in the jumping apparatus of the earlier study, now painted mid-gray. Training procedures were identical with those used by Elam and Bitterman, but different stimulus-cards were employed. The relevant relation was form (4-in. equilateral triangles, upright vs. inverted) and the irrelevant relation was brightness (black vs. white). The forms were

TABLE I  
PERFORMANCE ON THE COMBINED PROBLEMS

Group	Initial errors	Total errors	Days
I	42.1	63.9	20.8
II	63.1	99.5	31.4
Diff.	21.0*	35.6*	10.6*

\* Significant beyond the 5% level of confidence by Wilcoxon's test for paired replicates.

painted on mid-gray grounds which were the same color as the rest of the apparatus. The animals were divided into two equal groups which were given 8 trials per day by the correctional method. Half of each group was reinforced on upright and half on inverted triangles. For Group I the two members of each pair of cards differed only in form, while for Group II the cards differed both in form and in brightness of triangles. The criterion of learning was two errorless days, and as each animal mastered its problem it was trained to the same criterion on the problem of the other group.

The course of learning is plotted in Fig. 1 in terms of the cumulative number of animals in each group to reach criterion on both problems. In Table I the performance of the two groups on the two problems combined is summarized in terms of mean initial errors, total errors, and days to criterion. The performance of Group I, evaluated by Wilcoxon's non-



parametric method for unpaired replicates,<sup>2</sup> was in all respects superior to that of Group II beyond the 5-% level of confidence. This significant order-effect is the same as that obtained in the second part of each of the Elam-Bitterman experiments (following the establishment of a set for the irrelevant relation in the first part of each experiment). That the results of the present study cannot be attributed to a preëxisting brightness

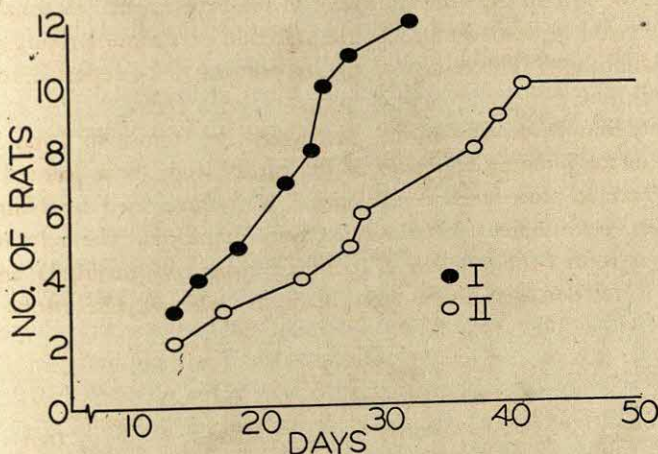


FIG. 1. THE COURSE OF LEARNING ON THE COMBINED PROBLEMS. Cumulative number of animals reaching the criterion is plotted as a function of number of days of training.

preference is demonstrated by a tabulation of the responses of the animals of Group II on the first day of training. The group as a whole averaged 4.16 responses to the white card, and only two of the animals made as many as 6 of the 8 initial responses to either of the cards. These results are taken to mean that discriminative learning may be retarded by the relational presentation of an irrelevant variable in the absence of a pre-established set for the irrelevant relation. The high positive transfer from the problem of Group I to that of Group II (the animals of Group I averaged only 4.7 errors on the second problem) suggests that the effectiveness of the irrelevant relation is largely eliminated when the relevant relation has been selected.

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<sup>2</sup> Frank Wilcoxon, *Some rapid approximate statistical procedures*, American Cyanamid Co., Stamford, Conn., 1949, 1-16.



CONTINUOUS STIMULATION AND  
APPARENT MOVEMENT

In a recent communication to this JOURNAL, Deatherage and Bitterman reported that continuous visual stimulation in the path of movement altered the threshold and the nature of the apparent movement perceived.<sup>1</sup> They cited this experiment as evidenced for the short-circuiting theory of stroboscopic movement (Wertheimer), and for the satiation hypothesis of Köhler and Wallach.<sup>2</sup> Similar results had been obtained in an independent study by the present author, but a further investigation of the effects of continuous stimulation demanded modifications of the explanation offered by Deatherage and Bitterman. The purpose of this communication is to report briefly the results of this investigation.

Fifteen normal young adults of high average intelligence were presented with apparent-movement stimuli using an inverted V-figure exposed in a viewing box. Exposure-time was 75 m.sec., and the time-intervals between alternate exposures were equal. The time-interval was varied continuously from 24 to 600 m.sec. Viewing was binocular and no instructions regarding movement were given. The Ss were retested half an hour after the initial test. Four types of continuous stimulation were given each S at half-hourly intervals. These were: (a) visual—fixating a circle of light in the viewing box; (b) auditory—an electric buzzer; (c) voluntary movement—pacing up and down the room; and (d) simple mental arithmetic. Each form of continuous stimulation was given for 2 min. and the order varied for different Ss. Retests were given immediately prior to and after continuous stimulation, and a final retest was given half an hour after the last test. The thresholds for simultaneity and succession were recorded and the difference between these thresholds represents the range of time-intervals over which apparent movement was perceived.

The increments and decrements in the range after each test were calculated for every S and the mean changes are shown in Table I. The decrement in the range on the first retest is insignificant. Each type of continuous stimulation resulted in a highly significant decrease in the range of movement which returned to normal during the half-hour rest-period. Every S showed this decrease, and the order of presentation did not affect the results. In all cases, the decrease in the range is a result of simultaneity being perceived at longer, and succession at shorter time-intervals. Some Ss reported that simultaneity changed directly to succession and no intervening

<sup>1</sup> B. H. Deatherage and M. E. Bitterman, The effect of satiation on stroboscopic movement, this JOURNAL, 65, 1952, 108-109.

<sup>2</sup> Wolfgang Köhler and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. Phil. Soc.*, 88, 1944, 269-357.



movement could be perceived. This, as well as alteration in the path and pattern of movement occurred regardless of the type of continuous stimulation employed.

Since all forms of continuous stimulation have an identical effect on the perception of apparent movement, an explanation which depends on a direct isomorphic relation between perception and cortical events is not tenable. Thus, contrary to the conclusions of Deatherage and Bitterman, the satiation hypothesis cannot be used as an explanatory model, since

TABLE I  
MEAN RANGE (R) ON INITIAL TEST AND INCREMENT OR DECREMENT ( $\Delta R$ ) ON FIRST  
RETEST AND ON RETESTS AFTER CONTINUOUS STIMULATION  
(I=immediately after continuous stimulation; H=half an  
hour afterward. Time in m.sec.)

R	1st retest	$\Delta R$							
		Visual		Audition		Movement		Mental	
		I	H	I	H	I	H	I	H
147	-14.4	-68.0	+51.1	-56.1	+57.4	-65.5	+55.8	-56.4	+58.0

satiation depends on a precise spatially localized effect and is produced only by objects and patterns.<sup>3</sup>

It is not possible at the present moment to characterize precisely the neural processes underlying the effects of continuous stimulation on apparent movement. It may be concluded, however, that this process must be localized in a part of the visual system which can be affected by stimuli from all sense modalities and by problem solving activity. It is reasonable to postulate that the preoccipital area (Brodmann area 19) is the region responsible, since this part of the cortex is concerned with visual elaboration and receives impulses from the entire cortex.<sup>4</sup> This hypothesis, at the same time, provides a basis for explaining the perception of apparent movement between two points which are represented in opposite striate cortices,<sup>5</sup> as well as the observation of transmodal phi.<sup>6</sup>

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<sup>3</sup> Köhler, Relational determination in perception, in cerebral mechanisms in behaviour, *The Hixon Symposium*, Ed. by L. A. Jeffress, 1951, 200-230.

<sup>4</sup> Gerhardt von Bonin, H. W. Garol and W. S. McCulloch, The functional organization of the occipital lobe, *Biol. Symposia*, 7, 1942, 165-192.

<sup>5</sup> K. R. Smith, Visual apparent movement in the absence of neural interaction, this *JOURNAL*, 61, 1948, 73-78.

<sup>6</sup> A. Galli, On the perception of apparent movement produced by various sensory stimuli, *Pubbl. Univ. Cattol. S. Cuore*, 5, 1931, 79-122. (*Psychol. Abstr.*, 6, 1932, 571.)



## THE CRITICAL FREQUENCY FOR TASTE

To substantiate a theory of sensory refractoriness, Allen and Weinberg, in 1925, determined fusion-frequencies for periodic electrical excitation of taste.<sup>1</sup> Fusion-frequencies (200–650 cps.) were found to vary with potential (0.2–0.7 v.). A family of four functions was generated, each of which was identified with corresponding tastes of sour, salt, sweet, and bitter according to shifts or disappearance when gymnemic acid and other adapting substances were applied to the tongue. The data were found to fit a Fechnerian equation analogous in form to those for hearing, vision, and touch. The experiment was carried out with only one *O*, and the four curves they obtained were averages of 400 or more observations. No measures of variability were reported. It is, consequently, difficult to interpret their results in terms of reliability or statistical significance.

Bujas and Chweitzer, using direct current as the stimulus, reported that the durations required for arousal of taste sensations varied inversely with voltage, the shortest time reported being 27 m.sec. (the period which a repetitive stimulus of only 19 cps. would have).<sup>2</sup> In a later study, using alternating current, these authors found that the quality and threshold of taste varied with frequency.<sup>3</sup> The only mention of temporally varying sensations was to the effect that a high intensities tactile and vibratory sensations obscured taste.

Allen and Weinberg's study was recently repeated by Jones and Jones.<sup>4</sup> An electronic square-wave generator, rather than the original commutator interrupter, was used with six *O*s. Their results were completely negative. No taste sensations could be aroused by the voltages at which Allen and Weinberg plotted their functions. There was no evidence of cyclical taste sensations at increased voltages.

Allen and Weinberg's results seemed to us to be too precise to be incapable of substantiation, hence we decided to repeat the experiment. A laboratory signal generator was used to drive a double-diode clipper followed by a clamper. The output thus consisted of positive square-waves variable from 0 to 3.6 v. The electrodes were of low galvanic action dental wire spaced about  $\frac{1}{4}$  in. apart.

<sup>1</sup> F. Allen and M. Weinberg, The gustatory sensory reflex, *Quart. J. Exper. Physiol.*, 15, 1925, 385-420.

<sup>2</sup> Z. Bujas and A. Chweitzer, Contribution à l'étude de goût dit électrique, *Année Psychol.*, 35, 1934, 147-157.

<sup>3</sup> Bujas and Chweitzer, "Goût électrique" par courants alternatifs chez l'homme, *C. R. Soc. Biol., Paris*, 126, 1937, 1106-1109.

<sup>4</sup> M. H. Jones and F. N. Jones, The critical frequency of taste, *Science*, 115, 1952, 355.



Five graduate students in psychology served as the *Os*. None of them spontaneously reported discrete sensations of taste for frequencies varying from 20 to 2000 cps. and none was able to report the appearance of such sensations, even though set by instructions to attend to them.

Little or no sensation was elicited at 1 v. and below. Varying complex sensations of cold, sour, and bitter were, however, produced at higher voltages. With excitations exceeding 3 v., pronounced tactual-kinesthetic vibratory sensations were evoked which seemed to bear no relation to the excitation-frequency. As some critical frequency was approached, this sensation rapidly diminished and disappeared. This fusion occurred between 500 and 1700 cps. and seemed to be related to the pressure of the electrodes against the tongue and the area stimulated. The higher frequencies were associated with the lightest pressure.

The negative results of Jones and Jones and of the present study, and the contra-indications in Bujas and Chweitzer's data, set the results and conclusions of Allen and Weinberg's study in question. It remains to be seen whether highly practiced *Os* can detect periodic taste sensations, and whether pressure and area of contact are significant variables. Fusion-frequencies are, of course, dependent on the ability to experience temporally discrete sensations, and these must be extremely subtle, if at all functional, in the sense of taste.

University of Maryland

SHERMAN ROSS

JOHN VERSACE

#### FORTY-NINTH MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The forty-ninth annual meeting of the Society of Experimental Psychologists was held at the University of Texas on March 30-31, 1953, in conjunction with the dedication of Mezes Hall, Texas' new psychological laboratory. The Chairman of the Society for the year, Dr. Harry Helson, presided at the business meeting held on the morning of March 30 and at the subsequent scientific sessions.

The following attended the meetings: Boring, Bray, Brogden, Warner Brown, Dallenbach, Dashiell, Geldard, Grant, Guilford, Harlow, Helson, Hilgard, Hunt, Kappauf, Kennedy, Landis, Melton, Neal Miller, Mueller, Muenzinger, Nafe, Neff, Spence, Underwood, Weld.

Three scientific sessions were held at which members reported informally upon research recently completed or in progress: the afternoon of March 30, and the morning and afternoon of March 31.



Following the dinner on March 30, the Warren Medal for 1953 was awarded to Dr. Kenneth W. Spence "For his persistent and rigorous theoretical and experimental work on fundamental problems of learning."

The 1954 meeting, the Golden Jubilee of the founding of the Society, will be held next spring at Cornell University.

University of Wisconsin

W. J. BROGDEN

#### FORTY-FIFTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The forty-fifth annual meeting of the Southern Society for Philosophy and Psychology was held in Austin, Texas, April 2-4, 1953, in conjunction with the dedication of Mezes Hall, the new psychological laboratory of the University of Texas, the host institution. All of the meetings, with the exception of the annual banquet, were held in Mezes Hall. Approximately 150 members and visitors were present. Sixteen papers in philosophy and 32 papers in psychology were presented. At the joint session a symposium was held on "Contributions to operations research." Gerard Hinrichs and Meredith Crawford were the principal speakers, representing philosophy and psychology respectively.

Following the annual banquet, Willis Moore delivered the presidential address entitled "The nature and causal antecedents of the current attack on education."

At the business meeting, one member was transferred from the status of associate to full membership, and 9 new associate and 19 new members were elected to the Society. The new officers elected were: Karl M. Dallenbach, President; Charles A. Baylis, Council Member for Philosophy; and M. C. Langhorne, Council Member for Psychology. William M. Hinton and Oliver L. Lacey continue as Treasurer and Secretary respectively.

The 1954 annual meeting will be held at Atlanta, Georgia, under the joint auspices of the University of Georgia and Emory University during the week end of the Easter vacation.

University of Alabama

OLIVER L. LACEY

#### TWENTY-FIFTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The Midwestern Psychological Association met at the Drake Hotel, Chicago, on May 1 and 2, 1953. Northwestern University and the University of Chicago were host institutions. George S. Speer, of the Illinois



Institute for Technology, served as chairman for local arrangements. A total of 1940 members and guests registered during the meeting, and about 300 additional psychologists were present. The program consisted of 173 papers and 5 symposia, emphasis being on learning, clinical psychology, and projective techniques, and experimental studies in personality and social psychology. The symposia dealt with: teaching clinical psychology, research on psychotherapy, union-management relations, learning and behavior pathology, and the psychocultural influence of television. The presidential address, "Discussion of sequences in stimulus-events and the transmission of information," was given by David A. Grant, of the University of Wisconsin.

At the annual business meeting, it was announced that Judson S. Brown, of the University of Iowa, had been elected president for 1953-54, and that Benton J. Underwood, Northwestern University, had been elected to the executive council for a three-year term. Ninety-one new members were elected, bringing the total membership to 1301. The 1954 meeting will be held on April 29, 30 and May 1, at the Neil House and Deshler-Walleck Hotels, Columbus, Ohio, under the sponsorship of Ohio State University.

University of Illinois

LEE J. CRONBACH

## TWENTY-FOURTH ANNUAL MEETING OF THE EASTERN PSYCHOLOGICAL ASSOCIATION

The Eastern Psychological Association met on April 24 and 25 at the Hotel Statler in Boston, Massachusetts. A total of 1274 persons registered at the meetings. Of these 631 were members of the Association, 402 were guests, and 241 were new members who joined the Association at the meeting. The present active membership of the Association totals 2251.

The program consisted of 151 scientific papers (presented in 20 sessions), 4 symposia, and 5 special meetings. The 20 sessions were concerned with the following topics: clinical psychology (3 sessions), evaluation and measurement (2 sessions), perception and personality, human learning, physiological psychology, social psychology, childhood and adolescence, animal behavior perception, vision, personality and social, general experimental, conditioning, animal learning, personality evaluation, perceptual-motor skills, and audition. The symposia were as follows: "The application of animal studies on acquired drive to problems in human motivation," "The meaning of psychological health," "The non-projective



aspects of the Rorschach experiment," and "The prognostic use of personality tests." Special meetings included a meeting of Psi Chi, The Committee on Sub-Doctoral Education, and meetings bearing the titles "The problems of private practice," "The relevance of social research for war prevention," and "Proposals for the Institute on Medical Psychology."

Dr. Neal E. Miller presented the annual presidential address, "A theoretical and experimental analysis of conflict behavior." New officers elected were Harold Schlosberg, President; Norman O. Frederiksen, Treasurer; Neal E. Miller and Charles N. Cofer, Board of Directors.

The 1954 meetings of the Association will be held at the Hotel New Yorker in New York City on April 9 and 10.

University of Delaware

GORHAM LANE

### TWENTY-THIRD ANNUAL MEETING OF THE ROCKY MOUNTAIN BRANCH OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The Rocky Mountain Branch of the American Psychological Association held its twenty-third annual meeting on April 3 and 4, 1953, at the University of New Mexico, Albuquerque, New Mexico. Over 100 members and guests attended.

There were 13 scientific papers read and a symposium was held on "Reinforcement theory." H. B. McFadden of the University of Wyoming chaired the symposium. A regional meeting and luncheon was held by Psi Chi. There was also a meeting to discuss problems in practicum training held under the auspices of the Education and Training Board, Committee on Practicum Training, and Division 12. Another special meeting was held at the request of the Committee on Subdoctoral Training to discuss the report of that Committee.

Ralph Norman, President, served as Chairman of the business meeting. The place for the next annual meeting has not been decided. Serious consideration will be given to breaking away from the Colorado-Wyoming Academy of Science. A meeting in the near future may be planned in the State of Utah. The following officers were elected for the ensuing year: President, H. B. McFadden, University of Wyoming; President-Elect, Lawrence S. Rogers, Veterans Administration, Denver; and Secretary, Margaret Thaler, Mental Hygiene Clinic, Colorado General Hospital, Denver, Colorado. Virginia M. Brown, Lowry Air Force Base, Denver, continues to serve as Treasurer.

Denver, Colorado

LAWRENCE S. ROGERS



THE 1953 MEETING OF THE NATIONAL  
ACADEMY OF SCIENCES

The annual meeting of the National Academy of Sciences was held in Washington on April 27-29, 1953. Of the 23 members of the Section of Psychology, 12 were present—Beach, Boring, Carmichael, Harlow, Hilgard, Hunter, Lindsley, Miles, Pillsbury, Richter, Stevens, and Yerkes. Henry W. Nissen was elected to membership.

Of the eight papers read at the Monday afternoon session, six were by psychologists or psychophysicists. It was the strongest program in experimental psychology and psychophysiology that has ever been presented to the Academy. These were the papers:

C. F. Richter, "Behavior cycles in man and animals," dealt with activity-apathy cycles (2-40 days) in patients, and with cycles in rats induced by endocrine insult and by stress.

F. A. Beach and Jerry Kagen, "Effects of early experience on mating behavior in male rats," showed that the induction of early mating behavior in rats tends to continue the behavior fixed in immature form throughout life, whereas, when initial sexual experience comes at a later period, it tends to persist in a mature pattern.

H. F. Harlow, "Learning by Rhesus monkeys on the basis of manipulation-exploration motives," showed that the opportunity to manipulate puzzle devices and also to get a view of the outside world by getting a window open in the confinement box acted directly as reinforcements.

Carl Pfaffmann, "Species differences in taste sensitivity," reported differences in sensitivity of the rat, the rabbit and the cat to quinine, hydrochloric acid, sucrose, sodium chloride and potassium chloride (the last two not always the same), measuring the response electrically from the chorda tympani dissected out in the anesthetized animal.

H. K. Hartline, E. F. MacNichol, Jr., and H. G. Wagner, "Electrical responses to illumination of isolated receptor elements," described two types of electrical response to photic stimulus of the ommatidium of *Limulus*, obtained by the insertion of micropipette electrodes into the sensory cells.

D. B. Lindsley, "Effect of photic stimulation in visual pathways from retina to cortex," described the effects of varying photic stimulation in respect of five parameters upon excitation at seven different points in the optic tract and projection system.

Harvard University

EDWIN G. BORING



THE 1953 MEETING OF THE AMERICAN  
PHILOSOPHICAL SOCIETY

The American Philosophical Society held its annual meeting in Philadelphia on April 23-25, 1953. Of the psychologists, Boring, Carmichael, Hunter, Miles, and Yerkes were present. Lewis M. Terman was elected a member. There were no papers of particular psychological import presented.

Harvard University

EDWIN G. BORING

## ERRATUM

Dr. Sidney M. Newhall reports that the change in the lamp-voltage given in his study in the January 1953 issue of this JOURNAL (Vol. 66, page 137) should be '3.0 v.' instead of '0.26 v.'



## BOOK REVIEWS

Edited by M. E. BITTERMAN, University of Texas

*Über Aufbau und Wandlungen der Wahrnehmungswelt.* By IVO KOHLER. Wien, Rudolf M. Rohrer, 1951, Pp. 118.

The monograph reports several experiments on the effect of the prolonged wearing of lenses which produce some systematic alteration in the character of retinal stimulation. There is a brief account of an experiment on the effects of inverting the retinal image, but most of the report is devoted to the effects of viewing the world for long periods of time through prisms and colored glasses. Kohler's principal interest is in sensory "adaptation" to the changes produced by these glasses and subsequent "negative after-effects." Most of the evidence discussed consists of qualitative observations taken from daily records kept by the *O*s, though the results of some measurements are presented in graphical form.

In the initial experiments *O*s wore prisms, usually with base vertical, in front of both eyes for periods ranging from 5 to 124 days. The principal finding was that during this period all the changes from the normal appearance of the visual world produced by the prisms either were reduced in extent or disappeared altogether. Upon removal of the glasses there was present a kind of negative copy of all the changes that had been present initially: vertical lines now had a curvature opposite to that previously imposed by the prisms; objects now appeared expanded on that side of the visual field where previously they had appeared shrunken; head movements now resulted in apparent visual motion opposite in direction to that noticed when the prisms were worn; blue-green bands of color appeared along contours and edges where previously there had been red-yellow half-spectra; and so forth.

Although many of these observations are by no means new, details are added by Kohler that cannot be discovered in the earlier literature on this subject. By far the most interesting of his new observations concern what he calls the "situational after-effects" (*Situations Nacheffekte*)—when the lenses were worn for very long periods of time (of the order of a month or more) the after-effects found upon removal of the lenses varied systematically with the position of the eyes in the head. The distinguishing feature of these after-effects is their dependence upon eye-position. The effects obtained with a given test-pattern are also, of course, reported to vary with the region of the retina which is stimulated. Situational after-effects were first reported by one *O* who wore the prisms for 124 days, but most of the evidence is drawn from two further experiments. In the first of these the *O*s wore "half-prism glasses," *i.e.* prisms attached in front of each eye in such a fashion that with the eyes lowered *O* had a normal, free view of the world, but with the eyes raised he looked through the prisms. These glasses were worn by one *O* for a period of 50 days. Daily measurements during this period showed that after approximately the first three days the after-effects were always larger when the target was regarded with eyes raised than when it was regarded with eyes lowered. Kohler emphasizes repeatedly that in this situation the only variable was the position of the eyes in



the head, and in particular that the position of the image of the test-figure on the retina was always the same. His description of his procedure reveals, however, that surprisingly little attention was paid to the problem of assuring accurate fixation. Apparently no fixation marks were used, although the test-targets were quite large. When the after-effect of curvature was being measured, for example, the lines whose curvature was to be judged were 150 cm. long. The *O* 'looked at' these curved lines from a distance of 150 cm., either with eyes raised or with eyes lowered. No evidence is presented that the *O*s fixated the same point on the curved lines, or even approximately the same point, under the two conditions. The same considerations apply to all other observations of this sort. Thus the possibility is not excluded that the position of the eyes affected the after-effects only indirectly, by altering the position of the image on the retina.

In the second experiment *O*s wore two-colored glasses. The glass in front of each eye was divided into two half-fields. The left was made of 'blue' glass, the right half of 'yellow' glass. Thus the foveal region received light transmitted by the 'blue' glass when the eyes were turned to the left, light transmitted by the 'yellow' glass when the eyes were turned to the right. The more peripheral parts of the retina were exposed, of course, predominantly to one of the two colors. For approximately the first ten days, the *O*s noted (upon temporary removal of the glasses) only ordinary, although unusually persistent, negative after-images. After this period there again developed a situational after-effect. The *O*s now reported that the negative after-image was no longer carried along as the eyes were moved, but that objects in central vision looked yellowish when the eyes were turned to the left, bluish when the eyes were turned to the right. When *O* looked straight ahead the border between the yellowish and bluish halves of the field was sometimes visible, but it was not carried along when the eyes were moved to one side or the other. This particular observation clearly cannot be explained in terms of inaccurate fixation. Kohler also reports an increased sensitivity of the foveal region to yellow when the eyes were turned to the left, and an increased sensitivity to blue when the eyes were turned to the right. The quantitative results are not, however, presented. Again, the measurements were apparently made without the use of a fixation mark or other device to ensure accurate fixation.

Kohler speaks of these effects also as "conditioned perceptions." It is his view that if a given region of the retina is stimulated in the same way whenever the eyes have assumed a particular position in the head, but is not so stimulated when the eyes have any other position in the head, then manifestations of adaptation to this kind of stimulation will eventually appear only when the eyes again assume the position in question. Furthermore, he thinks it plausible that if any other stimulus-conditions consistently coincide with a given sort of visual stimulation, then the appropriate symptoms of visual adaptation will appear only when all of these stimulus-conditions are reproduced. In support of the latter statement he points to such facts as the reported enhancement of after-effects as a result of wearing the empty frames of the glasses worn during adaptation. The fact that in relatively short periods of "adaptation" no situational after-effects appear, he explains by assuming that the 'relevant' stimulus conditions have not accompanied the particular type of visual stimulation with sufficiently greater frequency than have many other 'accidental' stimulus-conditions.



The evidence presented in this monograph, as Kohler is aware, is far too scanty to support the broad generalization on conditioned perception. The validity of the more limited statement referring to the rôle of eye-position depends on how adequately the locus of retinal stimulation was controlled, and the information that Kohler gives on this point is far from conclusive. It seems likely that Kohler's work will lead to a great deal of further research in this area. The potential significance of the observations concerning the situational effects is immense. If these observations can be confirmed virtually every current theory of visual processes will require profound revision.

Harvard University

ERIC G. HEINEMANN

*Organization and Pathology of Thought.* By DAVID RAPAPORT. New York, Columbia University Press, 1951. Pp. xviii, 786.

Included in this source book are 27 selections which, in Rapaport's opinion, constitute significant contributions to the organization and pathology of thinking. Significance was evaluated in terms of a psychoanalytic orientation, according to which thinking is an internalized experimental action that develops when direct expression of certain impulses is blocked. This conception is broad enough to embrace the varied phenomena of perception, memory, fantasy, creative thinking, and organic pathology, all of which are topics of discussion in this volume, and their integration within a single system is attempted in a final chapter. Three other criteria guided the choice of papers. The first was unavailability of the publication to the average American reader either because it had originally appeared in a foreign language or in a journal not usually read by psychologists. Most of the selections are foreign papers which have been translated by Rapaport. A second criterion was historical importance, and a third was coverage of the primary issues in the field of thinking.

The readings are divided into five sections: directed thinking, symbolism, motivation, fantasy, and pathology. There are papers by Ach, Buehler, Claparede, Lewin, Piaget, Silberer, Betleheim and Hartman, Stekel, Freud, Fenichel, Blueler, Kris, Varendonck, Schilder, Buerger-Prinz and Kaila. An examination of Rapaport's selections suggests that the demands of his partially incompatible criteria made the satisfaction of any one of them impossible. The readings present only a partial perspective of the issues in the field of thinking; the historical antecedents of current theory are not adequately represented; and some of the selections are readily available in English translation. Among the significant omissions are papers by Wertheimer, Bartlett, Duncker, Katona, Pick, and Goldstein. There is no reference to the literature on associationism and conditioned response which, in Rapaport's opinion, has not contributed significantly to the problem of thinking.

The highlight of the book is provided less by the essays than by Rapaport's voluminous notes concerning their meanings and implications. Written as informal comments, the notes help guide the reader through a labyrinth of fascinating conjectures and confusing contradictions. Passages of lesser importance are omitted in the articles and summarized by Rapaport, and some chapters are supplemented by abstracts of more recent writings by the same authors. When Rapaport feels that a passage is vague, he speculates upon its possible meanings. If he thinks the termi-



nology abstruse, he discusses the context in which it is used and gives the current equivalents. The footnotes summarize significant references in psychoanalytic literature to such topics as fixation, repression, structural aspects of thinking, dreams, psychosis, and obsessive thinking. Accompanying these summaries are critical reports of recent experiments and theoretical papers on the implications of the hypotheses. In discussing these implications, Rapaport displays an amazing erudition, referring to such varied subjects as ornithology, higher mathematics, pre-Socratic philosophy, fine arts, geology, entomology, and epistemology. The comments on each topic are flavored by a richness of clinical observation that reflects years of experience. Many topics for research are suggested: the possible application of certain non-metric mathematical techniques; testable hypotheses pertaining to the learning of obsessions; and the role of interpersonal relationships in the development of thinking.

In a concluding chapter, Rapaport attempts "to extract without documenting" a systematic set of propositions about thinking, and the result is the first complete description of the psychoanalytic system of thinking. In his basic theory he covers such aspects of the development of thinking as the primary process, learning to delay immediate discharge, development of defense-mechanisms, binding of cathexes in the secondary process, states of consciousness, development of derivations, and reality-testing. The principles are then applied to more complex phenomena such as concept-formation, anticipation, attention, creative thinking, communication, and pathological thinking. Unfortunately, however, Rapaport provides only the barest outline of a system. It is so condensed and abstract that sometimes even definitions and simple deductions are difficult to follow. An attempt to digest the material is nevertheless repaid by many fruitful and testable hypotheses.

In general the writing in the book seems unduly abstruse. Conceptualizations of the thinking process are abstract of necessity, and Rapaport compounds the difficulty by translations that are so faithful to the original language that "the central meaning of the authors came out best, and the English second best. . . ." A further bar to communication is created by the nature of footnotes which keep changing both with the topic and with the editor's sophisticated range of associations. Additional problems are created by writing which is occasionally scanty, and occasionally overly involved.

Despite serious drawbacks in communication and the nature of selections, the book should provide an invaluable reference for many groups of psychologists. The neophyte will find in it stimulating introductions to the works of important contributors. The clinician will gain insights into the behavioral implications of certain psychoanalytic theories. The researcher will find intriguing discussions of theoretical systems, strengths and weaknesses of different methodological techniques, and experimental hypotheses. The average reader, regardless of his speciality, will be left with the impression of a rich field, scarcely touched, containing many important and testable problems. As Rapaport notes, even the phenotypes of thought processes have scarcely been described; what is needed, he feels, is a thorough investigation of the forms and varieties of thinking. Rapaport's volume represents a major contribution to the exploration of that field.

University of Michigan

DANIEL R. MILLER



*The Explanation of Human Behaviour.* By F. V. SMITH. London, Constable and Co., 1951. Pp. ix, 276.

One-third of this stimulating book deals with "the activity of explaining" and culminates in a set of criteria by which six systems of psychology are appraised in the remaining two-thirds. The reader is constantly aware of the book's aim to "promote a sympathetic understanding of the difficulties with which authors of systems in Psychology contend."

The activity of explaining is discussed in the light of Piaget's studies with children and with references to kindred features in the explanations of adults. Causal relationships are analyzed and are found to have characteristics in addition to the regular conjunction of antecedent and consequent, the most significant of which are functional relationships which play such a large rôle in the physical sciences. The author is well aware of the possibility that it might be a handicap for psychological systems to follow the pattern of the physical sciences whose methods might not be appropriate to the intrinsic nature of psychological processes. The consequences of this expressed doubt are not, however, followed up in detail. One serious omission in this discussion of explanatory techniques, a surprising one in view of the author's otherwise thorough acquaintance with the literature, would seem to be a consideration of Köhler's principle of dynamic self-distribution of forces as an alternative to machine theory. In a later part of the book a reference to the possible use of this principle might have given added significance to some critical comments on behavioristic systems.

The six psychological systems selected for treatment are identified with the names of McDougall, Lewin, G. W. Allport, Watson, Hull, and Tolman. The first three are regarded as examples of explanatory techniques which seek for instigators of behavior within the person, and the last three—"three possible forms of behaviourism"—deal with behavior as "process in the external world." In each case the author asks: What is this system-maker trying to do? A lucid and sympathetic presentation of the system is then followed by a fair appraisal. The longest chapter is devoted to the position of McDougall whose "attempt to penetrate the essential mystery of instinct" is presented in an impressive and yet critical manner. "Until instincts . . . are fully substantiated by research, the significance of modifications of them at higher evolutionary levels cannot be assessed." If, however, one overlooks the difficulties of the basic concepts, McDougall's system is said to have "an extraordinary breadth of application."

Of Lewin, whom the author treats with eminent fairness, it is said in the end that he did not "succeed in formulating laws more penetrating or extensive than the generalisation of ordinary experience." Yet "even if inconclusive, there was always a certain fresh and provocative quality about Lewin's contributions." The appraisal of Allport's system lays stress on the concept of traits which are associated with neurophysiological structures that, like the neural organization of McDougall's instincts and Lewin's inner tension, "can only be a topic of speculation." Watson's position is treated most briefly, and quite rightly so. One might even question the appropriateness of its inclusion in a book, which is supposed to deal with present-day psychology. Watson is said to have been "never systematic in his presentation nor rigorous in following out his methodological programme."



Hull, next to McDougall, is accorded the most extensive presentation and appraisal. Hull's plan "has consistently been to reveal the basic mechanics" of behavior. "There is no other system in Psychology which affords quite the same satisfaction for the perseverative activity of the human mind," because there is "no unrelated hiatus between stimulus and response." Hull "offers a system which is more rigorous than any others examined. . . . Perhaps the most consistent impression arising from the study of Hull's system is the possibility of extension" as more empirical data are accumulated.

The author considers Tolman's system "a very significant and indeed an ingenious form of behaviourism," although it has not attained "full status as an explanatory system." It contains "a wealth of concepts" and provides "a broad plan for their integration," even though it is inconsistent in the use of language.

The lucid and painstaking treatment of these systems never deviates from the major theme of how each one in turn is capable of explaining behavior, and how each one falls short of this goal. Praise is always tempered with sympathetic criticism. The author's own position is indicated, although not developed, in a final chapter in which it is stated that "actually, a form of behaviourism which admitted the reality of conscious and purposive activity would appear to give greatest promise of approaching the requirements" of a system of psychology. (This is a position which the reviewer has elsewhere called "psychological behaviorism.") One might wish that the author will find it possible to develop his position into a full-fledged system. The book may be said to provide a good answer to the question of why psychology is the only science which is beset with conflicting systems. Since it is also intended for the intelligent layman the author might have done well to emphasize the large communality of interests and working methods among psychologists irrespective of their adherence to a particular point of view.

University of Colorado

KARL F. MUENZINGER

*The Dynamics of Morals.* By RADHAKAMAL MUKERJEE. London, Macmillan and Co., 1951. Pp. xxvii, 530.

Professor Mukerjee attempts in this most erudite volume to develop a rational ethical theory. In doing so, he blends an amazing grasp of sociology, psychology, economics, philosophy, theology, and aesthetics into a coherent, if somewhat staggering, work. The very breadth of this Indian sociologist and economist, who is credited with 31 volumes on economics and sociology, is awe-provoking to a Western psychologist. It is necessary to appreciate Professor Mukerjee's abilities in order to get the full flavor of his work to which a review such as this can scarcely do justice. Perhaps this quote from Gardner Murphy's introduction will make the author's scope apparent: "From human ecology to psycho-analysis, and from group dynamics and the UNESCO studies of social tensions to the broadest problems of human culture, I found him an inexhaustible treasurehouse of ideas."

The thesis of the work is that man can and must in the course of evolutionary progress interiorize the highest moral values by indentifying with the Commonality which is characterized by love, equity, and solidarity. In order to arrive at this ultimate moral development, Mukerjee treats ethics as arising out of man's identification with society and consequent internalization of its moral norms. In so doing



he leans heavily on psychoanalytic theory, especially of the Jungian variety, but at a level of social development Freud never dreamed of. "Ethics, then, is man's developmental mechanism of the mind by which as he gropes his way through various types of groupings, he seeks and realizes those integrative, comprehensive emotions, attitudes and beliefs which can release his inner tension and burden of unconscious guilt, and bring about a most profound self-fulfillment through oneness or communion in society. It is accordingly the *deus ex machina* of his biological and social progress. Man's moral world is structured. His moral principles and norms are defined roughly as those created, selected and strengthened in a stable, progressive integrated structure of the group or groups he lives in" (p. 69).

The groups man "lives in" and progressively identifies with are, according to Mukerjee, of four basic types: crowds, interest groups, society, and commonality. Crowds are held together with "impulsive, hallucinatory, charismatic" bonds and lead to "emotional, irrational, single-tracked, stereotyped" behavior with no resulting moral principles or norms and no "constellation of cardinal virtues." Next on the path of moral development man identifies with interest groups which bind him by contractual agreements; lead him to engage in "impersonal, fractional, casual" behavior; provide principles of "reciprocity, fair play, and consideration"; and develop virtues of "propriety, honesty, integrity." The third type of group, so the thesis goes, is the society or communion, in which man is bound to his fellow man by "affective-rational" ties and engages in "personal, integral, enduring, social" behavior guided by principles of "justice and equity." The virtues of "uprightness, rectitude, impartiality, respect for human dignity and personality, respect for fellow man's body, life, merit, property, social status, reputation or honour" are said to result.

Finally, man will arrive, or has the potentiality to arrive, at the highest order of group identification which Mukerjee terms "commonality." Here we find the author easily accepting what would be termed the mystical by tough-minded Western scientists, but such a designation would not disturb Mukerjee in the least. Indeed one of the refreshing aspects of the work is the willingness of the Eastern social theorist to explore those levels of human experience usually excluded as improper by Western standards of investigation. It is no doubt this aspect of the theory that aroused a feeling of warmth in Gardner Murphy, who has long championed such attempts. Commonality so relates men through "affective-rational-mystical" ties that they engage in "personal-symbolical, integral-cosmic, timeless, beyond-social" behavior, such as Yogi. Their moral principles are those of "love, equity and solidarity" and they hold "wisdom, universal love, pity, compassion, charity, and 'radiant virtue,'" in high regard.

Early in the work the author points out that man does not passively and blindly submit to the group norm with which he has identified, but rather develops new rôles and thereby new morals as a result of the projection of his own needs, wishes, and phantasies on the group. Thus the mechanism of progress is for the moral hero of society to follow his own image of self-actualization and to break with the established groups and thereby set up a pattern for others.

These ideas are developed in the volume in relation to a thorough citation of the relevant literature from all the areas of social science. As is obvious, however, Mukerjee goes beyond the present stage of social science which he finds compartmentalized, mechanistic, and individualistic, and suggests that the research of the



future be oriented toward the creation of an ethic, to quote Murphy, "in which the biosocial nature of man's needs is fully realized with equal attention to what is common to all humanity and to what is special, unique, unrepeatable in the 'individual ethical choice.'" Those social psychologists who have advocated a frontal attack on values will find much of use in this volume.

Bryn Mawr College

DONALD R. BROWN

EUGENE V. SCHNEIDER

*Child Psychology.* By HORACE B. ENGLISH. New York, Henry Holt, 1951. Pp. ix, 561.

*Child Psychology.* By GEORGE G. THOMPSON. New York, Houghton Mifflin, 1952. Pp. xii, 667.

These books, although they bear the same title, deal with the field of child psychology quite differently. Thompson's treatment resembles that found in the typical textbook on the subject while English's approach is more of a departure from the usual. More specifically, Thompson has attempted to write a book for "the serious student who generally prefers an integrated interpretation of the research literature" (p. x). English, on the other hand, has prepared his volume for the teacher (in the general as well as the more specific sense of the word) who seeks practical training and assistance in helping children to grow and develop into healthy and efficient adults. From these comments it is apparent that it will be profitable to consider each volume separately and in the light of its specific objectives.

As already noted, English is interested in the application of the major theories and established facts of the science of child psychology to further the general welfare of children. To do this he has chosen to limit himself to a discussion of the more critical concepts and principles (such as discipline and authority, emotion) and the major developmental dimensions (such as physical, intellectual, and social development). In the treatment of each topic he has presented the main facts and some of the prevalent thinking that attempts to interrelate them. Throughout the book there is a strong emphasis on the need for, and training in the art of, observing behavior. This keynote is sounded in the second chapter entitled "A dynamic study of children" and emphasized throughout by means of suggestions and illustrations in special sections at the end of each major division of the book. In a way, the book may be thought of as a well-conceived extension and elaboration of the little volume published in 1941 by English and Raimy called *Studying the Individual School Child: A Manual of Guidance*. If one views child psychology as a psychotechnology, or if one is particularly concerned with training students, particularly students of education, in the art of observing the behavior of children, this volume should serve well. It reads easily and the material is presented in an interesting fashion, which is perhaps a reflection of the author's extreme sincerity and strong conviction.

Thompson has set for himself the Herculean task of bringing together the relevant literature in some sort of a systematic fashion. Although his book is designed primarily as a contribution to the science of growth and behavior, it is not devoid of suggestions for application. A great mass of literature (about 1000 references) is brought together in fourteen chapters. The three concepts selected to integrate the widely diversified techniques of observation, methods of measurement, data, theories



and speculations are: maturation, learning, and personal-social adjustment. These "fundamental processes" are treated in the first part of the book "in the hope that they will demonstrate the interrelatedness of all phases of psychological growth, and the intimate relationship that exists between psychological growth and environmental circumstances" (p. 203). Eight of the ten chapters that follow (about 60% of the pages) are devoted to a topographical presentation of the usual segmental aspects of behavior development. The final chapter is concerned with developmental trends in personality.

Thompson's attempt to integrate the subject matter of child psychology represents a much needed and laudable effort, but the product seems to fall short of his goal. After formulating his principles, Thompson fails to show how they may be employed to relate the essential stimulating conditions in the various phases of development to the behavior changes observed. Instead he offers the typical descriptions of the longitudinal sequences of behavior and hopes that the reader will be able to relate them to the pertinent antecedent conditions. It might be noted that an integrated treatment should deal with the problem of personality in the formulation of the principles rather than as something to be superimposed on the data relating to the various aspects of development. Nevertheless, Thompson's books represents one of the first attempts at systemization in the field of child psychology. Considered in this light it has much to commend it.

University of Washington

SIDNEY W. BIJOU

*An Introduction to Child Study.* by RUTH STRANG. New York, Macmillan, 1951. Pp. xi, 705.

This is an introductory textbook, written with the intention to reach a wide variety of persons who work with children, but who are not specialists in child psychology or child development. The author mentions, as people who are often baffled about children's behavior and who need to learn something about normal children and methods of teaching and guidance, "parents, grandparents, friends, teachers, doctors, dentists, nurses, social workers, policemen, recreation leaders." The book is obviously intended for all of these, with the greatest emphasis on information which would be useful for parents and teachers. The method is primarily didactic, taking the form of teaching the facts about young children and giving suggestions and directions for the best methods of caring for children and guiding their development. The accent is on the positive, and the concern is with setting up optimal conditions for the development of normal children. There are, however, frequent references to good current research in support of many of the statements, and there are fairly extensive bibliographies, one at the end of each of the book's six parts. The students are invited to look through these bibliographies for references for further reading on subjects of interest to them.

The main parts of the book are, (1) The roots of behavior, (2) Early preschool period, The first two years, (3) The preschool period—Years three, four, and five, (4) The primary period, (5) From the primary period to adolescence, and (6) The adolescent years. Part 1 is concerned with the baby at birth, and hereditary and prenatal influences. Each of parts 2 through 5 contains chapters on development, learning, problems, and child study and guidance. In these five parts there is a



certain amount of repetition, but with emphasis on the particular age under consideration. The repetitions, however, are for the most part generalizations and many references which occur in only one age division are equally applicable to the others. Part 3, on the preschool period, contains an additional chapter on "The family's daily routine and the child's diet," while part 6, on adolescence, is rather short, containing a single chapter on development and guidance. The advantage claimed for this organization is that it directs study to successive age-periods in children's growth, calling attention to all aspects of the child, but with repetitions for teaching purposes of important things which recur in each successive period. The author also suggests that by use of the index of subjects the student can read as a unit all of the text discussion of any given topic. Such a procedure would appear to be rather complicated for the beginning student. It would be better, for this purpose, to insert cross references at appropriate places in the text.

The subject-matter covered is broad, but general, and many subjects are touched on only briefly. For example, in the preschool section there are two sentences under the heading "Personal health habits." "Laughter and crying" and "Individual differences" take half a page each. Other topics are treated at somewhat greater length, but nothing is discussed in detail or with any indication of conflicting views or theories, or of currently inadequate experimental data.

The non-technical nature of the book is indicated by the fact that there are no tables and just two schematic figures, the other illustrations being photographs of children playing or in social or family groups. There are frequent brief illustrative case-sketches and examples of behavior. Each chapter ends with "questions and problems for class discussion or study groups." In general this book should be useful for an elementary course or a study group, but it is probably too elementary for a text in child psychology or development at the college or university level.

University of California

NANCY BAYLEY

*Lehrbuch der Psychologischen Diagnostik.* By RICHARD MEILI. Bern, Verlag Hans Huber, 1951. Pp. 372.

This is essentially a new book, not merely a revised edition of the author's *Einführung* which appeared in 1935. In that volume Meili wrote in the introduction: "Psychodiagnosis is an art and therefore essentially not teachable." In the *Lehrbuch*, however, he insists that "the application of psychodiagnostic methods can only be justified as long as we maintain towards them a very critical attitude and continually check and scientifically control the results." The difference between the two attitudes reflects the development of clinical psychology in the intervening years.

The book consists of five sections plus an appendix. In Section I there is a discussion of the types of problem which confront the clinical psychologist, a brief review of the history of mental testing, a critique of pre-scientific evaluations of personality, and a rapid survey of the scientific methods available to the clinician for the psychological appraisal of the patient. Even in this introductory section a good deal of experimental evidence is marshalled at relevant points. The discussion is as brief as possible and, in every instance, clear and to the point. In Section II on 'Psychological characteristics and their study'—the core of the book—problems of general intelligence, special abilities, and personality are treated systematically. Section III deals with techniques of clinical testing and the evaluation of quantitative



results, together with the writing of reports. The statistical problems of reliability and validity are treated in Section IV on 'Analysis and control of tests.' There is a good discussion of normal and non-normal distributions, of factors influencing reliability, and the question of validity as a function of correlation with other tests. In the final section Meili treats a variety of topics such as the effect on test results of chronological age, sex, education, and occupational history, the so-called constancy of the IQ, practice effects, and related problems. Detailed descriptions of selected tests and a brief introduction to statistical methods are appended.

Meili's book has two principal shortcomings. Its coverage of the more recent American literature is deficient, and it could do with a chapter on the details of test-construction. Were these weaknesses remedied, a translation of the volume would make an ideal introductory text for graduate students in clinical psychology.

College of Medicine  
University of Illinois

MARIANNE L. SIMMEL

*A Handbook of Psychosomatic Medicine; with Particular Reference to Intestinal Disorders.* By ALFRED J. CANTOR. New York, The Julian Press, 1951. Pp. xvii, 302.

Dr. Cantor is an eminent proctologist and a Founding Fellow of the International Psychosomatic Society. In this badly misnamed volume, he earnestly and effectively urges his fellow proctologists "to avoid the pitfall of single causation" in dealing with diseases of the gastro-intestinal tract. The major emphasis, as befits his professional preoccupation, is on the colon, "the sounding board of the emotions." Balanced attention is given to the multiplicity of factors underlying diseases of the colon. The first chapter, entitled "Psychosomatic Medicine is Medicine," is a model of simple and clear exposition. If the rest of the book maintained the level of the first chapter, it would be one of the major publications in the field of psychosomatic medicine. It does not, however, possibly because the author has chosen to do too much. There really are two books here. The first half of the volume is devoted to the introduction of a new psychotherapeutic system; the second half to a review of the physiology of intestinal disorders, the possible contribution of psychogenic factors, and therapeutic management.

The new technique of therapy is presented in 69 pages of text. Why the author chose to present a major departure in abbreviated psychotherapy in so short a space is a mystery. The theory is of interest to the psychologist, especially the clinical psychologist. To this reviewer, the essence of the technique lies in the semantic proposition that "the word is not the thing," a proposition to which Dr. Cantor devotes much attention.

It is a new technique only in its emphasis on *repeated* abreaction to the point of boredom and laughter, and on *systematic* abreaction of all the important episodes in the patient's life, insofar as these can be determined. Proof of the relative efficacy of the method is not presented, and the relation of this therapy to other methods is hardly hinted at. If Dr. Cantor's techniques are put to systematic test, valuable knowledge is sure to follow. If they are not, we will have another school of abbreviated psychotherapy, providing methods that are effective for *some* therapists, with *some* patients, and for unknown reasons.

The second half of the book, dealing with various gastro-intestinal disorders, will be of minor interest to readers of the JOURNAL. No real attention is paid to the



knotty problems in psychosomatic medicine that face the psychological investigator, such as the validity of the personality profiles allegedly characteristic of different disorders, or the problem of the differential pattern of somatic response in different affective states.

Fels Research Institute.

JOHN I. LACEY

*Current Trends in the Relation of Psychology to Medicine.* Edited by WAYNE DENNIS. University of Pittsburgh Press, 1950. Pp. 189.

In this monograph the relationship of psychology to a number of fields allied with medicine is maturely considered. The book is introduced by Wayne Dennis who had the responsibility of organizing the symposium on this topic at the University of Pittsburgh. Robert H. Felix, Carlyle Jacobsen and Nathan W. Shock outline the development of psychology in public health, in medical schools, and in gerontology. Paul E. Huston describes the recent trends in the interaction between psychology and psychiatry. Hans J. Eysenck deals with clinical psychology in England. Robert A. Patton and Yale D. Koskoff discuss the contributions of psychology to experimental psychopathology and to neurological research in chapters which afford a stimulating view of some current theories and the research which these theories invite.

On very rare occasions the reader may question some statement such as the one made by Eysenck to the effect that "the aim of the American clinical psychologist (is) to be similar to the psychiatrist in function and area of work," a view which reflects an inadequate grasp of American clinical psychology. With the general tenor of the approach, however, no quarrel will exist. The contributions of psychology to the field of medicine are not minimized, but the fact is emphasized that such contributions represent only an initial step in a field which as yet remains almost wholly unexplored. The monograph is highly recommended for psychologists who work in medical situations.

Northwestern University Medical School

G. K. YACORZYNSKI

*Mémoire et Personne.* By GEORGES GUSDORF. Paris, Bibliothèque de Philosophie Contemporaine, Presses Universitaires de France, 1951. In two volumes, Pp. 563.

This is a fascinating and stimulating text, though it is highly speculative and fails to meet the minimum requirements of a scientific study. The author, a philosopher, has not attempted to approach his subject as a scientist. The data provided by scientific psychology constitute in this study nothing more than a stepping stone for far-reaching expeditions into the realms of philosophy or metapsychology.

In accordance with a method which is quite popular in Europe at present, the author conducts a painstaking phenomenological analysis of the various aspects of forgetting and remembering. The data on which that analysis is based are quite controversial, being drawn mainly from psychiatric texts chosen rather arbitrarily and from a great number of literary sources, (e.g. Proust, Valéry, Goethe, Gide).

With the criteria obtained through this analysis, the author conducts a critical evaluation of the best-known philosophical theories of memory, adopting finally the one proposed by Jean Delay who differentiates two main aspects of memory: an autistic aspect through which events are tied to the flow of consciousness, and a rational aspect based on the participation of the individual memory in the memory of the group. Gusdorf, however, attributes much more importance to the autistic



aspect of memory than to the rational one. He speculates at length on the ties between this autistic aspect of memory and certain features of the Nietzschean philosophy, neglecting almost entirely the process of cultural elaboration which is behind the rational aspect of memory.

In summary, a complex and very unstable structure of philosophical interpretations is built on the basis of data which are incomplete and often of doubtful quality. This leaning tower does, however, contain stimulating material, since from a wealth of literary and philosophical writings it draws a number of original suggestions for research.

Montreal

GEORGES DUFRESNE

*Air War and Emotional Stress.* By IRVING L. JANIS. New York, McGraw-Hill, 1951. Pp. ix, 280.

This monograph is based on a study by Dr. Janis for the RAND Corporation, undertaken under Air Force sponsorship, "to evaluate the effects of air warfare and to indicate the nature of problems in this field which may arise in planning the defense of the United States against air attack." The report is effective in integrating a widely scattered literature on these problems. It will be of interest to students of personality as well as to medical and administrative officers of civilian defense groups.

The discussion is in three parts. Part I provides a concise but comprehensive analysis of the emotional impact and sequelae of the atomic bombing of Hiroshima and Nagasaki. Part II considers the effects of aerial bombing in World War II on the British, German, and Japanese civilian populations. The generalizations concerning psychiatric disorders, fear and emotional adaptation, morale and adjustment follow a consistent pattern. Part III deals with problems of civilian defense against atomic attack through the air, including disaster-control, organizing and training civilian defense forces, and educating the civilian population for survival. Since many opinions on crucial issues concerning the morale, feelings of security, and adaptation of people in vulnerable areas are largely untested hypotheses, the author makes a number of proposals for much needed research. These problems, which are so important for national survival, deserve the attention of social and clinical psychologists, psychiatrists, and anthropologists, both in and outside of government service.

U.S.A.F. School of Aviation Medicine

S. B. SELLS

*Die Krampfschädigungen des Gehirns.* By WILLIBALD SCHOLTZ. Berlin, Springer-Verlag, 1951. Pp. vi, 116.

The author provides an exhaustive, chronological introduction to the histopathology of epileptoid convulsions. With the aid of valuable microscopic material and a world-wide literature, he discusses the general pathogenic situation in the brain, with special emphasis on levels and areas of preference, and provides an objective approach to the morphological diagnosis of convulsive conditions. Typical cases which involve recent and older damages are described and accurately analyzed. More complex cases, in which convulsive damages develop in addition to other progressive or stationary pathological conditions, also are treated. In the final chapters Scholtz considers the consequences of experimental and therapeutic convulsions, as well as the rôle of spontaneous convulsions, in the etiology of somatic and psychologi-



cal disorders. He believes that epileptic or epileptiform convulsions of any origin, spontaneous or induced, provoke a vasomotor reaction. The hemodynamic deviation results in a temporary ischemia and, consequently, in anoxemia, which is responsible for the elective parenchymal necrosis at various neural levels.

Austin State Hospital  
University of Texas

C. DE CSERNA

*Large Scale Rorschach Techniques.* By M. R. HARROWER and M. E. STEINER. Second edition. Springfield, Illinois, Charles C Thomas, 1951, Pp. xx, 353.

This is a second edition of an earlier work reviewed by Super (this JOURNAL, 58, 1945, 410-415). Much of the first volume is reprinted verbatim, and several minor errors noted by Super remain uncorrected. Two new chapters have been added, one on recent developments and one on a study of 'card pull.' The reader is informed in the first of these that many workers initially skeptical of the group Rorschach have found that "the Rorschach is foolproof" (p. 232). Little emphasis is placed upon validating research (although summaries of normative data are presented) and negative results are minimized. The chapter on 'card pull' deals with the relation between group and individual data. Numerous hard-to-interpret tables are presented with no integration of the information they contain. Although the authors insist that the group Rorschach is "a different type of test . . . a procedure in its own right" (p. 233), they have obviously been influenced by traditional concepts and long personal experience with the individual test.

University of Texas

GLEN P. WILSON, JR.

*Psychological Factors of Peace and War.* Edited by T. H. PEAR. New York, Philosophical Library, 1950. Pp. 262.

This collection of ten timely papers (with an introduction by T. H. Pear) was sponsored by the United Nations Association. Although the book is divided into two parts on the basis of the technical knowledge demanded of the reader, several papers in the 'non-technical' part deal seriously with basic social psychological problems.

Professor Pear (University of Manchester) contributes a paper relating problems of peace and war to social organization and value. Then follow, in order, a survey of attitude studies on war and aggressiveness (by H. J. Eysenck, Maudsley Hospital, London); a brief treatment of personality viewed in relation to cultural pattern and social role (by Madeline Kerr, Liverpool University); a treatment of the role of women (by J. Cohen, Hebrew University, Jerusalem); a discussion of moral, political and 'psychological' (in this case psychoanalytic) approaches to the integration of peoples (by J. C. Flugel); a strong plea for international cooperation in social research and for greater emphasis on such research in policy-making (by Gordon W. Allport); an evaluative review of experiments on the consequences of frustration (by Hilda Himmelweit, Maudsley Hospital); a study of German prisoners of war (by H. V. Dicks, The Tavistock Clinic, London); two provocative statistical chapters—one dealing with threats ("warlike preparations") and their consequences, the other dealing with "deadly quarrels"—by L. F. Richardson. Professor Pear's introduction summarizes the high points of the volume, integrating its diverse contents in terms of his own theoretical orientation and in terms of a serious concern with the



plight of man. The volume will interest expert and student in intergroup relations.  
University of Oklahoma MUZAFAER SHERIF

*Relation of Psychological Tests to Psychiatry.* Edited by PAUL H. HOCH, and JOSEPH ZUBIN. New York, Grune and Stratton, 1952. Pp. viii, 301.

At its 1950 meetings, the American Psychopathological Association invited a number of experts to discuss critically the role of tests in psychiatry. The present volume reports the proceedings of that conference. Altogether, fifteen papers are presented, some describing original research and others raising points for discussion or summarizing previously published work. The main papers are loosely organized under four headings: (1) the historical bases for psychological tests, (2) the diagnostic use of tests, (3) the influence of exogenous factors on testing procedures, and (4) the influence of the "psyche" on test-performance. As one might expect from the heterogeneity of the intended audience and the wide variety of participants, the quality of the individual contributions varies considerably. Although there are several reports of original research which are worthy of note, the collection is too omnibus in nature to be of great value to most psychologists.

University of Texas

WAYNE H. HOLTZMAN

*The Bender-Gestalt Test.* By GERALD R. PASCAL, and BARBARA SUTTELL. New York, Grune and Statton, 1951. Pp. xiii, 274.

For the first time, an adequately standardized and validated quantitative scoring procedure for the Bender-Gestalt Test has been presented for general use. Following a short introduction and theoretical orientation to the problem, the authors describe in detail a series of studies dealing with scoring and reliability, standardization and validation on a population of 474 normal adults and 356 adult psychiatric patients. A second section consists of several short chapters profusely illustrated with representative test-records demonstrating the clinical use of the Bender-Gestalt Test as scored by the authors' system. Constituting the major portion of the book, the appendix contains numerous richly illustrated examples of scorable deviations in test-performance. Ample directions are given for administering and scoring the test to ensure adequate reliability.

The scoring system yields a single score for the complete protocol, the higher the score the more pathological the record. It will seem to many psychologists that much valuable information has been sacrificed at the expense of obtaining a single quantitative index, although the index has sufficiently high reliability and validity in a variety of situations to prove highly useful. Three cross-validation studies of the power of the index in separating normals from psychiatric patients yielded biserial correlation coefficients above .70. Although the reported validity of the score in distinguishing between psychotics and neurotics is considerably lower, the results are impressive when contrasted with others in this field.

University of Texas

WAYNE H. HOLTZMAN

*De la Douleur.* By F. J. J. BUYTENDIJK. Translated from the German by A. REISS. Paris, Presses Universitaires de France, 1951. Pp. xvi, 160.

It is difficult to think of any man better qualified than Professor Buytendijk to write on the mysterious subject of pain. Philosopher, physiologist, psychologist,



Buytendijk has turned his talents to considering the physiological nature of pain, the psychological explanation of pain, and the philosophical significance of pain.

For his physiological evidence, Buytendijk relies principally on the classical studies of pain that appeared in the first quarter of this century and on the more recent work of J. D. Hardy. Pain, he concludes, is related to the functioning of the sympathetic nervous system, and although it "peut bien être un élément d'une impression sensorielle, . . . un sens de la douleur n'existe pas comme tel." The discussion of the psychological aspects of pain is confounded with considerations of metaphysics and the biological utility of pain. The contemporary American psychologist would be most interested in the seven pages devoted to pain and habit-formation. The references are principally to the work of Pavlov and his students, and the conclusion is simply that a weak shock, properly positioned and timed, aids habit-formation. For the most part the author's psychological consultants are Titchener, Stumpf, Külpe, and their contemporaries. Buytendijk strives to avoid their elementism, insisting that the problem is more dynamic. Nevertheless, although he does suggest a behavioral definition, he concludes, with Stumpf, that pain must be considered psychologically as a "'sensation d'un sentiment' ('Gefühlsempfindung')." Buytendijk is most concerned with the philosophical aspect of pain. He feels that it is useful in, what might be called, the growth of the psychological self.

The tenor of this book is best summed up by the author's own statement. "We conclude, therefore, that the simple fact of the existence of the sentiment of pain in higher animals can be understood neither through comparative anatomy nor through the knowledge of animal behavior. Pain has, from all evidence, a more profound reason which must be searched for in the notion of animal existence."

Knox College

ROBERT S. HARPER

*Traité de Psychologie Appliquée; Volume ii, Methodologie psychotechnique.* By H. PIERON, PIERRE PICHOT, J. M. FAVERGE, and JEAN STOETZEL. Paris, Presses Universitaires de France, 1951. Pp. viii, 123-339.

A brief anticipative sketch of Pieron's large undertaking on behalf of the applications of psychology was drawn here (63, 1950, 159) when the first volume (*La psychologie différentielle*) was under review. Now appears the second, the psychotechnical installment, the four authors writing (in the announced order) the four chapters, Measurement and methods of testing (psychometry), Investigations of personality, Statistics, and La connaissance des opinions. The bibliographies of Western Europe and America are widely and ably regarded, emphasis being laid upon method and its issues. The reviewer gains the impression that most new integration and criticism are to be found in Stoetzel's section (iv) upon the investigation of opinion; although here occur also the greatest transgressions into non-psychological territories. Freshness and clarity of arrangement and exposition characterize the entire volume, which is not likely to suffer from neglect in these states. The next volume to appear (*Livre iii*) is to treat of *Aptitudes*.

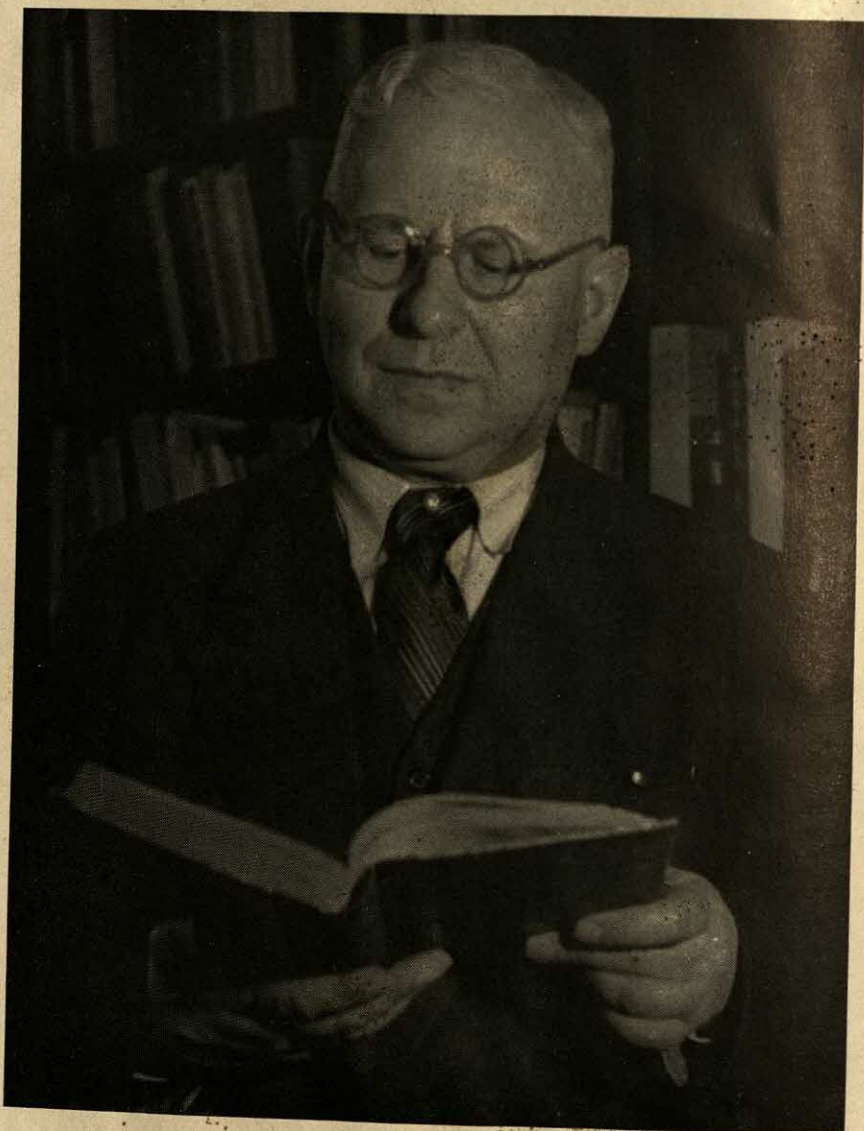
Stanford University

MADISON BENTLEY









David Katz

(See page 638)



# THE AMERICAN JOURNAL OF PSYCHOLOGY

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## "FACIAL VISION": THE PERCEPTION OF OBSTACLES OUT OF DOORS BY BLINDFOLDED AND BLINDFOLDED- DEAFENED SUBJECTS

By CAROL H. AMMONS, University of Louisville, PHILIP WORCHEL, and  
KARL M. DALLENBACH, University of Texas

In the Cornell series of studies upon the perception and avoidance of obstacles without vision, it was found (1) that audition is the necessary and sufficient condition; (2) that pitch is the auditory dimension involved in the perception; and (3) that high audible frequencies of approximately 10,000 ~ and above are necessary stimulus-conditions.<sup>1</sup> In the light of these results, anyone, blind or blindfolded alike, possessing normal hearing should be able to acquire the ability.

These conclusions, particularly those bearing upon the normalcy of the perception, stand in opposition to the findings of the earlier investigators. Many of the earlier writers regard 'facial vision' as a special ability, like artistic, mathematical, or musical talent, which is not shared by everyone.<sup>2</sup> Diderot, the first to investigate this problem, regarded obstacle-perception as an "amazing ability" possessed only by a few of the blind.<sup>3</sup> This point of view has found support in the fact that not every blind person possesses the ability nor is able to acquire it. Villey, for example, found that only 42 (66.6%) of 63 soldiers blinded in World War I had been able to

\* Accepted for publication March 25, 1951. This study, the experiments of which were conducted at Tulane University by the junior authors during the fall of 1947 and winter of 1948, was suggested by the senior author who is responsible also for the present report.

<sup>1</sup> Michael Supa, Milton Cotzin, and K. M. Dallenbach, "Facial vision": The perception of obstacles by the blind, this JOURNAL, 57, 1944; 133-183; Philip Worchel and Dallenbach, "Facial vision": Perception of obstacles by the deaf-blind, *ibid.*, 60, 1947, 502-553; Cotzin and Dallenbach, "Facial vision": The rôle of pitch and loudness in the perception of obstacles by the blind, *ibid.*, 63, 1950, 485-515.

<sup>2</sup> S. P. Hayes, The psychology of the blind, in Helga Linde's *What of the Blind?*, 1938, 93; also *Contributions to the Psychology of Blindness*, 1941, 50.

<sup>3</sup> Denis Diderot, Letter on the blind, 1749, *Early Philosophical Work*, trans. by M. Jourdain, 1916, 63-141.



acquire the ability during the dozen or more years since their injuries.<sup>4</sup> Wolfflin found large individual differences among his Ss: some possessed a "fine obstacle sense," some had only a "weak sense," and some lacked it entirely.<sup>5</sup> Lamarque similarly found wide variations among his Ss.<sup>6</sup> The superiority in performance of blind over blindfolded sighted Ss, repeatedly demonstrated in experiments, has also been regarded as evidence of a special ability which reaches its highest development only in the blind.<sup>7</sup>

### PROBLEM

The purpose of the present study was twofold: (1) to determine whether the results and conclusions of the Cornell studies—which were conducted indoors in a large enclosed hall—could be duplicated when the experiments were conducted outdoors under conditions approximating more nearly those met by the blind in everyday life; and (2) to discover whether every person with normal hearing—blind and blindfolded alike—is able to acquire the ability to perceive obstacles—a consequence of the conclusion that *audition is the necessary and sufficient condition*.

### METHOD AND PROCEDURE

This study was divided into two parts (Parts I and II) of four and three series of experiments, respectively. All the experiments were conducted out of doors on a concrete walk 4 ft. wide which extended from the side door of the Psychological Laboratory of Tulane University across the campus quadrangle at an angle of about 11°. The experimental area was 60 ft. in length. The first 40 ft. were bounded on one side by the Laboratory but the last 20 ft. were in the clear, *i.e.* no building was on either side or in front of it nearer than 200 ft. Grass bordered both sides of the walk.

A heavily traveled boulevard was about 300 ft. from the experimental area. To people with normal hearing, the traffic noises were clearly audible, as were also the noises from adjacent construction—pneumatic hammers and drills, etc.—and from students going to and fro between classes. Passers-by were blocked from the experimental walk but they were permitted to go around it on the grass. The noise-level at the experimental area, measured frequently during the experimental periods, varied from 30 to 70 d.b., being at the higher levels most of the time.<sup>8</sup>

The procedure throughout all the experiments was the same as that used in the Cornell studies.<sup>9</sup> After being blindfolded, S was placed at one of five starting-positions—0, 3, 6, 9, or 12 ft. from a fixed point near the Laboratory door—and

<sup>4</sup> Pierre Villey, *The World of the Blind: A Psychological Study*, 1930, 101-131.

<sup>5</sup> E. Wolfflin, Untersuchungen über den Fernsinn der Blinden, *Zsch. f. Sinnesphysiol.*, 43, 1909, 187-198.

<sup>6</sup> Georges Lamarque, Sensations of obstacles in the blind, *J. de Psychol.*, 26, 1929, 494-522.

<sup>7</sup> August Krogius, Zur Frage von sechsten Sinn der Blinder, *Zsch. f. exper. Pad.*, 5, 1907, 77-89.

<sup>8</sup> Measured by a General Radio Sound-Level Meter, Type 759.

<sup>9</sup> For a detailed description see Supa, Cotzin, and Dallenbach, *op. cit.*, 152, 162.



instructed to go down the walk toward an obstacle that was at one of five distances—6, 12, 18, 24, or 30 ft.—from the starting-position. Both starting-position and obstacle-distance were selected by planned haphazard choice which guaranteed that each was used as frequently as every other one. None of the Ss knew the obstacle-distances used nor that he was placed at different starting-positions at successive trials. When brought to a starting-point, S, at a tap on his back, walked towards the obstacle until he perceived it or collided with it. If he perceived it, he stopped, raised his right arm (first perception) and then, at a second tap, continued to approach it, attempting to come as near as possible to it without touching it, when he again stopped and raised his left arm (final appraisal).

Two Es were used throughout the trials.  $E_1$  had charge of the obstacle, placing it at different positions according to the design of the experiment.  $E_2$  had charge of S, placing him at the proper starting-point, giving him the starting- and continuing-signals, recording the distances of his 'first perceptions' (p) and 'final appraisals' (a) from the obstacle, and leading him back after a trial to the next starting-point. The return to the starting-point was made in a circuitous route over the grass that S would be disoriented and would not know how far he had walked during the trial. The Es interchanged duties on alternate days that no habitual, involuntary cues regarding the position of the obstacle would be given by them to the Ss. If either gave the Ss cues involuntarily, that should become evident in differences in the Ss' performances on alternate days.<sup>10</sup>

A series of 30 trials was completed by an S during an experimental hour. S served at the same hour every day except Sunday (which was omitted because the noises of the other days were either greatly reduced or entirely absent) and until he had completed 8 series of 30 trials each or had clearly demonstrated that he had learned to perceive obstacles—whichever came first.

Our criterion of learning was 25 successes in 30 trials; a success being scored when S reported his first perception and final appraisal without touching the obstacle. If he could not accomplish that criterion within 240 trials, it was assumed that he could not learn to perceive obstacles under the conditions under which he was serving, and that part or phase of the study was discontinued with him.

*Subjects.* The Ss, 20 in number (7 women and 13 men), were students majoring in psychology at Tulane University. All were naïve regarding 'obstacle perception' and the purpose of this study. When the immediate task was explained to them—that they were to learn to detect the presence of obstacles without vision—they all expressed grave doubts regarding their ability to do so but were willing to try.

The Ss were divided by chance into two groups (Groups A and B). In Part I of the study, the Ss of Group A were merely blindfolded, their hearing was left intact, whereas the Ss of Group B, in addition to being blindfolded, had their hearing impaired by having their ears stopped. In Part II, these conditions were reversed. The Ss of Group A were blindfolded and deafened and those of Group B were merely blindfolded.

Audiograms by air conduction were made with a Maico Audiometer for all the Ss. The results are summarized for the Ss of each group in Table I. As this Table shows, 8 Ss of Group A and 7 of Group B possess normal hearing in one or both

<sup>10</sup> No constant difference was detected between the performances on alternate days, hence the Es gave either similar involuntary cues or none. We believe they gave none.



ears<sup>11</sup> and 2 of Group A and 3 of Group B have hearing losses in both ears of varying amounts at different frequency-levels. For a chance division of the Ss, the two groups are fairly well matched.

*Apparatus: (1) Obstacle.* The obstacle placed in S's path, a duplicate of the one used in all the Cornell experiments, was a 1/4-in. Masonite board 4 ft. wide and 4 ft. 10 in. high. It was attached to a portable standard and so placed that its lower edge was 2 ft. above the walk. Its upper edge (6 ft. 10 in.) was therefore well above the height of S's ears.

*(2) Blindfold.* U. S. Navy Dark Adapter Goggles, cotton-filled and equipped with

TABLE I  
SUMMARY OF THE MAICO AUDIOGRAMS OF THE TWO GROUPS OF Ss

Group A				Group B			
S	Age (yr.)	College standing	Maico audiogram	S	Age (yr.)	College standing	Maico audiogram
1. AL	19	Jr.	normal, both ears	1. MB	19	Soph.	normal, both ears
2. CW	22	Sr.	normal, both ears	2. CF	20	Gr.	normal, both ears
3. NW	19	Soph.	normal, both ears	3. MS	20	Sr.	normal, both ears
4. SR	23	Jr.	normal, both ears	4. WW	24	Gr.	normal, both ears
5. DL	20	Jr.	normal, both ears; superior lower ranges	5. JB	23	Sr.	normal right ear; 60-db. loss in left ear at 11,584~
6. LA	21	Sr.	normal, both ears; superior upper ranges	6. JR	24	Sr.	normal right ear; 20-db. loss left ear at 1024~ and above
7. GS	20	Sr.	normal left ear; 50-db. loss in right ear at all frequencies	7. BJ	21	Soph.	normal right ear; 60-db. loss left ear at 11,584~
8. AM	20	Gr.	normal left ear; 55-db. loss in right ear at all frequencies	8. DE	26	Sr.	right ear: normal except 40-db. loss at 4096~ and above; left ear: normal except 30-db. loss at 11,584~
9. BH	21	Sr.	normal both ears up to 8192~; 50-db. loss in both ears at 11,584~	9. BG	22	Jr.	normal both ears to 2896~; 25-db. loss in both ears at 4096~ and above
10. JE	21	Sr.	normal both ears up to 5792~; 20-db. loss in both ears at 8192~ and above	10. ML	23	Sr.	right ear: 10-db. loss at all frequencies to 8092~ and 40-db. loss at 11,584~; left ear: normal below 512~ 30-db. loss between 1024~ and 8192~; and 60-db. loss at 11,584~

an opaque shield, were used as the blindfolds. They were sealed around the edges to S's face with adhesive tape that they would be completely lightproof.

*(3) Auditory impairment.* S's hearing was impaired as follows: An MSA ear-defender was inserted into the meatus of each ear.<sup>12</sup> Over this was placed a plug of modeling clay which was fitted snugly into the concha. Two layers of cotton batting,

<sup>11</sup> Unlike auditory localization, the 'obstacle sense' is not dependent upon binaural stimulation. Monaural stimulation is, as Supa, Cotzin, and Dallenbach found (*op. cit.*, 180 f.) a sufficient condition which yields performances that are equal to those obtained with binaural stimulation.

<sup>12</sup> The ear-defender (HA-15369)—obtainable in two sizes from the Mine Safety Appliances Company, Pittsburgh, Pennsylvania—is a tapered rubber tube containing an outer barrier of heavy metal and an inner barrier of soft rubber. There are thus two barriers separated by an air space through which the noise must penetrate before it can strike the eardrum.



one of sponge rubber, and a woolen ear-muff filled with cotton were then laid over the ears and held tightly in place by the elastic bands of the blindfold.

The hearing-loss under these conditions averaged for our 20 Ss approximately 30 db. at 64-4096~ and 50 db. at 5792-11,584~. Despite this loss the Ss could intermittently hear the louder noises of the experimental area and the click and scrape of their shoes on the walk. They could also understand *E* when he raised his voice above the usual intensity of normal speech.<sup>13</sup>

(4) *Measurement of performance.* The edges of the walk were marked in feet. *E*<sub>2</sub> was able therefore to note by immediate inspection the distance from the obstacle at which *S* gave his judgments. *S*'s first perceptions were measured to the nearest foot and his final appraisals to the nearest quarter-foot.

## PART I

### EXPERIMENT 1

The object of Experiment 1 was twofold: first, to discover whether blindfolded Ss, possessing normal or near-normal hearing, could learn to perceive obstacles out of doors; and secondly, to discover whether Ss having their ears stopped in addition to being blindfolded could, out of doors, acquire the ability to perceive obstacles.

*Procedure.* The Ss of the two groups (Groups A and B) served in chance order as their schedules permitted but always, for a given *S*, at the same hour every day.

*Instructions.* At the beginning of every experimental period the following instructions were read *S*.

After you have been blindfolded you will be placed on a concrete walk facing an obstacle. When you are tapped on the back, walk forward to the obstacle. You must stay on the walk; if you step off, return to it and continue forward. When you perceive the obstacle raise your right arm. At a second tap on the back, lower your arm and continue toward the obstacle. Approach it as closely as possible without touching it. When you have reached that point, raise your left arm.

After reading the instructions, no word was spoken until after the conclusion of the trial which was ended with *S*'s final appraisal of, or collision with, the obstacle. *S* was frequently asked after a good performance to describe the bases of his judgments.

*Incentives.* Since this was an experiment in learning we utilized the following incentives which have been found to be of aid to *S*: (1) punishment—*S* was allowed to crash into the obstacle; (2) reward—*S*'s successes were highly praised; (3) withholding reward—when *S*'s final appraisal was more than 3 ft. from the obstacle, praise was omitted, hence he knew that he had done poorly; (4) knowledge of results—after every final appraisal, *S* was led to the obstacle, thus he knew the amount of his error after every trial; (5) avoidance of fatigue and ennui—the trials

<sup>13</sup> The hearing-loss was surprisingly less than that obtained indoors in the first Cornell Study by a similar method of impairment. In that study the loss was 65 db. at all frequency-levels and the Ss could not hear their footsteps nor understand speech unless shouted (Supa, Cotzin, and Dallenbach, *op. cit.*, 169).



during an experimental hour were reduced to 30 and S was given frequent rests; and (6) knowledge of task incomplete—S was frequently informed, particularly during the later part of the experimental hour, of the number of trials still to be made.<sup>14</sup>

**Results: (1) General.** The Ss of both Groups had difficulty at first in approaching the obstacle without the guidance of the edges of the walk. During the early stages of the study they frequently veered in their course and walked onto the grass but with practice this occurred less and less frequently until finally they were able to negotiate it.

Collisions with the obstacle were of three kinds: (1) pre-first-perception collisions, *i.e.* collisions made before S had reported that he had perceived the obstacle; (2) post-first-perception collisions, *i.e.* collisions made after he had reported his first perception but before he had entered upon the

TABLE II

EXPERIMENT I: THE NUMBER OF SERIES REQUIRED BY THE Ss OF EACH GROUP TO REACH CRITERION, THE MEAN DISTANCES AND SD (IN FT.) OF THE FIRST PERCEPTIONS (*p*) AND FINAL APPRAISALS (*a*), THE RATIOS OF THESE DISTANCES (*p/a*), AND THE NUMBER OF COLLISIONS IN THE SERIES IN WHICH CRITERION WAS REACHED

Group A (blindfolded only)							Group B (blindfolded and deafened)								
S	No. of series required for criterion	p		a		p/a	No. of collisions	S	No. of series required for criterion	p		a		p/a	No. of collisions
		M	SD	M	SD					M	SD	M	SD		
1	2	6.61	5.06	1.79	2.21	3.6	4	1	4	10.97	8.19	9.19	8.52	1.1	5
2	2	7.69	8.40	1.41	2.18	5.5	2	2	—	—	—	—	—	—	12*
3	8	1.88	1.34	.33	.38	5.7	5	3	—	—	—	—	—	—	24*
4	3	5.04	5.18	3.38	3.08	1.5	5	4	3	.43	.24	.32	.21	1.3	3
5	2	8.47	7.09	2.40	2.06	3.5	3	5	3	8.92	7.95	5.16	7.32	1.7	5
6	1	11.66	8.16	2.93	2.12	4.1	3	6	8	8.13	7.61	3.26	3.78	2.5	5
7	5	5.03	1.68	1.34	1.43	3.8	5	7	—	—	—	—	—	—	8*
8	5	5.31	4.92	1.83	2.28	2.9	3	8	3	2.90	6.04	1.06	2.37	2.7	5
9	7	7.03	6.74	.68	.86	10.3	5	9	5	14.77	9.44	7.48	8.40	2.3	5
10	5	2.93	3.94	.43	.26	6.8	1	10	—	—	—	—	—	—	19*
Av.	4.0	6.06	5.25	1.65	1.69	6.8	3.6	Av.	4.3	7.68	6.58	4.41	5.10	1.9	4.1
		(86%)		(102%)						(86%)		(138%)			

\* Number of collisions in Series 8.

phase of final appraisal; and (3) final-appraisal collisions, *i.e.* collisions made during the final appraisal while S was attempting to improve his record by 'inching up' to the obstacle which he knew was close before him.

The Ss attaining criterion frequently reported that their judgments were based upon "a change in the sound" of their footsteps and upon the "sudden appearance of a black curtain or shade" before them. These 'dark shades' were experienced only during the 'near approaches' and were often, particularly during the early phases of the study, reported to be the basis of the final appraisals.

<sup>14</sup> These incentives were the same as those used in the second Cornell Study in the learning-experiments with deaf-blind Ss (Worchel and Dallenbach, *op. cit.*, 518).



In every other respect except these just mentioned, the behavior and performance of the *Ss* of the two Groups differed greatly.

(2) *Learning*. As Table II shows, all the *Ss* of Group A (blindfolded only) met our criterion within the trial-limits (8 series of 30 trials each) of the experiment.

For example: one *S* (*A*<sub>6</sub>) reached criterion in Series 1. He possessed, as Table I shows, normal hearing in both ears at the lower audible ranges and superior hearing in both ears at the higher audible ranges. Three *Ss* (*A*<sub>1</sub>, *A*<sub>2</sub>, and *A*<sub>5</sub>) reached criterion in Series 2; one (*A*<sub>4</sub>) in Series 3; three (*A*<sub>7</sub>, *A*<sub>8</sub>, and *A*<sub>10</sub>) in Series 5; one (*A*<sub>9</sub>) in Series 7; and one (*A*<sub>3</sub>) in Series 8. As will be recalled (see Table I), eight *Ss* of this Group possessed normal hearing in at least one ear and two had defective hearing in both ears at the higher audible ranges.

The results of Group B (deafened and blindfolded) present a very different picture. Four of this Group were unable to meet our criterion of learning.

As Table III shows, two of these four (*B*<sub>2</sub> and *B*<sub>7</sub>) improved slightly during the series. They reduced the number of collisions from maxima of 18 and 24 to 12 and

TABLE III  
EXPERIMENT I: NUMBER OF COLLISIONS IN SUCCESSIVE SERIES

Group A (blindfolded only)									Group B (blindfolded and deafened)								
S	Series								S	Series							
	1	2	3	4	5	6	7	8		1	2	3	4	5	6	7	8
1	15	4	(0)	(1)*					1	12	9	9	4				
2	10	2	(2)	(2)					2	10	12	15	13	9	12	11	12
3	27	25	30	23	17	20	8	5	3	30	28	22	22	27	25	22	24
4	13	11	5						4	6	6	3					
5	7	3	(2)	(1)					5	17	6	5					
6	6	(0)	(0)						6	30	25	19	17	19	14	9	5
7	14	15	17	11	5				7	24	23	16	13	20	15	17	8
8	19	21	12	10	3				8	27	22	5					
9	29	24	24	13	23	17	5		9	17	21	14	11	5	(6)		
10	30	23	14	9	1	(2)	(2)		10	30	25	19	23	22	18	20	19

\* Numbers in parentheses are collisions made in series given after criterion had been reached.

8, respectively. The other two (*B*<sub>9</sub> and *B*<sub>10</sub>) made practically no improvement. From maxima of 30 collisions in Series 1, the number of their collisions continued high, falling only to 24 and 19, respectively, in Series 8. The six remaining *Ss* of this Group reached our criterion of learning: *B*<sub>4</sub>, *B*<sub>5</sub>, and *B*<sub>8</sub> in Series 3; *B*<sub>1</sub> in Series 4; *B*<sub>9</sub> in Series 5; and *B*<sub>6</sub> in Series 8. Of these *Ss*, four possessed normal hearing in at least one ear and two (*B*<sub>3</sub> and *B*<sub>7</sub>) had defective hearing, particularly at the higher audible ranges, in both ears.



(3) *Performance in additional series.* To determine whether the Ss had really learned to perceive obstacles or had merely met our criterion by chance, we gave five members of Group A ( $A_1$ ,  $A_2$ ,  $A_5$ ,  $A_6$ , and  $A_{10}$ ) and one of Group B ( $B_9$ ) additional series of trials. All the Ss from Group A were, as shown in Table III, consistent in their performances; once they had met criterion they continued to meet it.

For example:  $A_1$ , who reached criterion in Series 2 with 4 collisions, made 0 and 1 collision in Series 3 and 4, respectively; and  $A_{10}$ , who reached criterion in Series 5 with 1 collision, made 2 and 2 collisions in Series 6 and 7, respectively.

The S from Group B, who reached criterion in Series 5 with 5 collisions, was unable to duplicate his performance in Series 6. Since additional Series were to be given in Experiment 2, further tests of this kind were not made. These results suggest, however, that the depth of learning is not as great among the Ss of Group B as of Group A.

(4) *Course of learning.* The course of learning for the Ss of Group A, as measured by the reduction in the number of collisions, was, by and large, sudden or insightful. It was marked, as Table III shows, by an abrupt drop to criterion.

For example:  $A_1$  collided with the obstacle 15 times in Series 1 and only 4 times in Series 2;  $A_2$  collided 10 and 2 times in the two series with him;  $A_9$  collided 29, 24, 24, 13, 23, 17, and 5 times in the eight series with him; and  $A_{10}$  made 30, 23, 14, 9, and 1 collisions in the five successive series with him.

The Ss apparently, after trying various unreliable cues, hit upon one that enabled them to reduce immediately the number of their collisions. Of the six Ss reaching criterion from Group B, three reached it suddenly and three gradually. For example:  $B_8$ , a sudden learner, made 27, 22, and 5 collisions in successive series;  $B_6$ , a slow learner, made 30, 25, 19, 17, 19, 14, 9, and 5.

(5) *Judgments.* The Ss of Group A not only met our criterion of learning but their performances, both in the series in which they first met it and in the series thereafter given them, indicated that they were basing their judgment upon the obstacle. As Table II shows, their first perceptions and final appraisals differed from each other by considerable amounts. Their performance-ratios ( $p/a$ ) average  $4.5 \pm 1.86$  with individual ratios varying from 1.5 to 10.3, which are values very like those obtained from normal, blindfolded Ss in the earlier studies.<sup>15</sup> These values are very different, however, from those obtained by the Ss meeting criterion from

<sup>15</sup> Cf. Supa, Cotzin, and Dallenbach, *op. cit.*, 153; Cotzin and Dallenbach, *op. cit.*, 494 f.



Group B whose ratios averaged  $1.9 \pm 0.56$  with individual ratios varying from 1.1 to 2.7.

(6) *Collisions*. For the Ss of Group A, the number of collisions of the first and second types—pre- and post-first-perception collisions—decreased with learning until finally, when criterion had been reached, collisions of the third type—final-appraisal collisions—were the only ones being made. These collisions occurred when S was attempting to improve his performance; when he was inching up to the obstacle which he knew was immediately before him. These cases were recorded as 'collisions' but in reality they were not 'failures' because the Ss were aware, as indicated by their behavior and later reports, of the presence and nearness of the obstacle. The collisions of the Ss of Group B after they had reached criterion were, on the contrary, chiefly of the second type, *i.e.* post-first-perception collisions. After detecting the obstacle in their 'near approaches' they rarely risked collisions by attempting to better their positions.

(7) *Standard deviations*. The SD of the first perceptions and final appraisals are large for the Ss of both Groups, being 86% and 102% of the means of these performances respectively for Group A and 86% and 138% respectively for Group B. That the SD should be large is not surprising as the conditions under which the Ss of both Groups served were highly complex and variable. That they should be larger for Group B than for Group A is due in part, as we believe, to the dependence of the Ss of Group B upon more variable and fluctuating cues which resulted in 'good' performances when present and 'poor' performances when absent; and in part to a limitation of the distance that they walked in rendering their judgments—a point discussed below (p. 529).

*Discussion*. The Ss of neither Group learned as rapidly as the Ss in the Cornell studies who served indoors, and their performances (both first perceptions and final appraisals) were more variable, as shown by the size of the SD, than those of the Cornell Ss.<sup>16</sup> Delay in learning and greater variability in performance are both due, as we believe, to the greater complexity of conditions out of doors. The ambient noises of the experimental area partially masked and at times totally obscured the sounds of S's footsteps. Learning to perceive obstacles would, therefore, be delayed as more trials would be required under these unfavorable conditions for S to discover and to utilize the auditory cues necessary for the perception of obstacles than under the relatively noiseless conditions indoors in the laboratory. The adventitious noises of the experimental area out of doors, such as the

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<sup>16</sup> *Idem*.



sounds of pneumatic hammers from nearby construction, totally obliterated at times the cues upon which *S* based his judgments. If, therefore, the noise-level was high when *S* reached a critical point in a trial, his performance would be worse than usual; if, on the other hand, the noise-level chanced to be low, his performance would be better than usual. Chance variations in noise-level, such as our *Ss* experienced, would necessarily result in large variations in performance—such as our *Ss* yielded.

The problem of learning to perceive obstacles out of doors was further complicated by the wind, the sun, and the clouds. These agencies produced cues at times that were used as the basis of our *Ss*' judgments—especially those of Group B. On occasions when there was a wind and *S* walked with or against it, he was made aware of the obstacle by changes in pressure against his face. When he walked into the wind, he could tell he was coming close to the obstacle by a drop in the pressure as the obstacle acted as a shield. When he walked with the wind, he could tell that the obstacle was near by the air currents reflected from it. When the wind died down, or blew across *S*'s path, these cues were, of course, absent and performance based upon them suffered.

When the sun shone hot, the obstacle was detected by temperature changes; either by a drop when *S* walked into its shadow, or by a rise when the sun shone upon it and its heat was reflected to *S*'s face as he neared it. The sun also yielded odors by means of which the obstacle could be detected. Though the Masonite board was chosen in the first Cornell study because it was odorless indoors,<sup>17</sup> it gave off a distinct odor out of doors in the hot sun. *S* could tell by the sense of smell alone when he was near the obstacle. On a cloudy day, or when the sun receded behind a cloud, the temperature cues were lacking. The olfactory cues were also lacking on a cloudy day, but the recess of the sun behind a cloud did not immediately bring about a cessation of the odor or the heat of the board. Those cues lingered and were effective as long as the board retained its heat.

Any of these cues was at times sufficient for the perception of the obstacle and all, particularly during the early stages of learning, were used by the *Ss*. Like the proverbial drowning man and the straw, our *Ss* grasped at any and every cue that would serve them.<sup>18</sup> Though the cues from the wind and sun were at times sufficient, they were neither necessary nor always present. That they were used when available accounts, in part at least, for

<sup>17</sup> *Op. cit.*, 140 (footnote 24).

<sup>18</sup> For other 'straws,' see Supa, Cotzin, and Dallenbach, *op. cit.*, 138 (footnote 22), 149 (footnote 29), and 159.



our Ss delay in learning—*i.e.* in discovering and utilizing cues (auditory) that were sufficient under most conditions—and for the large variability, shown in Table II, in their performances.

The Ss of both Groups knew that an obstacle was in the experimental path. It was at every trial; there never was an exception in Experiment 1. If *S* had not collided with it before giving his final appraisal, he was led up to it that he might know how far he was away from it. In every trial, therefore, the presence of the obstacle was confirmed. Though encouraged to approach the obstacle "as closely as possible" in his final appraisal, an *S*, even though he lacked a reliable cue, would soon learn that a 'far' performance not only escaped the punishment of a collision but that it also equalled a 'close' performance in being counted as a 'success.' If an *S* were, therefore, to walk 8–10 ft., raise his right arm, signifying his first perception, and then advance a little less than 4–2 ft. and raise his left arm, signifying his final appraisal, his first perceptions would average  $10.5 \pm 6.5$  ft.; his final appraisals,  $8.5 \pm 6.5$  ft.; his performance-ratio would be 1.2; and he would collide with the obstacle only 6 times—results that are very similar to those of *B*<sub>1</sub> who met our criterion of learning with the following performance: average first perceptions,  $10.97 \pm 8.17$ ; average final appraisals,  $9.19 \pm 8.52$ ; performance-ratio, 1.1; and collisions 5.

*Conclusions.* The results of Experiment 1 indicate (1) that blindfolded Ss possessing normal or near normal hearing (Group A) are able to acquire the ability to perceive obstacles under the complex and varying conditions met out of doors; and (2) that some blindfolded Ss with impaired hearing (six members of Group B) are able, by methods undetermined, to avoid collisions with the obstacle.

We cannot, from the results at hand, determine the basis of the performances of the Ss who met our criterion. Experiment 1 was an experiment in learning. We could not introduce any condition that would interfere with the process of learning. Now, however, that we know that Ss of both groups learned something, we can safely set conditions to discover what they learned and what cues they employed. Experiment 2 was the first of several undertaken for this purpose.

## EXPERIMENT 2

The object of Experiment 2 was to determine what precisely the Ss had learned in Experiment 1. We wished to discover whether they had really learned to perceive the obstacle, *i.e.* had based their performances upon cues derived from it, or had merely learned to avoid collision by restricting,



consciously or unconsciously, the distances they walked in making their reports.

*Procedure.* In every respect but two the procedure was the same as in Experiment 1. The new conditions were the following: (1) check trials (Vexirversuchen), in which there was no obstacle in the experimental path, were introduced into the experimental series; and (2) *S*, unless he collided with the obstacle, served without knowledge of his results, as he was given no information about his performances and was not permitted to extend his hand to confirm the accuracy of his final appraisals.

Since this experiment was a test to determine what was learned and not to aid learning, only one series of 30 trials was given each of the *Ss*. This series consisted of 20 trials in which the obstacle (the one used in Experiment 1) was placed in the experimental path and 10 trials in which it was not present. The obstacle-distances and the starting points were the same as in Experiment 1.

The check trials were randomly distributed through the obstacle-series. None of the *Ss* knew that they were to be given. When used, *S* was permitted to walk the entire length of the experimental path (60 ft.) if he did not report a final appraisal before reaching that point. From the end of the path, *S* was led without comment back to the starting-point for the next trial—just as was done when he gave his final appraisals or had collided with the obstacle.

*Subjects.* All the *Ss* of Groups A and B who met our criterion of learning in Experiment 1, and *B*<sub>2</sub> and *B*<sub>7</sub> who showed progressive improvement, served in Experiment 2. *B*<sub>3</sub> and *B*<sub>10</sub> were excused from this part of the study because their results gave no evidence of learning of any kind.

*Instructions.* Since *S* was no longer to be led to the obstacle after his final appraisals and would not be permitted to extend his arms to test the accuracy of his judgments, the instructions were so modified that he would not suspect the purpose of the change in procedure. They were as follows.

We believe that you have learned to perceive obstacles. Now, after raising your left hand indicating that you are as close as you possibly can come to the obstacle without touching it, you will no longer be led up to it. Do not reach out to verify your judgment after you had given it because in this Series we wish to determine how closely you can come to the obstacle without knowledge of your results.

*Results: (1) Group A.* The results of Experiment 2, given in Table IV, show clearly that the judgments of the *Ss* of Group A were based upon cues derived from the obstacle. Five of them (*A*<sub>2</sub>, *A*<sub>3</sub>, *A*<sub>7</sub>, *A*<sub>9</sub>, and *A*<sub>10</sub>) made no errors in the check trials, *i.e.* they did not report the obstacle when it was not there;<sup>19</sup> two (*A*<sub>1</sub> and *A*<sub>5</sub>) made only one, and three (*A*<sub>4</sub>, *A*<sub>6</sub>, and *A*<sub>8</sub>) made but two. The *Ss* were under the instruction to perceive an obstacle which they had learned in the previous experiment would always be in the path. That only half of them reported it under those cir-

<sup>19</sup> In this respect their performances out of doors equalled those of the Cornell *Ss* indoors, see Supa, Cotzin, and Dallenbach, *op. cit.*, 154, 158.



cumstances, and that they reported it only once or twice out of 10 check trials, is indication that their perception of obstacles is so compulsory that it cannot easily be replaced by imaginal components.

Their collisions, which varied in number from 1 to 5 (four Ss made 1 each, five made 3 each, and one made 5), were all of the final-appraisal type, *i.e.* were made while attempting to better their records, hence their collisions in no way detract from the high quality of their performances. It is true, as a comparison of Tables II and IV reveals, that their per-

TABLE IV

EXPERIMENT 2: MEAN DISTANCES AND SD (IN FT.) OF THE FIRST PERCEPTIONS (*p*) AND FINAL APPRAISALS (*a*), THE RATIOS OF THESE DISTANCES (*p/a*), THE NUMBER OF COLLISIONS, AND THE NUMBER OF TIMES OBSTACLES WERE REPORTED IN THE CHECK TRIALS (FALSE REPORTS)

Group A (blindfolded only)								Group B (blindfolded and deafened)							
S	<i>p</i>		<i>a</i>		<i>p/a</i>	No. of collisions	No. of false reports	S	<i>p</i>		<i>a</i>		<i>p/a</i>	No. of collisions	No. of false reports
	M	SD	M	SD					M	SD	M	SD			
1	3.13	5.02	2.31	3.24	1.3	1	1	1	10.28	7.90	6.99	7.62	1.5	3	10
2	5.73	5.56	1.98	1.31	2.8	1	0	2	3.44	7.16	2.30	5.31	1.5	10	1
3	—	—	1.64*	2.06*	—	3	0	3	—	—	—	—	—	—	—
4	5.58	6.70	3.60	5.36	1.6	3	2	4	—	—	1.22*	2.33*	—	3	0
5	10.07	9.27	6.13	5.48	1.6	3	1	5	6.46	8.80	4.64	7.50	1.4	1	5
6	4.36	7.05	1.67	3.20	2.6	1	2	6	6.71	6.21	1.70	2.19	3.9	15	1
7	10.07	9.27	6.13	5.48	1.6	5	0	7	6.25	3.46	2.14	1.88	2.9	13	4
8	9.47	7.26	4.18	6.09	2.3	3	2	8	2.66	.99	.38	.31	7.0	9	0
9	3.07	3.06	1.98	.85	1.6	3	0	9	10.72	8.72	8.56	7.92	1.3	4	10
10	2.70	.69	.48	.58	5.6	1	0	10	—	—	—	—	—	—	—
Average	6.02	6.09	3.34	3.74	2.3	2.4	0.8	Average	6.65	6.18	3.81	4.81	2.6	7.2	3.9
	(101%)		(112%)						(93%)		(126%)				

\* Results not included in the averages because first perceptions were not reported.

formances (means and SD of their first perceptions and final appraisals) were poorer than in the criterion-series of Experiment 1, but the differences are not great and are due, as we believe, to the fact that they were working without knowledge of their results.

One of these Ss, A<sub>3</sub>, who made no false reports and had only three collisions, stopped his approach at the first perception, or rather his first perception was also his final appraisal. The average differences between these reports in Experiment 1, where the instructions called for two reports in every trial, was only a little over a foot—about half a step (see Table II). In the test-series we permitted S freedom in report and A<sub>3</sub> chose to give only one judgment in every trial.

(2) *Group B.* The Ss of Group B present again a very different picture. Two of them (B<sub>1</sub> and B<sub>9</sub>) failed utterly. They reported first perceptions and final appraisals in every one of the check trials. Though they collided with the obstacle only 3 and 4 times, respectively, it is clear that they had merely learned in Experiment 1 to avoid collisions—not to perceive obstacles. When questioned at the conclusion of the test-series, neither was



able to describe the basis of his judgments. The means and *SD* of their performances suggest, however, that they were merely limiting the distances walked in approaching the obstacle. We do not believe that they did this consciously as a planned procedure but rather that they arrived at it by trial-and-error as a means of escaping the punishment of the collisions.

Two members of this Group ( $B_4$  and  $B_8$ ), on the other hand, made no false reports in the check trials. Though they collided with the obstacle 3 and 9 times, respectively, the fact that they did not report the obstacle when it was not present is evidence that their judgments were based upon it. Their collisions, moreover, were chiefly of the final-appraisal type. Indeed,  $B_4$ , like  $A_8$ , refused in this experiment to report first perceptions—or rather, her first perceptions were her final appraisals. In Experiment 1, in which she was required to give two reports in every trial, her first perceptions (see Table II) were given only a few inches before her final appraisals.

As Table I shows,  $B_4$  possessed normal hearing in both ears and  $B_8$  had normal hearing in his left ear except at the highest audible range tested (11,584 ~). Both of these *Ss* were, moreover, among those of Group B reporting that they could at times hear their footsteps despite our efforts to deafen them. How these two *Ss* detected the obstacle, we cannot definitely say, but that they had learned to detect it is clearly demonstrated, as we believe, by their result in this Experiment.

The results of the *Ss* we have thus far considered are definite and decisive; now we come to *Ss* whose results are ambiguous.  $B_2$  and  $B_6$  reported the obstacle once each when it was not present—nine times they avoided failure in the check trials. One failure was not regarded as sufficient to question the ability of the *Ss* of Group A, hence the ability of  $B_2$  and  $B_6$  would not be questioned if it were not for the fact that they made 10 and 15 collisions, respectively, in the 20 trials with the obstacle. Such large percentages of collisions (50 and 75, respectively) are hardly indicative of the 'obstacle sense.' Their performances (means of their first perceptions and final appraisals), despite their large variations (*SD*), do not, however, appear to be dictated by chance nor by a limitation of the distances walked during the trials. They seem, rather, to be the result of variable cues (wind and temperature reflections from the obstacle, odors derived from it, shadows cast by it, and sounds echoed from it during fortuitous drops in the level of the ambient noises) which were adequate in varying degrees when conditions permitted them to be noticed. If such were the case, results like those given by these *Ss* would be obtained. Every time these cues were absent, *S* would continue walking until he col-



lided with the obstacle or reached the end of the experimental path. This would mean, if these cues were frequently lacking, a large number of collisions and few errors in the check trials.  $B_4$  and  $B_6$  must, therefore, as we believe, be credited with learning to perceive obstacles—at least at a low level, but low only because their hearing was obstructed. If their hearing were unobstructed, they would, we predict, be facile in the perception of obstacles.

The records of the two remaining Ss of Group B ( $B_5$  and  $B_7$ ) are still more difficult to interpret.  $B_5$  made one collision and five errors in the check trials. The errors in the check trials (50%) suggest chance but his performances in the trials in which the obstacle was present negates that conclusion. The mean of his first perceptions is good but the means of his final appraisals and his performance ratio ( $p/a = 1.4$ ) are poor. The SD of the means of his performances are high (being 136% and 162%, respectively) and suggest that the cues upon which his performances were based were not dependable.  $B_5$  possessed normal hearing in one ear (see Table I), hence it may well be that he, like  $B_2$  and  $B_6$ , reacted to cues that were so highly variable and weak that they were easily imagined. Upon the basis of these assumptions, we may credit  $B_5$  as being among those in Group B who learned to perceive obstacles, though at an extremely low level.

$B_7$  made 13 collisions and 4 errors in the check trials, just about what would be expected by chance. His results indicate that he failed in learning to perceive obstacles and, furthermore, that he also failed, as his numerous collisions (65%) attest, in learning to avoid them. He was, as will be recalled, one of the two Ss included in this experiment who failed to meet our criterion of learning in Experiment 1.  $B_2$ , the other S included under like conditions, demonstrated, as shown above, that he had learned something, but  $B_7$  cannot be credited with learning anything other than the sheer mechanics of the experiment. He must, therefore, be classified with  $B_3$  and  $B_{10}$  who gave no evidence in Experiment 1 of learning anything about obstacle-perception.

*Summary and conclusions.* As these results show, all the Ss of Group A and half of those of Group B ( $B_2$ ,  $B_4$ ,  $B_5$ ,  $B_6$ , and  $B_8$ ) demonstrated that they had learned to perceive obstacles, *i.e.* to localize them from cues derived from them. Not all of the Ss meeting our criterion of learning in Experiment 1 were able, however, to do this. Two from Group B ( $B_1$  and  $B_9$ , see Table II), who had met criterion were not able in Experiment 2 to differentiate between the check and the obstacle-trials. They had not learned to perceive the obstacle but had merely learned, as Worchel and Dallenbach's deaf-blind Ss had done, to avoid collisions by limiting the distances they



walked.<sup>20</sup> Contrariwise, one *S* (*B*<sub>2</sub>), who failed to meet our criterion in Experiment 1, demonstrated that he had acquired the ability by distinguishing between the presence and absence of the obstacle.

These results demonstrate the necessity of check trials in experiments of this nature in determining who learned and what they learned. Now that we have the answers to these questions, we may turn to discovering what cues were used by the *Ss* as the basis of their judgments.

### EXPERIMENT 3

Most of the *Ss* of both Groups had at one time or another during the preceding experiments mentioned that they detected the obstacle—a Masonite board—by its odor when they came close to it. Though this board was originally selected as the obstacle because it was odorless indoors,<sup>21</sup> it did, as mentioned above (p. 528), give off a distinct odor when it stood in the heat of the sun. Experiment 3 was undertaken to determine the rôle played by smell. If an *S* detected the obstacle by smell, then the elimination of that modality of sense should immediately show itself in an increase in the number of his collisions, or in the distance of his final appraisals, or both.

*Method and procedure.* Holding all other conditions of Experiment 2 constant, smell was eliminated by plugging *S*'s nostrils. Cotton wool was inserted in *S*'s anterior nares and covered and held in place by strips of adhesive tape. If any odor penetrated the plugs it was the constant odor of the tape. The experiment consisted of one series of 30 trials—10 check trials randomly distributed among 20 obstacle-trials.

*Subjects.* Only three *Ss* (*A*<sub>9</sub>, *A*<sub>10</sub>, and *B*<sub>8</sub>), who mentioned the odor of the board most frequently, were used. All of them had met criterion in Experiment 1 and all had demonstrated in Experiment 2 that they perceived and reacted to the obstacle.

*Results and conclusions.* The nasal plugs did not adversely affect the performances of any of the *Ss*. Their performances equalled those in Experiment 2 in which the olfactory cues were available to them. *A*<sub>10</sub>, for example, gave the following results:  $p = 2.50 \pm 0.86$ ;  $a = 0.66 \pm 0.67$ ;  $p/a = 3.8$ ; 3 collisions; and 0 false reports. A comparison of these results with those given by him in Experiment 2 (see Table IV) reveals no significant differences. The records are of a kind. As with *A*<sub>10</sub>, so also with the other *Ss*. Both hearing and deafened *Ss* seemed to be unaffected in their performances by the loss of smell.

By the time of this experiment, all the *Ss* were highly practiced and

<sup>20</sup> *Op. cit.*, 523.

<sup>21</sup> Supa, Cotzin, and Dallenbach, *op. cit.*, 60 (footnote 24).



proficient in the perception of obstacles. It may well be, therefore, that smell, though significant in the early stages of learning, had been replaced at our Ss' level of training by more dependable cues that were always present and always available. However this may be, the results of this experiment lead us to conclude that odor is not a necessary condition for the perception of obstacles, though at times it may be sufficient—particularly during the early stages of learning before more subtle and reliable cues have been discriminated and assimilated. Few obstacles yield an odor—our Masonite board did only now and then when under the heat of the sun—hence an S, if he acquired the ability to perceive obstacles, must discover and rely on cues that are more universal and dependable.

#### EXPERIMENT 4

Experiment 4 was a repetition of Experiment 2 under the darkness of night. It was undertaken to eliminate various factors that seemed to be inherent in the experiments conducted during the day. These factors, *i.e.* the experience of 'blackness' and temperature and olfactory changes, were reported by all the Ss, who learned to perceive obstacles, as cues of their final appraisals (see p. 524).

The 'blacknesses' reported may be visual experiences derived from the sun. Though we found that our blindfolds were lightproof when tested in the laboratory, the intensity of the light used (a 200-w. lamp) is hardly comparable with that of the sun, hence it is possible that the Ss were discriminating visually when they passed from the light of the sun into the deep shadow of the obstacle in their near approach of it. Temperature changes—an increase by reflection or convection from the obstacle, or a decrease when S stepped into the obstacle's shadow—and the smell of the board are cues that also derive from the sun.

All of these factors were immediately eliminated by conducting the experiments at night. In addition, night work reduced the intensity of the ambient noises because neighboring construction had temporarily ceased and traffic in the street and through the campus had greatly lessened. By working at night and observing the effect upon S's performances, we hoped to be able to evaluate the significance of these factors in the perception of obstacles. If there is a decrement in S's performances, their value will be demonstrated; if there is no significant difference in performance, little or no value may be attached to them; and if there is an increment in performance, their insignificance, in comparison with the auditory cues which emerge with the reduction of the surrounding noise-level, will be revealed.

*Procedure.* The procedure was the same as that of Experiment 2 with the exception that the series of 30 trials (10 check interspersed among 20 obstacle-trials) was



conducted between 8-10 P.M. Though this series was conducted during December, there was sufficient light at the experimental area for the *Es*, after being dark adapted, to discern the starting-points, the obstacle-placements, and the distances traversed by the *Ss* without the aid of special illumination.

**Subjects.** All the *Ss* of Group A, except *As*, and six *Ss* of Group B served in this experiment. Of the six *Ss* of Group B, four demonstrated in Experiment 2 that they had learned to perceive obstacles; one (*B<sub>7</sub>*), that his judgments were chance; and one (*B<sub>9</sub>*) that he had learned merely to avoid obstacles, not to perceive them. *B<sub>7</sub>* and *B<sub>9</sub>* were included in the hope that we might be able to discover the cause of their previous failures.

**Instructions.** The instructions were the same as in Experiment 1 except for the addition of the following sentence: "We wish to see how well you can perceive obstacles at night."

**Results: (1) Group A.** The results of this experiment are given in Table V. As a comparison of this table with Table IV reveals, all the *Ss* of Group

TABLE V

EXPERIMENT 4: THE MEAN DISTANCES AND SD (IN FT.) OF THE FIRST PERCEPTIONS (*p*) AND FINAL APPRAISALS (*a*), THE RATIOS OF THESE DISTANCES (*p/a*), THE NUMBER OF COLLISIONS, AND THE NUMBER OF TIMES OBSTACLES WERE REPORTED IN THE CHECK TRIALS (FALSE REPORTS)

Group A (blindfolded only)							Group B (blindfolded and deafened)								
S	p		a		p/a	No. of collisions	No. of false reports	S	p		a		p/a	No. of collisions	No. of false reports
	M	SD	M	SD					M	SD					
1	1.79	2.82	.54	.74	3.3	2	0	2	6.76	5.42	1.71	2.34	3.9	9	2
2	3.00	2.41	.68	.78	4.4	0	2	4	.97	1.84	.87	.84	1.1	11	0
3	2.00	1.90	.29	.24	7.1	5	0	6	16.65	9.39	11.00	6.35	1.5	14	10
4	3.11	2.76	.31	.16	10.0	5	2	7	10.71	6.66	8.56	6.51	1.3	10	5
5	5.16	4.63	.63	1.62	8.5	5	0	8	2.63	3.22	1.00	.85	2.6	2	1
6	1.83	2.83	.48	.49	3.8	3	1	9	12.46	8.67	5.79	6.59	2.1	5	2
7	4.94	2.22	2.46	1.31	2.0	5	0								
9	2.70	3.06	.43	.45	6.3	5	0								
10	4.76	3.99	.56	.53	8.5	0	0								
Av.	3.08 (96%)	2.96	0.71 (104%)	0.74	6.0 3.3	0.55		Av.	8.36 (71%)	5.87	4.84 (81%)	3.91	2.1 8.5	3.33	

A improved their performances at night. Their performance-ratios are larger (averaging 6.0 as against 4.5), the means of their 'final appraisals' are much smaller (being for 8 of the 9 *Ss* between 3-8 in.),<sup>22</sup> and their records in the check trials are better (six *Ss* made no errors, one made one, and two made two). Their collisions, though slightly more numerous (averaging 3.3 against 2.4) were all of the third type, *i.e.* made during the 'final appraisal'.<sup>23</sup>

<sup>22</sup> These results are much like those of the Cornell studies in which the final appraisals were inches away from the obstacle. Cf. Supa, Cotzin, and Dallenbach, *op. cit.*, 143, 150, 153; Cotzin and Dallenbach, *op. cit.*

<sup>23</sup> Indeed some of the *Ss*, because they had not been explicitly forbidden to do so, reached out and touched the board after making their final appraisal to discover how far they were away from it. These cases were counted as collisions. Though these



The conditions at night were beneficial to the Ss of Group A. The accuracy of their judgments was unaffected by the loss of the cues dependent upon the sun—unless the elimination of their distracting influence be regarded as a contributing factor. The improvement made by all of these Ss is primarily due, as we believe, to the reduction of the intensive level of the ambient noises which permitted more accurate discrimination among the auditory stimuli.

(2) *Group B.* The picture is again very different for the Ss of Group B. The conditions at night variously affected them. The performances of two ( $B_6$  and  $B_7$ ) suffered a marked decrement. Their performance ratios ( $p/a$ ) were smaller than during the day, falling to 1.3 and 1.5 from 2.9 and 3.9, respectively; their final appraisals were much greater, being from 4 to 6 times as large; and the number of their collisions and false reports being altered, by and large, for the worse.  $B_6$  was counted, upon the basis of his results in Experiment 2, as being among those demonstrating that they had learned to perceive obstacles, *i.e.* his judgments were based upon cues derived from the obstacle (see p. 525). With 14 collisions and 10 false reports in the present experiment, he clearly demonstrated his dependency upon the day-time cues—without them he failed utterly. The results of the second of these S ( $B_7$ ) confirm our previous judgment; namely, that he had learned nothing of the perception of obstacles (p. 533). His records in Experiment 2 and here denote failure, hence the decrement here is probably due to chance variations and not to the loss of cues that proved to be of no value to him in Experiment 2.

Two members of Group B ( $B_4$  and  $B_8$ ) gave results that varied but little from those in the day-time trials, hence we may safely assume that the cues available during the day were of slight if of any significance to them. The only cues available in this Experiment were wind pressures and sounds. Since wind pressures are fortuitous and the Ss' performances were not, we can only conclude that the Ss were reacting to auditory cues in Experiment 2 as well as in this Experiment. That there was no change in their results follows from the fact that there was for them no change in the experimental conditions.

The two remaining Ss of Group B ( $B_2$  and  $B_9$ ) bettered their performances;  $B_9$  slightly and  $B_2$  markedly (cf. Tables IV and V).  $B_9$  was one of the Ss meeting criterion in Experiment 1 who demonstrated in Experiment 2 that he had learned nothing about the perception of obstacles but merely

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Ss were told that they would be charged with a collision if they touched the obstacle, some were willing to suffer that demerit in return for the knowledge that they gained from it. Knowledge of results is an important factor in the acquisition of this ability.



how to avoid collisions. He showed here, however—by increasing his performance ratio to 2.1 from 1.3 and reducing his false reports to 2 from 10—that he was beginning to learn and that under the more favorable conditions at night reliable cues of the obstacle were available.  $B_2$  on the other hand showed a marked improvement. His performance-ratio was increased from 1.5 to 3.9 and, except for the large number of collisions, his results are more like those of Group A than of Group B. This result is surprising as the analysis of his results in Experiment 2 revealed that he was responding in that study to the very cues that were eliminated in this one (see p. 533).  $B_2$  had normal hearing, however, in both ears; it is probable, therefore, that the reduction in the noise-level at night permitted him for the first time to notice and to use the auditory cues. Since most of his collisions occurred during the early trials and he shuffled his feet along the walk and commented upon the changes in their sounds more and more as the experiment progressed, the explanation of his results in terms of his discovery and utilization of auditory cues has a high degree of plausibility. However this may be, it is certain that his performance in Experiment 4 did not suffer because of the lack of the day-time cues upon which he seemed to depend in Experiment 2.

(3) *Verbal reports.* Some of the Ss of Group A again reported experiences of 'blackness' ("a black curtain," "a dark shade," etc.) when they neared the obstacle. Since there was no possibility of visual stimulation under the conditions of this Experiment, these experiences must be regarded as imaginal and as being aroused associatively by cues which marked the presence of the obstacle. These cues were probably auditory, at least in the present Experiment, as many of the day-time cues were eliminated and audition was enhanced by the reduction in the surrounding noise-level.

All the Ss of Group A, and some of Group B who could hear the sound of their feet as they shuffled them along the sidewalk, commented upon the changes in sound of their footsteps as they approached the obstacle. For many this was an old story but for some it was new. For all, however, Experiment 4 offered the best opportunity, by virtue of the reduction of the surrounding noise-level, for the observance of the auditory cues.

*Summary and conclusions.* As these results show, only one S ( $B_6$ ), who had demonstrated his ability to perceive obstacles, suffered a decrement in his performance under night conditions. All the Ss of Group A and two of Group B bettered their performances considerably; the remaining Ss showed little or no change. Since the elimination of the day-time cues was accompanied by a reduction in the noise-level of the experimental area, the improvement in Ss' records is due, as we believe, to the increased effectiveness



of the auditory cues. These results show the importance of audition and indicate that thermal and olfactory cues, though sufficient under some conditions, are not necessary for the perception of obstacles.

Since "black curtains" and "dark shades" were again reported by the hearing Ss (Group A) under conditions impossible for visual stimulation, we conclude that these experiences are imaginal, being aroused associatively by auditory cues which mark the presence of the obstacle.

## PART II

Part II is a repetition of Part I with an interchange of the experimental conditions between the two Groups of Ss. The Ss of Group A were now blindfolded and deafened; those of Group B were blindfolded only. Short of doing this we could not be certain that the differences between the results of the two Groups in Part I were due to the experimental conditions and not to individual differences. By interchanging the experimental conditions, each Group served as its own control.

### EXPERIMENT 5

Experiment 5 was a repetition of Experiment 1; an experiment in learning. The procedure, instructions, and Ss of the two Groups were the same with the single exception, mentioned above, that the experimental conditions under which the two Groups served were interchanged. We repeated this experiment to teach the Ss to perceive obstacles under the new conditions and also to see whether they would learn more or less rapidly, because of their previous training, than the comparable Group in Experiment 1. What would be the effect of giving Ss hearing who had previously been deprived of it; and, conversely, what would be the effect of depriving Ss of hearing who had previously been accustomed to it?

*Results: (1) Group A.* With one exception, all the Ss of Group A, now the deafened group, learned under the new conditions to perceive the obstacle (see Table VI). The immediate effect of impairing their hearing was a great increase in the number of their collisions but learning proceeded rapidly and within three series all but one S, A<sub>9</sub>, had attained criterion (25 successes in 30 trials, *i.e.* five or fewer collisions).

The single exception, A<sub>9</sub>, was unable to learn within the trial-limits of this study as he collided with the obstacle 15 times in the final series and from 13 to 15 times in the preceding series. As Tables II and III show, he was a slow learner, requiring 7 series to reach criterion in Experiment 1. This was possibly due to the fact that he had a 50-db. loss in both ears at 11,584  $\sim$  (see Table I) and required, because



of it, more practice than the other Ss to meet criterion. Once it was met, however, he was a good performer as his records in the other experiments of Part I clearly indicate. His inability to meet criterion in the present experiment is due, as we believe, to the successful impairment of his hearing. With ears plugged, his threshold was 65 db. at 8192 ~ and 90 db. at 11,954 ~. Since the noise-level of the experimental area varied between 30-70 db., he was without doubt totally deaf to these higher frequencies—the very ones which are, according to Cotzin and Dallenbach, responsible for the perception of obstacles.<sup>24</sup>

The performances of all the other Ss of Group A in the criterion-series were good. As a comparison of Tables II and VI reveals, they are almost as

TABLE VI

EXPERIMENT 5: THE NUMBER OF SERIES REQUIRED BY THE Ss OF EACH GROUP TO REACH CRITERION, THE MEAN DISTANCES AND SD (IN FT.) OF THE FIRST PERCEPTIONS (p) AND FINAL APPRAISALS (a), THE RATIOS OF THESE DISTANCES (p/a), AND THE NUMBER OF COLLISIONS IN THE SERIES IN WHICH CRITERION WAS REACHED

Group A (blindfolded and deafened)								Group B (blindfolded only)							
S	No. of series required for criterion	p		a		p/a	No. of collisions	S	No. of series required for criterion	p		a		p/a	No. of collisions
		M	SD	M	SD					M	SD	M	SD		
1	3	1.93	3.40	.28	.15	6.9	2	1	1	6.16	5.22	2.97	3.89	2.1	2
2	1	3.37	6.34	1.11	3.91	3.0	3	2	1	5.68	4.49	.96	1.81	5.9	2
3	2	2.66	3.81	.62	1.70	4.1	4	3	4	6.18	6.49	1.52	1.48	4.0	5
4	3	6.15	7.83	1.46	1.74	4.2	4	4	1	—	—	1.18*	1.02*	—	3
5	2	3.61	6.74	1.48	2.88	2.0	3	5	1	6.70	6.04	3.29	4.02	2.0	5
6	3	1.89	.96	.33	.65	5.8	2	6	2	6.45	7.61	1.30	4.55	4.9	5
7	2	5.06	1.82	1.66	1.79	3.6	3	7	1	4.00	4.39	2.46	3.79	1.6	3
8	3	11.07	7.51	4.34	6.14	2.5	5	8	1	3.04	1.50	1.26	.98	2.4	0
9	—	—	—	—	—	—	15*	9	1	3.40	4.87	2.58	4.30	1.3	2
10	2	2.64	4.32	.62	2.14	4.2	3	10	4	7.72	6.96	1.43	3.11	5.4	5
Av.	2.33	4.36	4.75	1.32	2.34	4.0	3.2	Av.	1.7	5.48	5.28	1.97	3.10	3.3	3.2
		(109%)		(177%)						(96%)		(157%)			

\* Results not included in the averages.

good as when hearing was unimpaired. Criterion was met in fewer series (averaging 2.33 against 4.0); the performance-ratios are almost as high (averaging 4.0 against 4.5); and the number of collisions in the criterion-series is fewer (averaging 3.2 against 3.6). These results considered alone suggest that impairment of hearing interfered but little, if at all, with the ability of these Ss to perceive obstacles; but considered alone they are misleading. They merely show that the Ss, despite the impairment of their hearing, had regained their former ability.

The immediate effect of the impairment of hearing was, as we observed above, to increase greatly the number of collisions. From relatively few collisions, the Ss made many as soon as hearing was impaired (see Series 1, Table VII). That they were able with very little practice to reduce the

<sup>24</sup> Cotzin and Dallenbach, *op. cit.*, 507, 512 ff.



number of collisions to criterion indicates that they either found other cues or were successful in reinterpreting the auditory cues still available. We are inclined to the latter view because all the Ss of this group shuffled their feet along the sidewalk during their approaches—some rather vigorously<sup>25</sup>—which none of them had done before their hearing was impaired.

(2) *Group B.* All of the Ss of Group B met criterion very quickly under the new conditions (see Table VII). Of the six Ss attaining it in Experiment 1, five ( $B_1$ ,  $B_4$ ,  $B_5$ ,  $B_8$ , and  $B_9$ ) attained it again in this experiment in Series 1 and one ( $B_6$ ) in Series 2. Of the four Ss failing to reach cri-

TABLE VII  
EXPERIMENT 5: NUMBER OF COLLISIONS IN SUCCESSIVE SERIES

Group A (blindfolded and deafened)					Group B (blindfolded only)				
S	Series				S	Series			
	1	2	3	8		1	2	3	4
1	16	14	2		1		(2)		
2	3	(1)*	(1)		2	2	(1)		
3	15	4			3	18	14	13	5
4	9	7	4		4	3	1		
5	6	3			5	5	(5)		
6	7	11	2		6	15	5		
7	21	3			7	3			
8	12	6	5		8	0	(2)		
9	15	13	15	15	9	2			
10	10	3			10	17	10	10	5

\* Numbers in parentheses are collisions made in series given after criterion had been reached.

terion in Experiment 1, two ( $B_2$  and  $B_7$ ) now attained it in Series 1 and two ( $B_5$  and  $B_{10}$ ) in Series 4.

Despite the instructions,  $B_4$  persisted in the habit, acquired during the experiments in which she was deafened, of reporting only once during a trial. When pressed to give two reports in this experiment—a 'first perception' and a 'final appraisal'—she advanced infinitesimally and reported again, hence we once more permitted her to have her way. She met criterion in Series 1, hence had little practice with hearing intact. Later, as Tables VIII and IX show, she willingly and accurately gave both of these judgments.

<sup>25</sup> As in the Cornell studies, S was permitted to walk toward the obstacle in any manner he wished. He could click his heels on the walk, shuffle his feet, and make as little or as much noise in walking as he wished. He could intensify noises normally made in walking, but 'artificial' noises, such as jingling coins or keys in his pockets, snapping his fingers, slapping his hands or thighs, whispering, hissing, whistling, etc., were denied him. Cf. Supa, Cotzin, and Dallenbach, *op. cit.*, 141, 164 f., 165 footnote 36.



(3) *Performances in additional series.* After meeting criterion, a few of the Ss were given additional series of trials to determine the constancy of their performances. As Table VII shows, all were constant; once meeting criterion, they continued to meet it.

(4) *Course of learning.* Insofar as the Ss yielded results showing a course of learning—and not all of them did, as seven of Group B and one of Group A met criterion in Series 1 and one (A<sub>9</sub>) was unable to meet it within the limits of our trial series—all learned suddenly or insightfully.

As Table VII shows, the number of collisions of the Ss requiring two or more trials to reach criterion dropped abruptly: B<sub>3</sub> collided with the obstacle 18, 14, 13, and 5 times in successive series; B<sub>6</sub>, 15 and 5 times; B<sub>10</sub>, 18, 10, 10, and 5 times; A<sub>1</sub>, 16, 14, and 2 times; A<sub>3</sub>, 15 and 4 times; A<sub>4</sub>, 9, 7, and 4 times; A<sub>6</sub>, 7, 11, and 2 times; A<sub>7</sub>, 21 and 3 times; A<sub>8</sub>, 12, 6, and 5 times; and A<sub>10</sub>, 10 and 3 times.

Comparison of these results with those of Experiment 1 (cf. Tables III and VII) reveals that the course of learning is similar for the Groups with normal hearing and dissimilar for the Groups with impaired hearing. The deafened Ss in Experiment 1 seemed to learn gradually by trial-and-error but in the present experiment they seemed to learn insightfully, *i.e.* they reached criterion abruptly.

(5) *Judgments.* From the results shown in Table VI, the judgments of the Ss of both Groups seem to be based upon the obstacle. The first perceptions and final appraisals differ by considerable amounts and the ratios of these performances are high, averaging for the Ss of Group A  $4.0 \pm 1.1$ , with individual ratios varying from 2.0 to 6.9, and for Group B,  $3.3 \pm 1.6$ , with individual ratios from 1.3 to 5.9. The distances traversed in giving these judgments seemed to be correlated with the distances of the obstacle from the starting-points. None of the Ss of either group seemed to be restricting the distances he walked.

(6) *Collisions.* The collisions of the Ss of both Groups attaining criterion in Series 1 (see Tables VI and VII) were chiefly of the third type, *i.e.* final appraisal collisions. Of those requiring more series, and in particular of those colliding numerous times with the obstacle in Series 1, the collisions were at first of the pre- and post-first-perception types. As learning progressed, however, the types changed to the second and third types until finally they were chiefly of the third. The collisions made by the hearing and the deafened Ss were not differentiated as to type as they were in Experiment 1.

(7) *Standard deviations.* The standard deviations (SD) of the performances of the Ss of both Groups are much larger in this experiment than



in Experiment 1 (cf. Tables II and VI). The average *SD* of the first perceptions and final appraisals of the hearing *Ss* were 86% and 102%, respectively, of the means of these reports in Experiment 1 (Group A) and 96% and 157%, respectively, in Experiment 5 (Group B). Of the deafened *Ss*, the *SD* were 86% and 138%, respectively, of the means in Experiment 1 (Group B) and 109% and 177%, respectively, in Experiment 5 (Group A). We are at a loss for an explanation of the greater variability of the reports in Experiment 5. The practice that the *Ss* had had in the perception of obstacles since serving in Experiment 1 should have yielded smaller not larger *SD*. The *Ss* were, to be sure, serving under new conditions, but new conditions were no novelty as the *Ss* had been continually meeting them in the successive experiments in Part I.

Two explanations of these results, both of which seemed reasonable and highly probable, were examined and found wanting. The first, the rapidity with which the *Ss* reached criterion, rested upon the assumption that the *SD* decreases with practice. Since the *Ss* required twice as many series to reach criterion in Experiment 1 (4.0 and 4.3) as in Experiment 5 (1.7 and 2.3), the level of practice was lower in Experiment 5 than in Experiment 1, hence the *SD* should be larger in Experiment 5 than in Experiment 1—which is just what we found. If this explanation is true, then it should follow that the *Ss* requiring the most series to reach criterion should have the lowest *SD*. To test this hypothesis, the *Ss* of both Groups were divided into subgroups: Group A into those meeting criterion in the first or second series and those meeting it in the third series; and Group B into those meeting criterion in the first series and those meeting it in the second or later series. The explanation was substantiated by the results of Group A (the subgroup meeting criterion 'early' had *SD* of 137% and 226%; the subgroup meeting criterion 'late' had *SD* of 94% and 135%) but it was negated by those of Group B (the 'early' subgroup had *SD* of 91% and 114% and the 'late' subgroup of 103% and 212%).

The second explanation, the rapidity of improvement of performance within the criterion-series itself, rests upon the assumption that the *Ss* improved so rapidly during the criterion-series of the experiment that the early performances were very different from the later, hence the *SD* of the averages of all the performances would be large—just as we found them to be. If this explanation is true, the means of the performances of the first and the last 10 trials of this series should differ greatly. Unfortunately they do not.

It may be of course that the explanation lies in a change in the attitude of the *Ss* toward the experiment. They may have become bored by it and resentful of our claims upon their time, hence indifferent in their performances. Though this conclusion does not necessarily follow from the premises, the *Ss* gave us no evidence that their attitude had changed. They seemed to be as interested and as coöperative during this experiment as ever before.

*Summary and conclusions.* With the exception of one member of the deafened Group (*A*<sub>9</sub>), the *Ss* of both Groups, hearing and deafened alike,



reached criterion quickly. The performances of all, even of those of  $A_9$ , showed clearly the importance of hearing. The  $Ss$  of Group A, all of whom had learned to perceive obstacles with unimpaired hearing, immediately suffered a decrement in their performances when their hearing was impaired. This was marked at first and though it was soon overcome, by all except  $A_9$ , to a degree that permitted them to reach criterion, few attained their former level of competency, as indicated by the performance-ratios.  $A_9$ , who was totally deafened by the ear-blocks because of deficient hearing at the higher audible ranges, failed utterly. All the other  $Ss$  of this Group attempted to overcome the impairment of their hearing by increasing the intensity of the sounds from their footsteps. The  $Ss$  of Group B, even those failing to attain criterion in Experiment 1, reached it quickly when hearing was restored to them.

The  $Ss$  of both Groups who met criterion seemed to base their judgments upon the obstacle. Though it is tempting to assume that they did and that they had learned, some for the first time and some for the second time, to perceive obstacles, caution dictates that this conclusion is not justified short of a test-series with check trials. The next experiment is, therefore, indicated.

#### EXPERIMENT 6

Experiment 6 was conducted to determine whether the  $Ss$  had learned in Experiment 5 to perceive obstacles or merely to avoid collisions. The experimental area, apparatus, procedure, and instructions were the same as in Experiment 2. The check trials (10) were distributed haphazardly as before among 20 obstacle-trials. All the  $Ss$  of the deafened group (Group A),<sup>26</sup> which is the more critical of the two Groups as the basis of their judgments is less certainly auditory and 7 of the 10  $Ss$  of the hearing group (Group B, see Table VIII) served in this experiment.

*Results:* (1) *Group A.* The performances of the deafened  $Ss$  (Group A) were, as Table VIII shows, highly variable. Five  $Ss$  made no errors (false reports) in the check trials; one ( $A_6$ ) made 1; two ( $A_1$  and  $A_3$ ) made two each; one ( $A_8$ ) made six; and one ( $A_4$ ) made nine.

Of the five  $Ss$  returning no false reports, three ( $A_2$ ,  $A_5$ , and  $A_7$ ) made 1, 0, and 2 collisions, respectively. Since these collisions were of the third type (final-appraisal collisions) and the performance-ratios of these  $Ss$  were high, being 3.8, 3.3, and 2.1, respectively, we conclude that they perceived

<sup>26</sup> Though  $A_9$  was far from criterion in Experiment 5, he was included because he was willing and because we wished to discover how he would react in the check trials.



and reacted to the obstacle. These Ss possessed normal or superior hearing in one or both ears (see Table I). Since they were among those increasing the noise of their footsteps, we also conclude that they accomplished the perception, despite the blocking of their ears, by means of hearing.

The other two Ss making no false reports ( $A_9$  and  $A_{10}$ ) collided with the obstacle 13 and 11 times, respectively. Such large percentages of collisions (65 and 55) are hardly indicative of the 'obstacle sense.' We know from Experiment 5 that  $A_9$  did not learn to perceive obstacles with his ears blocked, hence his results here must be accepted as the chance performances of an S lacking the ability. Since his results and those of  $A_{10}$  are so very

TABLE VIII

EXPERIMENT 6: MEAN DISTANCES AND SD (IN FT.) OF THE FIRST PERCEPTIONS ( $p$ ) AND FINAL APPRAISALS ( $a$ ), THE RATIOS OF THESE DISTANCES ( $p/a$ ), THE NUMBER OF COLLISIONS, AND THE NUMBER OF TIMES OBSTACLES WERE REPORTED IN THE CHECK TRIALS (FALSE REPORTS)

Group A (blindfolded and deafened)							Group B (blindfolded only)								
S	p		a		p/a	No. of collisions	No. of false reports	S	p		a		p/a	No. of collisions	No. of false reports
	M	SD	M	SD					M	SD	M	SD			
1	2.50	5.57	1.37	4.42	1.8	7	2	1	6.24	4.32	1.28	1.05	4.9	4	1
2	6.07	0.12	1.61	3.76	3.8	1	0	2	.75	.78	.25	.00	3.0	1	0
3	6.45	7.74	.75	.77	9.2	16	2	3	5.59	6.62	3.26	3.39	1.7	4	3
4	8.09	5.16	1.98	.66	4.1	9	9	4	1.05	.84	.47	.35	2.2	1	0
5	4.14	5.62	1.24	.76	3.3	0	0	5	6.60	8.32	1.98	2.06	3.3	3	0
6	1.42	.22	.25	.00	5.7	3	1	8	3.75	1.51	.71	.60	5.3	0	0
7	4.10	1.30	1.93	1.08	2.1	2	0	10	3.84	4.14	1.11	2.36	3.5	3	2
8	7.21	1.88	3.98	1.11	1.8	6	6								
9	3.20	4.13	2.18	1.92	1.5	13	0	Av.	3.97	3.79 (95%)	1.29	1.53 (119%)	3.4	2.3	0.86
10	1.92	.88	.95	.74	2.0	11	0								
Av.	4.51	4.06 (90%)	1.62	1.52 (94%)	3.5	6.8	2.0								

similar in the present experiment, the conclusion that  $A_9$  failed must be extended to  $A_{10}$ .

Of the remaining Ss in this Group, two ( $A_3$  with 16 collisions and 2 false reports, and  $A_4$  with 9 collisions and 9 false reports) were classified with  $A_9$  and  $A_{10}$  as demonstrating that they had failed to learn to perceive obstacles; and three,  $A_1$ ,  $A_6$ , and  $A_8$ , were classified with  $A_2$ ,  $A_5$ , and  $A_7$  as having acquired that ability.

$A_6$  unquestionably belongs among the group demonstrating that ability. Not only are his collisions and false reports few in number (3 and 1 respectively) but his performance-ratio ( $p/a = 5.7$ ) is high and the SD of his performances are exceptionally low being 15% of the means of his first perceptions and 0% of the means of his final appraisals—every one of which (17), measured to the nearest quarter-foot, were 0.25 ft. from the obstacle.

The results of  $A_1$  and  $A_8$  are doubtful:  $A_1$  made 2 false reports,  $A_8$  made 6;



A<sub>1</sub> collided with the obstacle 7 times, A<sub>8</sub>, 6 times. Their performance-ratios (both 1.8) are moreover low, being next lowest to A<sub>9</sub> who, as we know, failed to learn. These two Ss might well have been classified among the failures but we preferred to err, if we erred at all, upon the side of inclusiveness. No harm would be done by that. If they should have been classified among the failures, that will be demonstrated in the next experiment in which cues that they might have been using in this one are eliminated.

(2) *Group B.* The results of Group B (the hearing group), like those of the hearing group in Experiment 2, show clearly that the judgments of the Ss were based upon the obstacle (see Table VIII).

Four of the 7 Ss (B<sub>2</sub>, B<sub>4</sub>, B<sub>7</sub>, and B<sub>8</sub>) made no errors in the check trials; one (B<sub>1</sub>) made one; one (B<sub>10</sub>) made two; and one (B<sub>9</sub>) made three. Their collisions varied in number from 0 to 4. One S (B<sub>8</sub>), who made no false reports, made also no collisions; two Ss (B<sub>2</sub> and B<sub>4</sub>) made one collision each; two (B<sub>5</sub> and B<sub>10</sub>) made three each; and two (B<sub>1</sub> and B<sub>9</sub>) made four each. All of these collisions were of the third, the final-appraisal type.

The performances of these Ss did not suffer in any respect by the introduction of the check trials.

*Summary and conclusions.* As these results show, all the Ss tested from the hearing group (Group B) and certainly three (and perhaps five) of the Ss from the deafened group (Group A) demonstrated that they had learned to perceive obstacles. Four (and perhaps six) of the Ss from Group A who had met criterion in Experiment 5, failed to demonstrate that they based their judgments upon cues derived from the obstacle. One S (A<sub>9</sub>) who did not meet criterion in Experiment 5, demonstrated again his inability to do so without hearing.

These results show again the necessity of conducting check trials in experiments of this kind. Except for them we should have had to conclude that nine instead of three (or possibly five) of the Ss from the deafened group (Group A) had learned to perceive obstacles. Now that we know who did, we can again set conditions to determine what cues were used as the basis of their judgments.

#### EXPERIMENT 7

Experiment 7, a repetition of Experiment 4 with the deafened and hearing groups interchanged, was undertaken to determine how the Ss reacted when temperature and olfactory cues were eliminated and auditory cues were enhanced by the reduction in the noise-level of the experimental area—things accomplished by conducting the series at night.

The procedure was the same as that of Experiment 6 with the exception that the trials were conducted at night, between 8–10 P.M., instead of dur-



ing the daylight hours. Six Ss from each group served in the Experiment—all from Group A who had given any indication in Experiment 6 that they had learned to perceive obstacles (we omitted only those clearly demonstrating failure) and all from Group B who were available.

*Results: (1) Group A.* The results of Group A (the deafened group) are given in Table IX. False reports vary in number from 2 to 9 and collisions

TABLE IX

EXPERIMENT 7: MEAN DISTANCES AND SD (IN FT.) OF THE FIRST PERCEPTIONS (*p*) AND FINAL APPRAISALS (*a*), THE RATIOS OF THESE DISTANCES (*p/a*), THE NUMBER OF COLLISIONS, AND THE NUMBER OF TIMES OBSTACLES WERE REPORTED IN THE CHECK TRIALS (FALSE REPORTS)

Group A (blindfolded and deafened)							Group B (blindfolded only)								
S	p		a		p/a	No. of collisions	No. of false reports	S	p		a		p/a	No. of collisions	No. of false reports
	M	SD	M	SD					M	SD	M	SD			
1	5.83	9.49	.75	1.00	7.8	15	2	1	6.38	4.21	1.76	1.96	3.6	3	1
2	10.65	9.50	4.20	6.47	2.5	5	9	2	2.93	4.34	1.24	1.98	2.4	5	1
3	4.90	8.40	2.78	2.88	1.8	12	5	4	3.00	1.42	.75	.48	4.0	0	0
6	3.40	6.08	.42	.33	8.1	11	3	5	7.47	1.94	3.63	.84	2.1	3	1
7	4.02	5.24	1.40	1.66	2.9	12	4	8	3.60	.94	1.27	.62	2.8	1	0
8	9.50	8.98	5.50	5.90	1.7	11	9	10	7.89	8.22	.68	.63	11.6	5	2
Av.	6.38	7.95 (125%)	2.51	3.04 (121%)	4.1	11	5.3	Av.	5.21	3.51 (67%)	1.55	1.08 (70%)	4.4	2.8	0.8

from 5 to 15. The S ( $A_1$ ) returning the fewest false reports (2), collided with the obstacle the greatest number of times (15); and the S ( $A_2$ ) with the fewest collisions (5), returned the greatest number of false reports (9). There is little evidence here that any of the Ss possessed the ability to perceive obstacles.

(2) *Group B.* The results of Group B, in marked contrast to those of Group A, were better in this experiment than in Experiment 6. Though the number of collisions and false reports are approximately the same in the two experiments (cf. Tables VIII and IX), their performances were in every other respect much better. Their average performance-ratios were increased to 4.4 from 3.4, and the average SD of their first perceptions fell from 95% to 67% of the average of the means and of their final appraisals from 119% to 70%. The Ss of Group B were more consistent and reliable in their performances in this experiment than in any other of this study.

*Discussion and conclusions.* While the results of the hearing group (Group B) were expected—they conformed to those of the hearing group in Experiment 4 and to their own results in Experiment 6—the results of the deafened group (Group A) were totally unexpected. The proportion of failures (100%) stands in disagreement with the results of the deafened Ss in Experiment 4 in which 4 Ss succeeded and 2 failed, and in complete



disagreement with their own results in Experiment 6 in which they all succeeded. Indeed, as will be recalled, they were selected to serve in this experiment upon the basis of their performances in Experiment 6. Since they increased the noise of their footsteps by shuffling their feet and clicking their shoes on the sidewalk, we concluded that their perceptions, in part at least, were still based upon sounds. We expected, therefore, when the trials were conducted at night and the intensive level of the ambient noises was greatly reduced, that the Ss would better, not worsen, their performances. That they worsened them to the point of complete failure negates the conclusion that their perceptions were based upon sound and leads to the conclusion that their 'final perceptions' in Experiment 6 were based upon cues (thermal or olfactory) the elimination of which caused them to fail in Experiment 7. If this is the case, and we do not see how we can, in the light of our results, avoid accepting it, what then is the meaning of the deafened Ss' attempts to increase the intensity of the sound of their footsteps? If some of the Ss failed because of the lack of sound and others because of the lack of thermal or olfactory cues, what justification is there for the conclusion that any single condition is *necessary* for the perception?

In regard to the Ss' attempts to increase the intensity of the sounds of their footsteps, the following observations may be pertinent. Failure in the trials (false reports, collisions, and low performance-ratios) are chiefly matters of the final appraisals, not first perceptions. Of these two judgments, the final appraisals are the more difficult. They are acquired later than the first perceptions and their percentage of variability (*SD*) from their means is usually greater (see all of the Tables). When hearing is unimpaired, as it was in Part I, the Ss of Group A based their judgments upon auditory cues—the most obvious and the most helpful—and learned rapidly. They met criterion in Experiment 1 and escaped the pitfalls of the check trials in Experiments 2 and 4. When the noise-level of the experimental area was reduced in Experiment 4 by conducting the trials at night, their performances were greatly improved. When they were deafened in addition to being blindfolded in Experiment 5, they lost their ability immediately and completely, as Series 1, Table VII, shows. Audition was for them at that period a *necessary* condition. As the trials progressed, they did two things: (1) increased the intensity of the sound of their footsteps so as to break the barrier of their ear-blocks; and (2) discovered other cues that were unnecessary when audition was available but were helpful when it was not. (Just as a blinded person discovers other means than vision of perceiving obstacles which were unnecessary when he could see.) By means of one of the other or of both of these methods, 9 of the 10 Ss soon recovered their ability to meet cri-



terion. One of the group ( $A_9$ ), who had defective hearing in both ears at the higher audible ranges, was unable to accomplish this.

If the increased intensity of their footsteps was just sufficient to pass their ear-blocks, the easier of the two judgments (the first perceptions) would suffer the less. The intensive increase might be sufficient for both judgments (first perceptions and final appraisals) for  $S$ s with particularly acute hearing or with ineffective ear-blocks, but for most of them criterion would not be reached until the weak auditory cues of the final appraisals were supplemented or replaced by thermal or olfactory cues, which are from their very nature perceived only when the obstacle (their source) is near.

If these assumptions are correct, then the results of Experiments 6 and 7 are readily explained. Of the  $S$ s (9) meeting criterion in Experiment 5, some (6) were able by the means described, to demonstrate that they perceived obstacles when check trials were introduced—their first perception being based upon auditory cues and their final appraisals upon thermal or olfactory cues. When the trials were conducted at night (Experiment 7) under conditions in which thermal and olfactory cues were lacking, the  $S$ s failed. They continued, however, to intensify the sound of their footsteps because those sounds were the cues of their first perceptions, which they continued to make.

The results of the  $S$ s of Group B, Part I are very similar to those of Group A, Part II; the only discrepancy being the proportion of successes and failures in the experiments conducted at night in which *all* of the  $S$ s of Group A failed (Experiment 7), whereas two-thirds of those of Group B succeeded (Experiment 4). This difference may be explained upon the basis of the  $S$ s' acuity of hearing, the intensive increase in the sound of their footsteps, or the effectiveness of their ear-blocks. If any or all of these factors were operative in an individual case, then  $S$  would be able to base his judgments upon auditory cues, hence his performances in the trials conducted at night, in which thermal and olfactory cues were eliminated and ambient noises were reduced, would either be unaffected or considerably bettered. Since the results of four of the  $S$ s of Group B were unaffected or considerably bettered in the night experiment (Experiment 4), we must assume that they, for one or another or all the reasons mentioned, were, in contradistinction to the  $S$ s of Group A, using auditory cues in the trials conducted at night.

Now for consideration of the conclusion that any condition is necessary for the perception. Since Diderot's formulation of the problem, search has been for the *necessary and sufficient conditions*. The results of this study



indicate, however, that the search is vain, that no single condition is *necessary* for the perception. Obstacles may be perceived without vision under many different conditions. Audition is the principal basis of the perception in the sense that it is the most reliable and accurate and most universal of the various cues; but blind or blindfolded people use, as we observed above (p. 528), any and every cue that serves them: cutaneous pressures caused by deflections from the obstacle of the wind or even of their breath; thermal cues, warmth or colds, radiated from or interrupted by an obstacle; olfactory cues; or auditory cues.

Some of these cues are rarely present—few obstacles, for example, give off an odor but when they do, and when the observer has learned to associate the odor with the obstacle, final appraisals are accurate and precise. None of these cues, on the other hand, is always present—not even audition. When none of them is present, as for example, in the case of the deaf-blind Ss in the second Cornell study, the Ss not only fail to perceive the obstacle but they resort to subterfuges to avoid collisions. If audition is eliminated, as it was for half of the deafened Ss in Experiments 2 and 6, those failing to detect and to associate the available thermal and olfactory cues with the obstacle also failed to perceive the obstacle. If, however, they learned to base their judgments upon these cues, they succeed but only to fail in Experiments 4 and 7 in which these cues were eliminated.

The results of this study reconcile, to a great extent at least, the discrepant theories and conclusions of earlier investigators. Like the descriptions of an elephant given by the five blind men of India, every author is correct from his own point of view. Sounds, pressures, warmth, cold, and smell are under certain conditions *adequate* and *sufficient* for the perception of obstacles. None is, however, *necessary*, but audition is credited with being necessary because it is more often available than any of the others; and it is *necessary* in the sense that a totally deaf person will not be able to demonstrate the ability sufficiently often by means of the other cues, which are frequently lacking, to be credited with it.

#### SUMMARY AND CONCLUSIONS

This study was undertaken (1) to determine whether the results and conclusions of experiments upon the perception of obstacles by blind and blindfolded Ss, which were conducted indoors under carefully controlled laboratory conditions, could be duplicated out of doors under the uncontrolled conditions of everyday life; and (2) to discover whether this perception was a special ability possessed by the gifted only or was one that was capable of being learned by every person possessing normal or near normal



hearing. It was made with 20 undergraduate students (7 women and 13 men), who, as audiometric tests revealed, varied normally for an unselected group in their ability to hear—some of them had normal hearing in one or both ears, some had acute hearing, and some deficient hearing, particularly at the higher audible ranges, in one or both ears.

The Ss, matched for their ability to hear, were divided into two groups of 10 each. In Part I of the study, one group (Group A) was blindfolded only and the other (Group B) was blindfolded and deafened. In Part II, a repetition of Part I, the rôles of the Groups were interchanged. Group A was blindfolded and deafened and Group B was blindfolded only. Each Group was, therefore, a control for itself as well as for each other.

The first experiments in each part of the study (*i.e.* Experiments 1 and 5) were in learning. An endeavor was made in each to teach the Ss to perceive the obstacle (a large Masonite screen) under the particular conditions under which they were serving. The second experiments (Experiments 2 and 6) were test experiments with check trials (trials in which the obstacle was not present). They were undertaken to determine whether the Ss had learned to perceive the obstacle in the first experiments, *i.e.*, to react to cues derived from the obstacle, or had merely met our criterion of learning (25 successful performances out of 30 trials, *i.e.* 5 or fewer collisions) by chance or by limiting the distances they walked in approaching it. The third experiment (Experiment 3), a subsidiary conducted only in Part I, was undertaken to determine the rôle of odor in the perception. The procedure of Experiment 2 was repeated with S's nostrils being so stopped that the possibility of detecting the obstacle by smell was eliminated.

All of these experiments were conducted during the day under highly variable conditions—ambient noises from near by construction, street traffic, the passing of students to and from classes, and the heat and glare of the sun which varied with the cloudiness of the day. The last experiments in each part (Experiments 4 and 7) were conducted, therefore, at night under conditions that were much more constant: the noise-level of the experimental area was considerably reduced and the cues, which owed their existence to the sun, were entirely eliminated.

The results of these experiments and the conclusions drawn from them are as follows:

(1) Ss possessing normal or near normal hearing, who were blindfolded only, learned rapidly to perceive obstacles under the complex and variable conditions met out of doors and demonstrated their ability in test-experiments. Our results confirm those obtained indoors under laboratory conditions.



That all of these Ss should have acquired the ability leads to the conclusion that it is not a special endowment possessed only by a few but is an ability that every normal person, possessing normal or near normal hearing, is able to acquire under the conditions of everyday life. The implications of this conclusion are far reaching: they are that all persons, blind but otherwise normal, are capable of learning to perceive obstacles; and that there is no reason, other than the lack of courage or the will to learn, for any of them leading a vegetative existence in which he has to be led about.

(2) The behavior of the Ss, who were deafened in addition to being blindfolded, was different from that of the Ss who were blindfolded only.

(a) The deafened Ss increased the intensity of the sound of their footsteps. They made more noise than the group with unimpaired hearing and also more than they themselves made when their hearing was unimpaired. They did this, as we concluded, in an endeavor to break through the barrier of their ear-blocks to obtain cues from hearing.

(b) The deafened Ss differed greatly among themselves in their performances. (i) Some learned nothing beyond the mere mechanics of the experiment; (ii) others learned at varying rates to meet criterion.

(i) Those failing to meet criterion were divided, according to their audiometric records, into two groups: those possessing normal hearing; and those whose hearing was defective at the higher audible ranges. The failures of the Ss, whose unimpaired hearing was normal, is due, as we believe, to the fact that they depended entirely upon auditory cues for their perceptions of the obstacle and the intensive increase in the sound of their footsteps was not sufficient to break through their ear-blocks. The Ss with defective hearing may have failed for the same reasons but it is also possible that they failed because their hearing was defective at the very ranges necessary for the auditory perception.

(ii) The Ss meeting criterion also fall into one of two groups accordingly as their performances were bettered or worsened in the test-experiments conducted at night.

The group, whose performances were bettered, found that they were still able when deafened—because of the increased intensity of their footsteps, or the acuity of their hearing, or ineffective ear-blocks—to detect the obstacle by means of auditory cues, hence sought and utilized no others. We are forced to this conclusion by the very fact that their performances were improved in the night tests in which the noise-level of the experimental area was reduced and all other cues except the auditory were eliminated.

The group, whose performances were worsened, sought other cues when they found, after being deafened, that the increased intensity of the sound



of their footsteps did not break through their ear-blocks. They finally discovered the thermal and olfactory cues which, though less efficient than the auditory, served them well enough in the learning experiments to meet criterion and, in the daytime test, to demonstrate that they were reacting to cues derived from the obstacle. When, however these cues were eliminated in the night tests, they failed completely.

(3) The fact that some of our Ss failed to perceive the obstacle because of the lack of sound and others because of the lack of thermal or olfactory cues, leads us to conclude that no single condition is necessary for the perception. Obstacles may be perceived without vision under certain conditions by many different means—sound, temperature (cold and warmth), wind pressure, and odor. Audition is, however, the principal basis of the perception and it is *necessary* only in the sense that its cues are the most reliable, accurate, and universal of all the cues yielding the perception.

(4) The course of learning for the Ss with hearing is sudden or insightful; for those deafened there is a tendency for it to occur gradually as by trial-and-error.

(5) The "black curtains" or "dark shades" reported by the hearing Ss when they came near to the obstacle are imaginal experiences that are aroused associatively by auditory cues.



## VISIBILITY-INVISIBILITY CYCLES AS A FUNCTION OF STIMULUS-ORIENTATION

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The original impetus for the present study was the observation of a recurrent perceptual phenomenon during prolonged fixation involved in a study of figural after-effects. Figures, or parts of figures, occurring in parafoveal regions were observed to disappear and reappear at irregular intervals. It was further noted that the frequency of disappearance seemed to be differentially related to the location in the visual field at which the stimulus-figure was presented.

Figural disappearances as a concomitant to long fixation have been previously noted. Weitz and Post,<sup>1</sup> Weitz and Compton,<sup>2</sup> and Marks<sup>3</sup> mention them incidentally in their studies of figural after-effects. Systematic studies, however, do not appear to have been undertaken.

There is a definite similarity between fluctuations of attention and the alternations of visibility we have investigated. The experimental conditions under which our observations were taken, though, are quite different from those encountered in such experiments. Fluctuation of attention is ordinarily observed under conditions of liminal intensities. In the present study, relatively heavy India-ink drawings against a white cardboard background were viewed under several foot-candles of illumination. Under such conditions, the stimulus-objects were strongly supraliminal.

In any case, there is evident need for exploratory study aimed at establishing the conditions under which the disappearances occur. This is especially true since we know of no instance in which perceptual variations of this type in vision have been related to stimulus-orientation. The problem of the basic etiology of such findings remains whether further investigations show our results to be special instances of some previously investigated phenomenon, such as fluctuation of attention. If such is found to be true,

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<sup>1</sup> Joseph Weitz and Dorothy Post, A stereoscopic study of figural after-effects, this JOURNAL, 61, 1948, 59-65.

<sup>2</sup> Weitz and Bertita Compton, A further stereoscopic study of figural after-effects, this JOURNAL, 63, 1950, 78-83.

<sup>3</sup> M. R. Marks, Some phenomena attendant on long fixation, this JOURNAL, 62, 1949, 392-398.



then our concepts relating to the phenomenon involved must be reorganized to take into account the factor of stimulus-orientation.

#### EXPERIMENTAL CONDITIONS

*Test-figure and background.* A straight black line 0.04 cm. in width and 13 cm. in length, radiating from a fixation-point, was used as the stimulus-figure. The fixation point consisted of an 'X' bounded by a circle 0.7 cm. in diameter. At the observation distance of 110 cm., the line subtended a visual angle of  $1' 15''$  in width and  $6^{\circ} 44'$  in length. The circle around the fixation-point subtended  $21' 50''$  of visual angle.

The test-figure was drawn in India ink against a circular white cardboard background 80 cm. in diameter, homogeneously illuminated to a level of 3.2 foot candles. This cardboard disk was mounted at right angles to *S*'s line of regard in such a manner that it could be rotated at the end of a run to bring the stimulus-figure to a different angular orientation.

*Apparatus.* The recording apparatus was a two-channel Brush recorder operating at a constant paper speed of 5 mm. per sec. A separate response key was wired into each channel. One key was operated by *S*, the other by *E*. *S*'s key recorded the frequency, duration, and time of occurrence of the response to the figural disappearances. *E*'s key recorded the response by *E* to observed blinks by *S*.

*S* was seated at a table on which the response-key was mounted. An adjustable chin-rest restricted his head movements and brought his eye-level to that of the fixation-point. He was also provided with a patch for his left eye. *E* sat behind and to the right of *S*, at such an angle that he could readily observe the blinking of *S*'s eye.

*Subjects.* The *Ss* (four men and six women) were college students. All were without serious visual defects in astigmatism, phoria, and acuity. The *Es* and various other laboratory personnel also acted as *Ss* during the supplementary explorations described below.

*Procedure.* For recording purposes,  $0^{\circ}$  was taken as the 12:00 o'clock orientation of the stimulus-figure,  $90^{\circ}$  as 3:00 o'clock, etc. The line at 24 angular orientations, from  $0^{\circ}$  to  $345^{\circ}$  in steps of  $15^{\circ}$ , was presented in a different random sequence to each *S* during the course of the experiment.

A 30-min. period for instruction and familiarization preceded the first test-run. The level of illumination during this period was the same as that used during the experiment, thus allowing *S* adequate adaptation. A test-run consisted of a 5-min. fixation of the stimulus-figure at one of the 24 positions. *S*'s task was to press the response-key on the disappearance of any part of the line and to release the key on its reappearance. Every test-run was followed by a rest-period of 5 min.—every third being for 10 min., and the twelfth being at least 60 min. It was usually possible to complete the entire series for an *S* during the same day. It was sometimes necessary, however, to use two half-day sessions separated by several days.

*Instructions.* The instructions to *S* were as follows:

Your task is to gaze fixedly at the 'X' encircled in the center of the field. Keep this fixation as consistently as possible throughout each test-session. While you are doing this, changes may take place in the appearance of the line which extends outward from the small central circle. At the end of the test-session we will ask



you to describe any changes you may have noticed. One possible kind of change is the disappearance of part, or all, of the straight line. On the other hand, it may well be that no disappearances occur. Whatever happens, do not feel concerned about it, since we are interested simply in finding out what occurs. If any part of the line disappears, press down on the telegraph key; release the key as soon as it reappears. A natural tendency that you should try to avoid is that of glancing toward the part of the line that disappears to see if it is actually gone.

*Treatment of the data.* Our first treatment of results consisted of a  $10 \times 10 \times 24$  three way analysis of variance, without replication in the 2400 cells. The independent variables were,  $S_s$  (10), time ( $10 \frac{1}{2}$ -min. divisions of the test-period), and angular orientation of the stimulus-object (24 angles, at intervals of  $15^\circ$ ). The dependent variable was the frequency that the stimulus-object disappeared. The effect of a fourth variable, the sequence of angular orientations in which the stimulus-object appeared, was balanced out by presenting a different sequence to each  $S$ . Each angle thus occurred in 10 different representative positions in the 24 angle sequence during the course of the entire experiment. Analysis on the basis of this arrangement revealed that sequence was not related to the frequency of disappearance, hence it could not, in any event, be a complicating factor.

TABLE I  
SUMMARY OF THE ANALYSIS OF VARIANCE

Variable	df	F-ratio re:error	F-ratio re:error plus inter- actions
Angle of stimulus (A)	23	22.18*	16.28*
Subject (S)	9	281.32*	206.46*
Time, $\frac{1}{2}$ -min. periods (T)	9	2.22	1.63
$S \times A$	207	2.84*	
$S \times T$	81	8.57*	
$A \times T$	207	.32	
Error ( $S \times A \times T$ )	1863		
Error plus interactions	2358		

\* Significant beyond the 1% level.

## RESULTS

Table I summarizes the analysis of variance. The relationship of frequency of disappearance as a function of the orientation of the stimulus-object is well beyond the 1% level of confidence. This is true, as shown in the last column of the Table, even when interactions are included in the error term used to ascertain the significance of the main variables. An angle by  $S$  is also significant but is small in comparison with the main variables.

The curve of frequency of disappearance as a function of the orientation of the stimulus-object is shown as a solid line in Fig. 1. Points are the averages of the performance of 10  $S_s$  (ordinate) at each angular orienta-



tion tested (abscissa). Maximal frequencies of disappearance occur at  $45^\circ \pm 15^\circ$  from the horizontal and vertical main axes of the visual field, while minimal frequencies occur at the main axes  $\pm 15^\circ$ . The difference between these maximal and minimal mean values was found to be significant far beyond the 1-% level. The non-parametric ranks test of significance, that

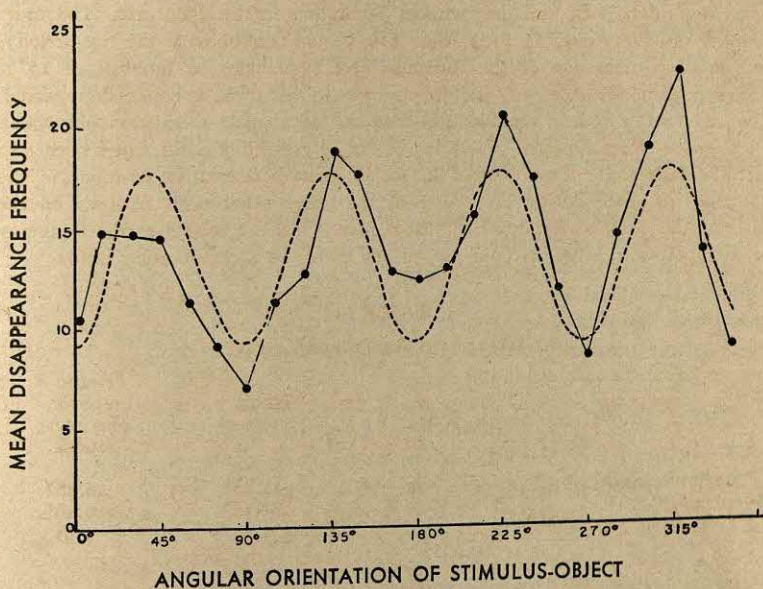


FIG. 1. FREQUENCY OF DISAPPEARANCE OF THE STIMULUS-OBJECT AS A FUNCTION OF ANGULAR ORIENTATION

Solid line shows the observed frequencies; broken line, the calculated frequencies.

we used, gave a rank-difference total of 108 between 12 unmatched pairs of means.<sup>4</sup>

The simplest curve adequately describing the plotted points was sinusoidal. Consequently the sine-curve  $y = 13.642 + 4.116 (\sin 4x - 90^\circ)$ , where  $y$  is the mean disappearance frequency and  $x$  the angle of orientation, was fitted to the data by the method of least squares. This best fit sine-curve is plotted as a dashed line in Fig. 1. Variance of the 24 plotted points around this calculated curve was compared, by the  $F$ -test, to their variance

<sup>4</sup> F. Wilcoxon, Individual comparisons by ranking methods, *Biometrics Bull.*, 1, 1945, 80-82.



around the grand mean of disappearance frequencies for all angles. The resulting  $F$ -ratio was 1.92 for 23 versus 22 degrees of freedom. This value is just short of the 5-% level of significance.

A further check on the non-linearity of results, utilizing all 240 points rather than just the means as in the sine-test, was desired. Eta and epsilon, measures of curvilinear regression unrelated to any particular curve, were fitted to this data. An eta of 0.326 was obtained. The coefficient epsilon, which corrects for bias in eta due to the breakdown of variance into the angular orientation categories, was found, and its square, 0.051, is very close to the 0.054 needed for significance at the 5-% level.

These measures in themselves are not wholly adequate tests in the present instance. Sources of variance other than angular stimulus orientation, determined to be large and significant in the initial analysis of variance, are not removed from the curvilinear data whose significance is under test. In view, however, of the results of the sine-curve fit, the values of eta and epsilon, and the fact that 7 of our 10  $S$ s exhibit the same non-linear pattern, we can be sure that the sine-like rise and fall of the frequency of disappearance with angular orientation of the stimulus-object is a real effect.  $S$ s who did not exhibit this rising and falling pattern experienced few disappearances. An extra  $S$ , a woman whose visual acuity was 20/40, also served in the experiment. She reported no disappearances at all, hence her results are not included in this study.

Table I shows the largest source of variance in frequency of the disappearances to arise from differences among  $S$ s. There is also, as Table II shows, a significant variance between  $S$  and the time of the first disappearance. Table II gives the mean and standard deviation of the number of disappearances in a 2-min. period following the first disappearance as related to the mean and standard deviation of elapsed time to these first disappearances.

TABLE II  
FREQUENCY OF DISAPPEARANCE IN 2 MIN. FOLLOWING THE FIRST DISAPPEARANCE  
AS RELATED TO THE TIME TO THAT DISAPPEARANCE

Group	Frequency of disappearance		Time (sec.) to first disappearance	
	M	SD	M	SD
1 (3 $S$ s)	10.39	5.83	11.54	7.48
2 (6 $S$ s)	2.94	2.36	60.05	47.35
3 (1 $S$ )	6.50	1.44	13.23	5.07

The 2-min. period was optimal since it enabled us to use almost all of the 240 combinations of  $S$  and angle. Included in these combinations were seven points for which initial disappearances occurred after 3 min. had elapsed. Since 2 min. did not remain for the disappearances to occur, these values were not included in the calculations.



Table II shows that all the Ss, except one, fell into distinctly dichotomous groups. The first group of 3 Ss experience their first disappearance relatively early in a 5-min. run and have low variability of elapsed time to that disappearance. Their mean frequency of disappearance and its standard deviation are, however, high. In contrast, the second group of 6 Ss first experience disappearances, on the average, further along in the test-period and show high variability in time to the first disappearance. Their disappearance rate is low with correspondingly low variability. The exception to this dichotomous grouping is for the S whose variability in frequency of disappearance and in time to the first disappearance was not large.

The data were analyzed for other relationships, which may be summarized as follows.

(a) Frequency of disappearance did not vary significantly with elapsed time between half-minute divisions of the 5-min. test (see Table I).

(b) The data for 2 Ss were tested for periodicity of disappearances according to the theory of runs.<sup>5</sup> At no angular position was there any significant departure from randomness in times between disappearances. Observation of the remainder of the data, in comparison to the mathematically treated portion, also indicated no periodicity.

(c) Average duration of disappearances was unrelated either to frequency of disappearances or to angular orientation of the stimulus-object.

(d) Frequency of disappearance, as mentioned previously in connection with Table I, bore no relation to the sequence in which a particular orientation of the stimulus-object was presented during the complete experiment.

(e) The number of eye winks in each 5-min. test-period was paired, for correlational purposes, with the frequency of disappearance in each of the test-periods. The 240 pairs of observations thus obtained gave a significant correlation of  $-0.22$  between frequency of eye blinks and frequency of disappearances of the stimulus-object.

Test-retest variation of the data was measured by having one S make a total of 30 runs, 5 each for observation with both eyes, right eye alone, and left eye alone for orientation of  $90^\circ$  and  $270^\circ$ . Standard deviations for the five measures under each of these conditions ranged from 1.2 to 5.0 disappearances per 5-min. run. The standard deviations vary directly with mean frequency of disappearance as was true in the main experiment. We had hoped also to gain from these data some insight into the peripheral or central nature of the cycles of visibility-invisibility. The only results from which conclusions might be drawn was the finding that in general binocular observation yielded as many disappearances as monocular observation.

When disappearances occur, the stimulus area involved takes on the appearance of the background. Disappearances usually take place gradually, proceeding inward from the periphery, although frequently, some central portion of the line, or the entire line disappears simultaneously. Reappearances are almost always sudden, however. The locus which had disappeared 'snaps back' into perception.

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<sup>5</sup> P. G. Hoel, *Introduction to Mathematical Statistics*, 1947, 177-182.



The experiment herein described is obviously not designed to test hypotheses about visual processes but rather to explore one major phase of visibility-invisibility alternations. Consequently, pilot observations were made under several conditions to provide some tentative material as a guide toward further research of a more theoretical nature. The results of these observations under the various conditions follow.

(a) *Background illumination.* Stimulus disappearances occur with background illumination ranging from 2 to 100 foot candles. Levels outside these limits were not tested.

(b) *Visual angle.* Disappearances for visual stimuli of wide angular substance, up to at least  $1^\circ$  at the eye, were obtained.

(c) *Stimulus-type.* Disappearances occur with both 'closed' and 'open' type figures.

(d) *Three dimensional objects.* Three dimensional objects disappear.

(e) *White stimulus-objects on a black background.* Disappearances of white stimulus-objects on a black background occur as they did under opposite conditions in the main experiment. In this case, however, the background at the locale of the disappearance appears black.

(f) *Area of disappearances.* The locus of areas along the stimulus line that are especially susceptible to disappearances vary with the different angles of stimulus-orientation.

(g) *Exit pupil.* Disappearances are not prevented when almost all light reaching the eye falls on the pupil during observation through a 1-mm. exit pupil.

(h) *Flicker.* Disappearances were reduced by interrupting the visual field at regular intervals.

(i) *Monochromatic light.* Stimuli on nearly monochromatic backgrounds disappear sooner, more frequently, and more completely than those on a white background.

(j) *Corneal reflection.* When one's own eye motion during a fixation period is observed by means of light reflected from the cornea into a mirror just above the linear visual stimulus, no gross eye motion correlated with stimulus appearance or disappearance could be observed. This type of observation is by no means precise enough to exclude the definite possibility that nystagmus of fixation is related to the disappearances.

## DISCUSSION

Of chief interest in the results is the evidence of inhomogeneities at some stage or stages of the visual process. This evidence is in the form of the roughly sinusoidal differential rate of stimulus-disappearances as a function of their angular orientation. Two problems suggest themselves: first, that of explaining the disappearances; and secondly, that of accounting for the differential rate as a function of angle.

Stimulus-disappearances are usually subsumed under the heading 'fluctuation of attention.' It has been maintained that these fluctuations are related to eye movement, pupillary changes, the circulatory and streaming phe-



nomena, central factors, and (most convincing and most generally accepted) adaptation to and recovery from constant stimuli at retinal levels.<sup>6</sup> Recently, minute and rapid eye motions were found during fixation which may have a possible relation to our disappearances.<sup>7</sup> Of the many prevailing theories, none except adaptation and nystagmus of fixation give satisfactory explanations of the disappearances reported in this experiment.

In the main, these theories are directed toward the explanation of the fluctuations of near-threshold stimuli. It has been shown by Guilford and others that disappearances become less frequent almost to the point of complete absence, when the intensity of the stimulus-light is increased to superliminal levels.<sup>8</sup> This is not the case for the high contrast, high brightness, stimulus-types used in this study.

Disappearances occur in the absence of macroscopic fluctuations in light-level at the retina as was evidenced by restricting the light entering the eye by use of an exit pupil with a diameter of opening smaller than that of the smallest pupil obtainable naturally under bright light. Fluctuations in the size of the pupil can have little, therefore, to do with the disappearances.

Circulatory and streaming phenomena are ruled out (a) by the intensity- and contrast-levels at which disappearances occur, and (b) by the suddenness with which stimulus-objects reappear over the entire area involved.

Adaptation, usually a gradual process, may be at least partially responsible for disappearances, although the sudden reappearances of the stimulus-objects would not in itself lead to that conclusion. A point in favor of adaptation or a similar process is the preliminary indication that periodic interruption of the field reduces disappearances. The interruptions of the field are thought to disrupt any adaptive processes which might be developing in the non-interrupted period of the cycle. Other arguments for adaptation are that (1) blinking always restores the stimulus to perception, and (2) considerable time must elapse before another disappearance.

Nystagmus of fixation of the type recently observed by Ratliff and Riggs results in minute, rapid oscillatory movements of stimuli on the retina.<sup>9</sup> Adaptation of any sort should proceed in inverse proportion to the degree

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<sup>6</sup> See J. P. Guilford, 'Fluctuations of attention' with weak visual stimuli, this JOURNAL, 38, 1927, 534-583; and C. H. Graham, Visual perception, in S. S. Stevens (Ed.), *Handbook of Experimental Psychology*, 1951, 905-906.

<sup>7</sup> Floyd Ratliff and L. A. Riggs, Involuntary motions of the eye during monocular fixation, *J. Exper. Psychol.*, 40, 1950, 687-701; Visual acuity and the normal tremor of the eyes, *Science*, 114, 1951, 17-18.

<sup>8</sup> Guilford, *op. cit.*, 544-554.

<sup>9</sup> Ratliff and Riggs, *op. cit.*, 1950, 687 and 1951, 17.



of nystagmus since the motion constantly relieves stimulus-areas. Loci on or around these areas of greatest motion would be most likely to remain visible, hence a strong case would exist for the involvement of nystagmus in the disappearances, should it be found that the components of nystagmus of fixation are maximal at angles for which disappearances are minimal or that the nystagmus movement, over unadapted retinal areas at the angles of the abscissa of Fig. 1, are inversely proportional to the frequencies of disappearance at those angles. Such an effect, superimposed on effects due to adaptation, would explain both the disappearances themselves as well as the differential rate of disappearance at the various angles. The nystagmus would, in effect, modulate the adaptive process. More gross eye movements may also contribute to the effect, in the same manner.

Another explanation related to adaptation is the possible neutralization of the actual visual stimulus by its negative after-image which develops during fixation. No evidence we now have disavows this possibility. Pace observed after-images during fluctuation of attention by replacing a near-threshold stripe of light by a gray field at different phases of the fluctuation cycles.<sup>10</sup> He found that a negative after-image was observed if the replacement was made at the beginning of a disappearance but not if it was made at the beginning of a reappearance. This would seem to support the suggested possibility of stimulus-neutralization by after-images. Here again, effects of nystagmus may modulate the strength of after-images. Areas of greatest motion would be least satiated, develop least intense after-images to oppose impulses from visual stimuli, and hence disappear least frequently.

It is not unlikely that several or all of the factors mentioned contribute to disappearances. In addition there is some evidence here, as in the studies on fluctuation of attention, of a central influence. Central factors were here investigated in a manner similar to that reported by Fry and Robertson, who, however, used near-threshold stimuli.<sup>11</sup> In this connection, our observations revealed about as many disappearances under binocular as under monocular observation. In the binocular case, had each eye acted separately without central interaction or dominance, disappearances should very rarely have occurred, since, as determined from the frequency and duration of disappearances during monocular observation, only rarely would disappearances occur in both eyes simultaneously. That disappearances are ex-

<sup>10</sup> E. A. Pace, Fluctuations of attention and after-images, *Phil. Stud.*, 20, 1902, 232-245.

<sup>11</sup> G. A. Fry and V. M. Robertson, The physiological basis of the periodic merging of area into background, this JOURNAL, 47, 1935, 644-655.



perienched when they occur in either eye indicates an influence at some level beyond the optic chiasma. A second argument for central contributing factors is the dichotomy in disappearance behavior depicted in Table II. The individual differences seen there are more than one could expect from peripheral anatomical and physiological differences alone. There is, though, the possibility that differences in *S*'s interpretation of directions and differences in willingness to report marginal disappearances were primarily responsible for the dichotomy.

To sum up, our tentative general beliefs are that some form of adaptation at a peripheral level, combined with central influences, is responsible for the disappearances. We further believe that small involuntary nystagmic oscillations, the effective components of which may not be equal in all directions, modulate the peripheral effects to establish the sine-like function shown in Fig. 1.

#### SUMMARY

*Ss* fixated the center of a large, white visual field for test-periods lasting 5 min. A thin black line radiated outward from the fixation-point. This line disappeared subjectively at irregular intervals and for irregular durations during the fixation-period. Frequencies of disappearances were related to the angle of orientation of the line, according to a function which is approximately sinusoidal. Explanation of the phenomenon in terms of adaptation and nystagmus of fixation is suggested.



## THE TIME-ERROR IN THE COMPARISON OF VISUAL SIZE

By LAWRENCE KARLIN, New York University

The constant error resulting from the successive comparison of two stimuli is described as the time-order error, or more briefly, the time-error. The sign of the time-error is positive if the second stimulus of a pair is underestimated when compared to the first member; it is negative when the second stimulus is overestimated.

Current theory emphasizes the rôle of some kind of hypothetical trace which bridges the temporal gap between two successively presented stimuli. The persistently negative time-error obtained in the successive comparison of many kinds of stimuli is explained by hypothesizing that the trace of the first stimulus 'fades' during the time-interval between the two stimuli. In general, the main support for a 'fading-trace theory' derives from studies which have shown that the time-error becomes increasingly negative with increasing length of interpolated interval.<sup>1</sup>

The emphasis in theory has been on the intensive character of the fading process which in turn has led to the predominant use in experiments on time-error of stimuli varying in intensity. While presumably the trace of any kind of stimulus might 'fade,' it seems to have been more or less explicitly assumed that only in the case of intensive judgments (loudness, brightness, heaviness) would the fading trace result in the kind of decrement which corresponds most directly to the dimension of the comparison being made.<sup>2</sup> Whatever it is, then, that is assumed to fade (trace, set, memory image), apparently the term 'fading' has acquired an intensive connotation.

In the further development of theory, it would be of interest, as Köhler has implied,<sup>3</sup> to determine whether the phenomena described above are restricted to the intensive type of judgment. A partial support for such

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<sup>1</sup> Wolfgang Köhler, *Zur theorie des Sukzessivvergleichs und de zeitfehler*, *Psychol. Forsch.*, 17, 1932, 130-177; J. G. Needham, The time-error in comparison judgments, *Psychol. Bull.*, 31, 1934, 229-243; C. C. Pratt, The law of disuse, *Psychol. Rev.*, 43, 1936, 83-93; R. S. Woodworth, *Experimental Psychology*, 1938, 439-449.

<sup>2</sup> Köhler, *op. cit.*, 167; Needham, *op. cit.*, 240.

<sup>3</sup> Köhler, *loc. cit.*



restriction is given by Postman's study of the time-error for pitch and loudness.<sup>4</sup> While he confirmed Köhler's original function for loudness, he found no systematic tendency for the time-error in pitch. In evaluating this finding, it should be noted that not only are judgments of loudness intensive, but that they also belong to the larger class of judgments that involve the degree or the amount of a given quantity; on the other hand, judgments of pitch are not quantitative. In brief, pitch and loudness may be distinguished from one another as examples of dimensions which involve, respectively, qualitative and quantitative changes.<sup>5</sup> On the basis of this classification, further experiment is suggested to determine whether time-error functions are characteristic only of intensive comparisons or whether they are characteristic of quantitative comparisons in general.

The present investigation was designed in part to answer this question and is concerned with the determination of the time-error function for visual extensity or size; this dimension of comparison was selected because it is not intensive and at the same time clearly involves comparisons of a quantitative nature.

The time-error for intensive stimuli (loudness) has also been determined as a function of stimulus-duration.<sup>6</sup> Because this variable would then provide an additional dimension for the comparison of intensive with extensive stimuli, it was included in the present study. An equally important reason for its inclusion was the desire to avoid a too arbitrary choice of stimulus-duration for the determination of the interpolated interval function since stimulus-duration might influence the reliability of such determination. The full purpose of the present study, then, may be restated as the determination of the time-error for visual size as a function of stimulus-duration and interpolated interval.

#### METHOD AND PROCEDURE

*Apparatus.* An analysis of some recent studies of the time-error for visual size suggests that certain factors concerned with the mode of presenting the stimulus may have vitiated their results. In direct tachistoscopic presentation, the proximity of the tachistoscopic frame and other background contours may have been undesirable.<sup>7</sup> In

<sup>4</sup> Leo Postman, The time-error in auditory perception, this JOURNAL, 59, 1946, 193-219.

<sup>5</sup> Theodore Koester, The time-error and sensitivity in pitch and loudness discrimination as a function of time-interval and stimulus-level, *Arch. Psychol.*, 1945, (No. 297), 69 pp.

<sup>6</sup> Y. Wada, The influence of stimulus-duration upon time-errors in the discrimination of auditory intensities, *Jap. J. Psychol.*, 12, 1937, 553-563.

<sup>7</sup> P. V. Marchetti, Time-errors in judgment of visual extents, *J. Exper. Psychol.*, 30, 1943, 257-261; D. C. McClelland, Factors influencing the time-error in judgments of visual extent, *ibid.*, 33, 1943, 81-95; M. E. Tresselt, Time-errors in successive comparison of simple visual objects, this JOURNAL, 57, 1944, 555-558.



projected presentation there has been a failure to maintain uniform brightness levels and to consider the possible distraction of glare factors and changing level of adaptation introduced by presenting a bright stimulus in a dark surround.<sup>8</sup> These studies may also have neglected to consider the possible importance of controlling fixation on the stimulus.<sup>9</sup> On the basis of these and other considerations, the apparatus in the present study was designed to provide: (a) uniform illumination at all times; (b) a large, homogeneous background; (c) constant locus of the projected image; and (d) control over S's head movements.

The apparatus consisted essentially of two projection tachistoscopes mounted on a swivel base and a set of timers and solenoids which operated the tachistoscope shutters.

Each projector was a 300-w., fan-cooled,  $2 \times 2$  slide projector manufactured by the TDC Company. The right-hand projector presented the stimulus-material, while the left-hand projector maintained the same level of illumination between presentations. Stimulus-duration was controlled in each projector by a shutter assembly mounted on the objective of each projector. Each shutter assembly consisted of an Ilex No. 3 Universal Shutter in which the shutter trigger was connected by a special linkage to a solenoid plunger.

The shutters were synchronized by a timing relay system which simultaneously energized one solenoid and deenergized the other. The energized solenoid activated a plunger which tripped the shutter mechanism. In this manner the screen was always illuminated by one projector. This synchronization of the two shutters accomplished the transition from one projector to another without any noticeable flicker. To an observer on the other side of the trans-lux screen, the effect was that of an image appearing and disappearing on a ground of constant brightness.

The stimuli were projected upon the center of a trans-lux screen, 2 ft. sq., which partially bounded the front of a completely enclosed booth lined with black muslin.

A head-rest within the booth was fixed and centered with respect to the screen. With the head-rest remaining fixed, adjustments for sitting height were so made with an adjustable chair that the head of each S, when placed within the head-rest, was in a relatively constant position with respect to the screen. The head-rest was 57 in. from the screen.

The timing-apparatus consisted of four synchronous, motor-driven timers connected to provide automatic repetition of any combination of four intervals in sequence.

*Stimulus-materials.* The stimulus-materials consisted of two glass-bound slides made on 35 mm. film. The projected figure was a black, surface circle, 92 mm. in diameter, appearing on a clear ground. A blank slide developed to the same density was used during the rest-periods and interpolated intervals.

*Procedure.* The method of successive comparison was used with stimulus-duration and interpolated interval constant for each session. The stimulus-durations were 1,

<sup>8</sup> McClelland, *op. cit.*, 82; Tresselt, The time-error in visual extents and areas, *J. Psychol.*, 17, 1944, 21-30.

<sup>9</sup> Control of fixation is of definite theoretical importance since the locus of the trace is an important factor in Gestalt trace-theory. See Otto Lauenstein, *Ansatz zu einer physiologischen theorie des Vergleichs und der Zeitfehler*, *Psychol. Forsch.*, 17, 1932, 130-177.



3, and 5 sec. and the interpolated intervals were 1, 4, and 8 sec. in length. A rest-period of 15 sec. between each pair of stimuli was used throughout. Stimulus-duration was varied only for the first stimulus of a pair. The duration of the second stimulus was always constant at 1 sec.

Nine naïve Ss were used. Each S was tested only once on any day and attended a total of 9 sessions at each of which 35 judgments were made. Three categories of judgment, louder, equal, and softer, were used.

The order of the nine combinations of durations and intervals was arranged so that each combination occurred only once for each S, and only once on any day.

The sequence of conditions was so arranged that over all the Ss every condition followed every other an approximately equal number of times.

No fixation-point was used since the stimulus was presented foveally and it was desired to keep the field clear of any extraneous objects. It is believed, however, that the nature of the task and the constant position of S's head in the head-rest secured some control over fixation.

Instead of a variable second stimulus, the same stimulus was used as both standard and comparison stimulus. Since the purpose of this experiment was to measure the direction and amount of the time-error, it was simpler and more economical in total time to eliminate the variable stimuli ordinarily used in experiments using the method of constant stimuli for securing measures of precision.

## RESULTS

Individual time-errors were computed from the formula:  $50 - (100)(L + 0.5E)/N$  where  $N$  is the total number of judgments,  $E$  the number of 'equal' judgments, and  $L$  the number of 'larger' judgments.<sup>10</sup> Due to

TABLE I  
MEAN TIME-ERROR AND SD BY STIMULUS-DURATION  
AND INTERPOLATED INTERVAL  
(The time-error for each condition is the mean of nine time-errors.)

Duration (sec.)	Interval						Mean
	1 sec.		4 sec.		8 sec.		
	M	SD	M	SD	M	SD	
1	-5.7	11.9	-7.5	6.5	-8.5	12.8	-7.2
3	-0.8	8.3	-1.4	10.7	-7.9	7.0	-3.4
5	-3.5	14.0	-4.1	6.9	-10.8	10.9	-6.1
Mean	-3.3		-4.3		-9.1		

limitations of space, only the means and standard deviations of the individual time-errors are shown. Individual time-errors are given in the original dissertation on file in the New York University library.

The major results are summarized in Table I where the mean time-errors

<sup>10</sup> This formula is equal to one-half of Lauenstein's measure of the time-error (D%). See Lauenstein, *op. cit.*; and Koester, *op. cit.*



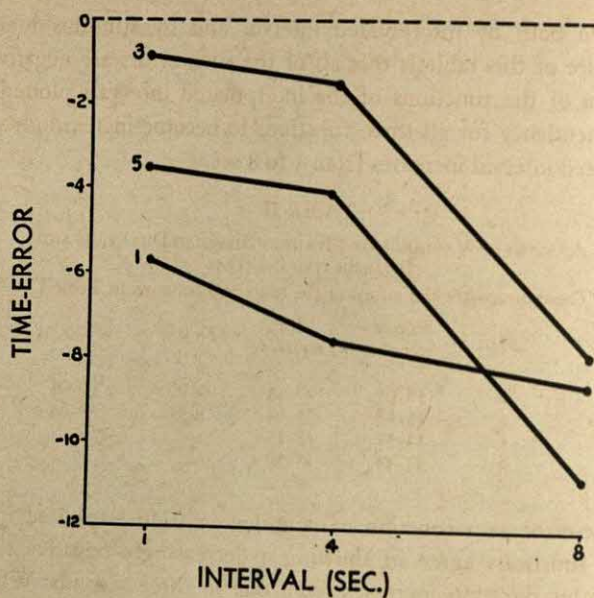


FIG. 1. TIME-ERROR AS A FUNCTION OF INTERPOLATED INTERVAL.

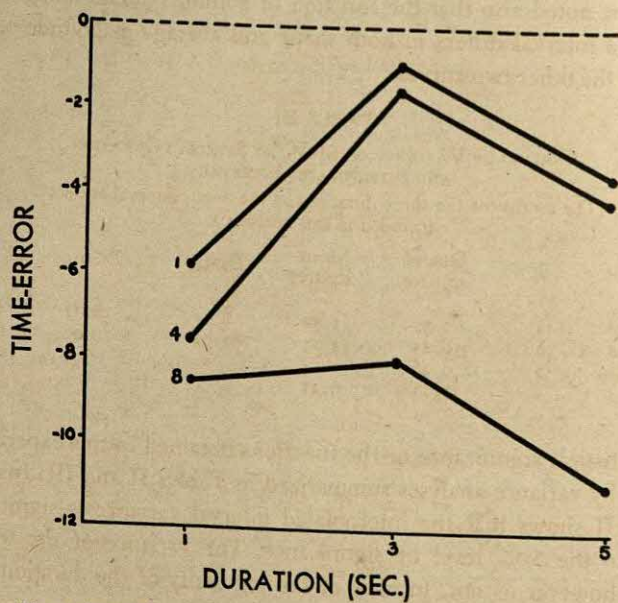


FIG. 2. TIME-ERROR AS A FUNCTION OF STIMULUS-DURATION.



are tabulated both by interpolated interval and by stimulus-duration. A salient feature of this table is that all of the time-errors are negative.

Inspection of the functions of the interpolated intervals plotted in Fig. 1 reveals a tendency for all three functions to become increasingly negative as interpolated interval increases from 1 to 8 sec.

TABLE II  
ANALYSIS OF VARIANCE OF MEANS BY STIMULUS-DURATION AND  
INTERPOLATED INTERVAL  
(These means are the means of the rows and columns in Table I.)

Source of variation	df	Sum of squares	Mean squares	F-ratio	$F < 0.05$	$F < 0.01$
Intervals	2	56.65	28.33	9.06	6.94	18.00
Durations	2	23.28	11.64	3.72	6.84	18.00
Residual	4	12.51	3.13			
Total	8	92.44	11.56			

The time-error as a function of stimulus-duration is plotted in Fig. 2. The three functions agree in showing a decreasingly negative time-error when stimulus duration increases from one to three seconds. When stimulus duration is further increased from three to five seconds, the trend is reversed and the time-error in all three functions becomes more negative. It should be noted also that the function of stimulus-duration at the 8-sec. interpolated interval differs in both shape and average magnitude of time-error from the other two curves.

TABLE III  
ANALYSIS OF VARIANCE OF MEANS BY STIMULUS-DURATION  
AND INTERPOLATED INTERVAL  
(The means for the three durations at the 8-sec. interval are not included in this analysis.)

Source of variation	df	Sum of squares	Mean square	F-ratio	$F < 0.05$	$F < 0.01$
Intervals	1	1.50	1.50	6.25	18.51	98.49
Durations	2	30.25	15.13	63.04	19.00	99.01
Residual	2	0.48	.24			
Total	5	32.23	6.45			

The statistical significance of the functions obtained in this experiment is evaluated by variance analyses summarized in Tables II and III. Inspection of Table II shows that the interpolated interval variance is significant at better than the 5-% level of significance. The variance of the stimulus-duration, however, is not. In view of the regularity of the duration-trends, further analysis is suggested.



Inspection of Fig. 1 indicates that the duration-function at the 8-sec. interpolated interval, while resembling the other two functions, does not have a slope like the other two. The difference in slope suggests some kind of interaction of interval with duration not present with the shorter interpolated intervals. The net effect in this case may be to increase the residual variance so that it does not provide a valid estimate of error variance but rather a significant interaction of interval with duration. This conclusion is confirmed by the results of the variance analysis summarized in Table III. These results demonstrate that with omission of the durations obtained at

TABLE IV  
MEAN TIME-ERROR BY EXPERIMENTAL DAY

	Day								
	1	2	3	4	5	6	7	8	9
Mean	-8.4	-7.6	-8.6	-9.4	-4.4	-2.7	-4.0	-3.2	-2.1
SD	10.3	11.4	7.9	7.2	12.8	9.6	10.3	14.1	10.6

the 8-sec. interpolated interval, the variation due to duration is highly significant. On the other hand, it is noted that variation due to interpolated interval is now not significant. The reason for this result is disclosed on examination of the curves in Fig. 1. Variation in the time-error, while uniform for all three functions, is rather small when interpolated interval changes from 1 to 4 sec., and this variation is all that is reflected in the variance analysis in Table III. Apparently, the uniform but slight trend from 1 to 4 sec. is not large enough to yield significance with this test.

The data shown in Table IV indicate a tendency, marked by numerous irregularities, toward a decrease in negativity with increasing practice.

#### THEORETICAL IMPLICATIONS

*Intensive vs. extensive stimuli.* The interpolated interval functions obtained for various kinds of intensive stimuli have often displayed a tendency to be positive for interpolated intervals shorter than three seconds.<sup>11</sup> In this respect, the functions obtained in the present study are different, since a negative time-error was noted for all functions at the 1-sec. interval. It is clear, however, that the present functions are similar to the intensive functions in that both display a trend to increasing negativity.

This last result lends support to the possibility mentioned in the intro-

<sup>11</sup> G. L. Freeman and L. H. Sharp, Muscular action potentials and the time-error function in lifted-weight judgments, *J. Exper. Psychol.*, 29, 1941, 23-36; Lauenstein, *op. cit.*, 160; Postman, *op. cit.*, 204.



duction; namely, that the function of the negatively directed time-error is not uniquely a characteristic of judgments of intensity but of judgments of magnitude in general. These results perhaps suggest that some common factor underlying all types of quantitative judgments is operative. It would be fruitless, however, to indulge in speculation about the nature of such a factor before the validity of this hypothesis is checked by further experiment with other types of stimuli in the various modalities.

*Trace theory.* There are, however, certain other theoretical aspects more specific to the type of stimulus employed in the present study which may provide a theoretical link between the intensive and extensive mechanisms involved in the time-error for visual stimuli. These theoretical possibilities arise from the consideration of Köhler's fading trace theory of the time-error and the Gestalt theory of gamma movement.<sup>12</sup> Before discussing this relationship specifically, Köhler's theory will be briefly described.

Köhler hypothesized a physiological after-effect or trace of initial stimulation consisting of a cortically localized concentration of ions. The autonomous diffusion of these ions resulting in a diminishing trace potential constituted the fading process.<sup>13</sup> The judgment of comparison itself was based on the potential gradient created by the potential difference between the excitation process of a second stimulus and the trace of the first stimulus. Whether the second stimulus was judged to be greater or less than the first depended on the direction ('ascending' for negative, 'descending' for positive time-errors) of the gradient that 'sprang up' with the appearance of the second excitation. The negatively directed function of time-error was attributed to the progressive fading of the trace which resulted in the establishment of a progressively steeper 'ascending' gradient with the passage of time.

Köhler's isomorphism refers to the potential gradient as the basis of intensive comparisons only. His rationale does not include or imply, as he himself states, any reference to other dimensions of sensation. The results of the present study could, therefore, not have been predicted from his theory and, hence, neither support nor refute it. If we adopt the pattern of theorizing used by Köhler in trying to infer from the behavioral data the corresponding 'brain' events, the data of the present experiment could be interpreted to mean that 'shrinkage' as well as 'fading' was characteristic of autonomous trace changes. *Ad hoc* theorizing of this kind, however, is of little value if it does nothing for the theory except to add another concept for each new finding. Considerations of parsimony require that an independent theoretical basis be found for the inference that traces shrink as well as fade.

It is possible that such a basis may be found in the Gestalt explanations

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<sup>12</sup> Erich Lindemann, Gamma movement, in W. D. Ellis (ed.) *A Source Book of Gestalt Psychology*, 1950, 173-181. L. Hartmann, Further studies of gamma movement, *ibid.*, 182-191.

<sup>13</sup> The term 'potential' refers to some form of energy, not necessarily electrical, at a higher level than the undisturbed cortical level. See Lauenstein, *op. cit.*, 143.



of gamma movement.<sup>14</sup> This type of illusory movement involving the expansion of a figure following its appearance and of contraction following its disappearance, has been attributed to similar changes in cortically localized excitation.<sup>15</sup> If it be assumed that the contraction process leaves the trace smaller when the external stimulus was present, it would account for the negative time-error found even at the shortest interval in this study. It would have to be further assumed that the trace shrinkage, which started with the disappearance of the figure, continued during the interpolated interval.

In the present study, the rapid transition of the shutter (about 0.001 sec.) was much faster than the transition required for optimal experience of gamma movement.<sup>16</sup> The fact that conditions were not optimal for its experienced occurrence does not, however, preclude its application to the present results since the 'physiological' gamma-process may, as noted by Lindemann,<sup>17</sup> always occur, regardless of whether or not it is experienced.

The above discussion suggests that exploration of the relationship between gamma-movement phenomena and the time-error may prove fruitful. It might be predicted that the conditions which enhance gamma movement will also enhance the time-error.

*Stimulus duration.* The results obtained when stimulus-duration is varied suggest that this variable is a significant parameter of the comparative judgment. In a general way, a part of these results are similar to those obtained by Wada in his study of the relationship between the time-error and stimulus duration for auditory intensities.<sup>18</sup> His stimulus-durations varied from 0.25 to 2.5 sec. For this range of values his results resemble ours in finding that positive time-errors increased and negative time-errors decreased. Whether there would be a reversal in trend beyond 3 sec., as was obtained in the present study, remains to be seen. At any rate, the reversal in trend found in the present study (see Fig. 2) may not be significant but, as suggested, may represent a random fluctuation about a curve that actually begins to level off beyond 3 sec. Working out the shape of this curve in greater detail should precede further speculation about the significance of stimulus-duration as a parameter of the time-error.

<sup>14</sup> The possible relation between gamma movement and the time-error has been noted by McClelland, *op. cit.*, 89. He discussed its effect not in terms of hypothetical trace change but rather in terms of its influence on the last impression of the first stimulus of a pair.

<sup>15</sup> Hartmann, *op. cit.*, 189.

<sup>16</sup> Lindemann, *op. cit.*, 24; E. B. Newman, Versuche über das Gamma-Phänomen, *Psych. Forsch.*, 19, 1934, 102-121.

<sup>17</sup> Lindemann, *op. cit.*, 181.

<sup>18</sup> Wada, *op. cit.*, 555.



## SUMMARY AND CONCLUSIONS

The results of the present study suggest that the time-error for the successive-comparison of circles becomes increasingly negative with increasing length of interpolated interval. It was pointed out that these functions are similar to those obtained in previous studies for stimuli varying in intensive magnitude. It was concluded that the function of the negatively directed time-error may be characteristic of judgments of magnitude in general. These results were further discussed in terms of possible relationships to Gestalt theory based on the phenomena of gamma movement.

It was also found that the time-error tends to become less negative when stimulus-duration is changed from 1 to 3 sec.; there may be a tendency for this trend to reverse when stimulus-duration further increases from 3 to 5 sec.



## FREQUENCY OF EXPERIENCE VERSUS ORGANIZATION AS DETERMINANTS OF VISUAL THRESHOLDS

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A considerable amount of experimental work has recently been brought to bear upon the rôle in perception of many so-called 'dynamic' factors such as value,<sup>1</sup> need,<sup>2</sup> emotion,<sup>3</sup> tension,<sup>4</sup> stress,<sup>5</sup> set,<sup>6</sup> and personality characteristics.<sup>7</sup> A great deal of experimental evidence has been accumulated—evidence which has established a number of relations between these 'behavioral units and perception—but for the most part this work has remained primarily empirical in character. While the reports which have appeared have contributed a great deal of evidence to support a common conviction that various kinds of past experience influence perceptual processes, a theory concerned with the underlying functional relationships has not yet been explicitly advanced. Indeed, such a theoretical framework was for a time avoided in an attempt, primarily by Bruner and his co-workers, to build up a body of facts from which to work. Recently, however, interest in theory has been revived and attention has been focused upon the need

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<sup>1</sup> J. S. Bruner and Leo Postman, Symbolic value as an organizing factor in perception. *J. Soc. Psychol.*, 27, 1948, 203-208; Postman, Bruner, and Elliott McGinnies, Personal values as selective factors in perception, *J. Abnorm. Soc. Psychol.*, 43, 1948, 142-154.

<sup>2</sup> Bruner, and C. C. Goodman, Value and need as organizing factors in perception, in Theodore Newcomb and E. Hartley, *Readings in Social Psychology*, 1947, 99-108; D. C. McClelland and A. M. Liberman, The effect of need for achievement on recognition of need-related words, *J. Personal.*, 18, 1949, 236-251.

<sup>3</sup> Bruner and Postman, Emotional selectivity in perception and reaction, *J. Personal.*, 16, 1947, 69-77; McGinnies, Emotionality and perceptual defense, *Psychol. Rev.*, 57, 1950, 235-240.

<sup>4</sup> Bruner and Postman, Tension and tension release as organizing factors in perception, *J. Personal.*, 15, 1947, 300-308.

<sup>5</sup> Postman and Bruner, Perception under stress, *Psychol. Rev.*, 55, 1948, 314-324.  
<sup>6</sup> Postman and Bruner, Multiplicity of set as a determinant of perceptual organization, *J. Exper. Psychol.*, 39, 1949, 369-377.

<sup>7</sup> E. Hanfmann, M. I. Stein, and Bruner, Personality factors in the temporal development of perceptual organization, *Amer. Psychol.*, 2, 1947, 284-285; Roy Shafer, and Gardner Murphy, The rôle of autism in a visual figure-ground relationship, *J. Exper. Psychol.*, 32, 1943, 335-343.



for a theoretical system which would give meaning to the results of the earlier empirical studies.

An examination of the framework of these papers reveals either broad concepts which yield few specific deductions or a list of determinants derived from specific results which have little general applicability. While the empirical validity of the modern studies has not seriously been questioned, considerable discussion has appeared regarding the interpretation of the data. Luchins has called attention to the *ad hoc* character of the concepts of *resonance*, *vigilance*, and *defense* advanced by Postman, Bruner, and McGinnies, and he has noted that the principle of *selectivity* put forth by these same investigators is not amenable to experimental verification.<sup>8</sup> In a recent paper Howes and Solomon have severely criticized McGinnies' hypothesis about *perceptual defense*, pointing out that his data could be accounted for in terms of word frequency, a variable which was not controlled in his experiment.<sup>9</sup>

Such factors as word-probability and frequency of experience have been shown to be important in a number of experiments,<sup>10</sup> but as an explanatory principle frequency has received little acceptance among adherents of a more 'dynamic' approach. Bruner and Postman have presented data which support the view that single repetitions of instructions may greatly overrule the effects of frequent experience, and they have denied that the empirical findings can be accounted for in terms of frequency alone.<sup>11</sup> While each side of the question has some credence, there has been little agreement among these investigators as to the best method of ending disagreement.

In a recent attempt to organize the problem of motivation and its rôle in perception in terms of a single conceptual framework, Hochberg and Gleitman have suggested the relevance of the trace theory of Gestalt psychology;<sup>12</sup> but their orientation toward the problem, like that of Bruner and Postman, is inductive, and they do not explore the deductive implications of the theory. In the same vein Krech, too, has noted that the organizational principles of Gestalt psychology may "generalize . . . to behavior units different than those for which those principles were originally invented."<sup>13</sup> Like Hochberg and Gleitman, however, he does not attempt to deduce empirical results from the principles. In his recent critique Luchins has also suggested that the trace theory may provide a basis for an understanding of the new data on threshold.<sup>14</sup>

If the implications of Gestalt theory are examined critically, it is possible

<sup>8</sup> A. S. Luchins, On an approach to social perception, *J. Personal.*, 19, 1950, 64-84.

<sup>9</sup> D. H. Howes, and R. L. Solomon, A note on McGinnies' 'Emotionality and perceptual defense,' *Psychol. Rev.*, 57, 1950, 229-234.

<sup>10</sup> K. W. Braley, The influence of past experience in visual perception, *J. Exper. Psychol.*, 16, 1933, 613-643; Mary Henle, An experimental investigation of past experience as a determinant of visual form perception, *ibid.*, 30, 1942, 1-21; W. Lambert, Solomon, and P. D. Watson, Reinforcement and extinction as factors in size estimation, *ibid.*, 39, 1949, 637-641.

<sup>11</sup> Bruner and Postman, Perception, cognition, and behavior, *J. Personal.*, 18, 1949, 14-31.

<sup>12</sup> J. E. Hochberg, and Henry Gleitman, Towards a reformulation of the perception-motivation dichotomy, *J. Personal.*, 18, 1949, 180-191.

<sup>13</sup> David Krech, Notes toward a psychological theory, *J. Personal.*, 18, 1949, 66-87.

<sup>14</sup> *Op. cit.*, 64-84.



to make at least some rudimentary deductions concerning perceptual phenomena such as those emphasized in more recent researches. While it has generally been assumed that the differential effects obtained in the modern threshold experiments may be attributed to the 'dynamic' character of the variables studied, Koffka's theory suggests that these effects may not be dependent upon the operation of affective or motivational forces *per se*.<sup>15</sup> Central to Koffka's approach to the problems of memory is the principle of communication between process and trace. The structure of trace-systems is determined, according to Koffka, by the structure of the processes which give rise to them, and, conversely, the structure of a new process may be influenced by the structure of a trace-system with which it communicates. Koffka has assumed that a stable trace-system may strengthen or lend stability to a poorly organized process. This formulation makes it possible to understand why the tachistoscopic recognitive threshold of a stimulus-word congruent with a dominant value-system (which, following Koffka's argument, may be considered as a stable trace-system) should be lower than that of a more 'neutral' word. The tachistoscopic perception of *any* word—*affectively loaded or neutral*—must, from this point of view, be facilitated, provided only that it is brought into communication with a stable trace-system.

The experiment to be reported here represents a test of a single deduction from this general proposition; namely, that differences in recognitive thresholds of stimulus-materials may be predicted from the laws of organization as they apply to learning and recognition. The assumption that organized traces strengthen and lend stability to subsequent processes leads to the proposition that stimulus-materials will be more readily recognized if they have been experienced in an organized manner than if they have not. More specifically, if Ss learn to associate pairs of items in such a way that half the pairs are associated in an organized manner and half are not, *i.e.* associated in an unorganized manner, recognitive thresholds for the organized items should be lower. The reduction in threshold attributable to organized experience should be superimposed upon any reduction in threshold attributable to frequency alone.

#### EXPERIMENT

The aim of the present study was to determine the effect of organized experience with affectively neutral materials upon the tachistoscopic recognitive thresholds of the materials in relation to the effect of relatively unorganized experience with similar materials equally often experienced.

<sup>15</sup> Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 429-569.



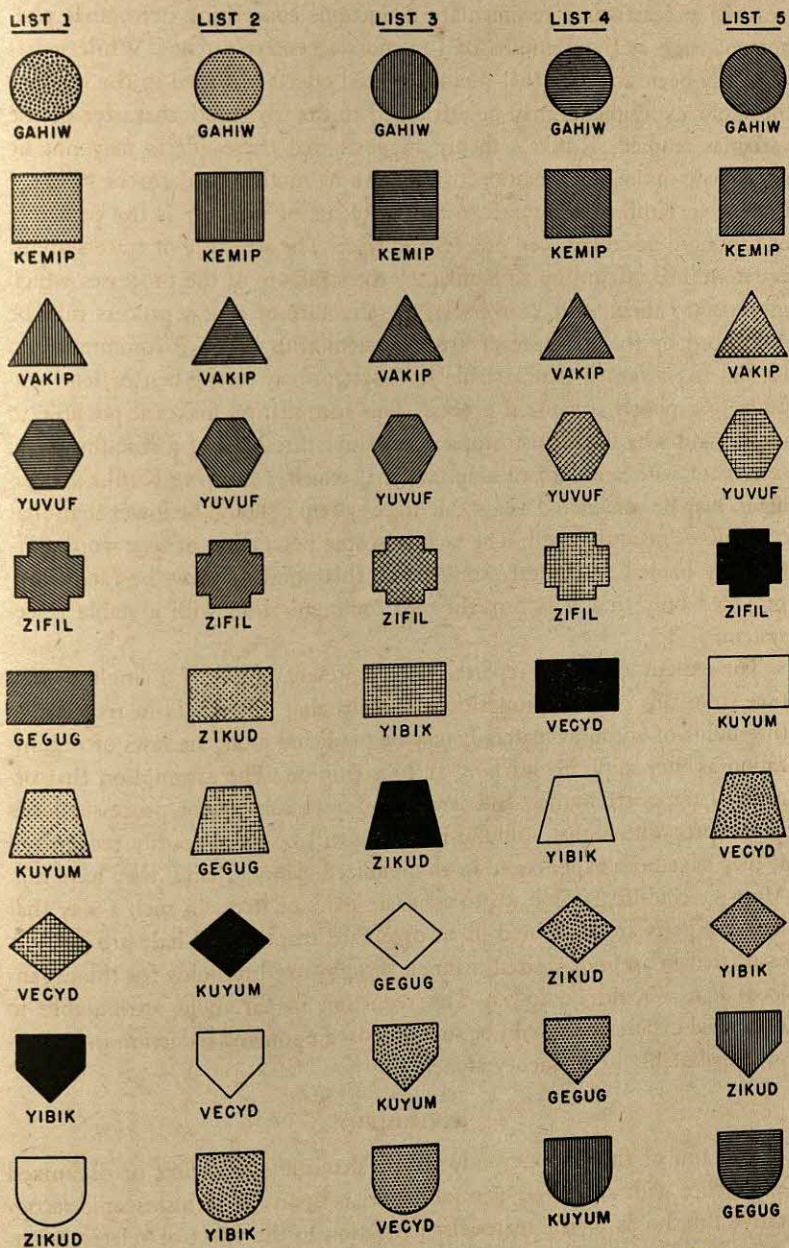


FIG. 1. SAMPLE LISTS OF FIVE PAIRED-ASSOCIATED LEARNED BY ONE SUB-GROUP OF THE SS



*Method and procedure.* Nonsense paralogs and geometric forms, representing the affectively neutral materials, were used. The experiment consisted of (a) a preliminary learning situation to provide controlled experience—organized and unorganized—with the stimulus materials and (b) a determination of the tachistoscopic recognitive thresholds of the materials. In the preliminary situation the Ss learned five lists of paired associates. Each list consisted of 10 pairs, and each pair consisted of a stimulus-figure, of a given shape and texture, and a response-paralog. Each of the 10 shapes, textures, and paralogs appeared equally often in each of the five lists and only once in each list. Each of five of the paralogs was associated with a figure varying in both shape and texture from list to list. This procedure is illustrated in Fig. 1, which represents the lists as they were learned by one sub-group of five Ss. In the learning situation the serial order of the paralog-figure pairs was varied systematically from trial to trial on each list as is customary in paired-associates training. An examination of Fig. 1 shows that five of the paralogs were always associated with shape in a consistent, and therefore organized, manner and that five of the paralogs were also associated with shape, but in an inconsistent, and therefore unorganized, manner. Frequency of experience with the materials in the learning situation was held constant for all the learned paralogs since each stimulus-figure and response-paralog appeared equally often.

TABLE I  
PARALOGS USED IN THE EXPERIMENT

GAHIW	GEGUG	GOJEY
KEMIP	KUYUM	KEREP
VAKIP	VECYD	VAVUK
YUVUF	YIBIK	YILIM
ZIFIL	ZIKUD	ZEWUH

After the five lists were learned in succession, each to a criterion of one correct anticipation of the entire list, and after a rest-period of 10 min., tachistoscopic recognitive thresholds were determined for the 10 learned paralogs and for a control group of 5, previously not experienced, paralogs. The thresholds were determined in the conventional manner, with one ascending series of exposures (10, 20, 30, m.sec., etc.) for each paralog until S identified it correctly. All of the paralogs used in the experiment are listed in Table I.

To minimize the effect of any structural differences among the paralogs which may have resulted in threshold differences independent of the experimental condition, six groups of Ss were employed. Each group of five paralogs shown in Table I served equally as the organized, unorganized, and not experienced materials.

The Ss were 30 students. A conventional memory-drum was employed for presentation of the materials in the learning situation, and a Marietta projection-tachistoscope was used for determination of the recognitive thresholds.

## RESULTS AND DISCUSSION

Three principal kinds of data were recorded: (1) the correct anticipations of the paralogs during learning of the paired associates; (2) the recognition threshold of each of the 15 paralogs; and (3) the reports of the Ss.



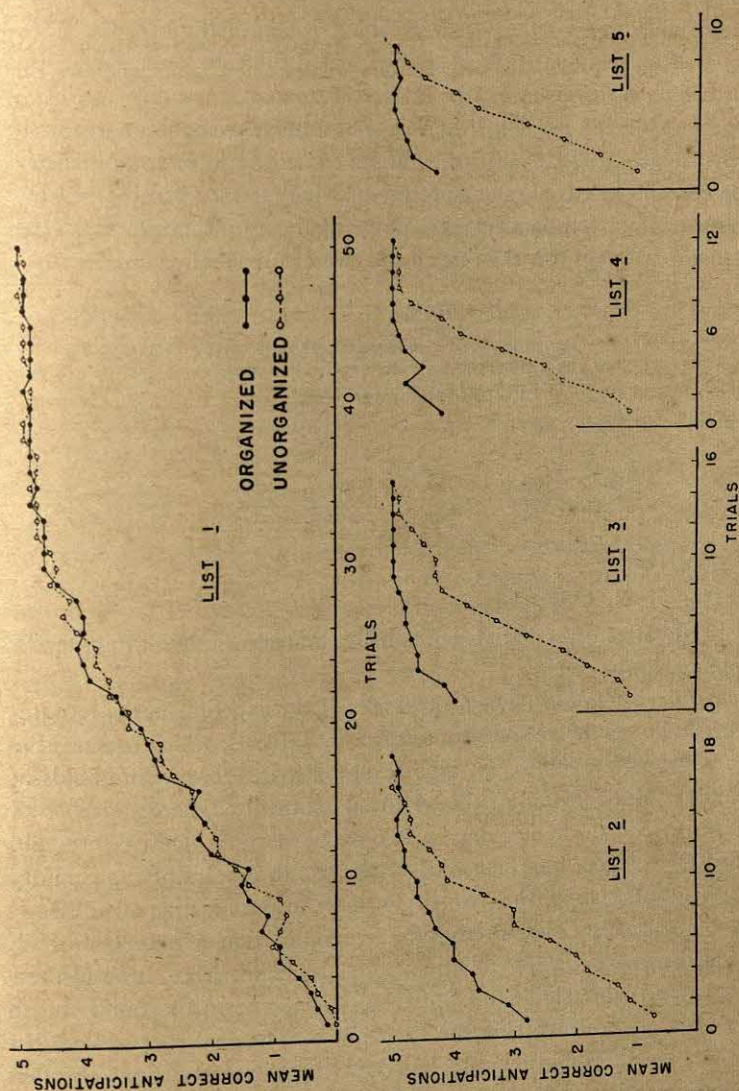


FIG. 2. LEARNING CURVES FOR THE ORGANIZED AND UNORGANIZED PARALOGS



*Learning curves.* For the total group of Ss the mean number of correct anticipations of the organized paralog and the unorganized paralog was computed for each trial on each list, and curves were plotted as shown in Fig. 2. Examination of these curves shows that both the organized and the unorganized paralog presented about equal difficulty on the first list. On succeeding lists, however, when differences in organization were introduced, the organized paralog had a clear advantage over the unorganized ones. Ss usually correctly anticipated at least one or two of the organized paralog on the first trial, and some Ss, by the time they had reached the third list, had correctly anticipated all five of them on the first trial. The unorganized paralog, on the other hand, were consistently correctly anti-

TABLE II  
MEANS, SD, AND SIGNIFICANCE OF THE DIFFERENCES AMONG THE THRESHOLDS  
OF THE PARALOGS AS A FUNCTION OF TREATMENT  
IN THE LEARNING SITUATION

Treatment	Mean threshold*	SD	SE <sub>m</sub>	Comparison	SE <sub>md</sub> †	t
	(m. sec.)					
Organized	201.7	48.4	9.10	organ. vs. unorgan.	8.80	6.023‡
Unorganized	254.7	68.4	12.71	organ. vs. unexper.	10.24	14.551‡
Unexperienced	350.7	69.6	12.92	unorgan. vs. unexper.	8.73	10.996‡

\* For 30 Ss with five paralog for each.

† For correlated scores.

‡ Significant beyond the 1% level of confidence.

ipated only after several trials on each list, and were learned to criterion much more slowly.

*Recognitive thresholds.* The main results of the experiment, the recognitive thresholds of the paralog, are shown in Table II, which contains the means, standard deviations, and significance measures for the thresholds of the paralog as a function of the treatments (organized, unorganized, and not experienced) in the learning situation. The mean recognitive threshold is based in each case upon the total of the five thresholds of the paralog for each S and computed across the 30 Ss. An examination of Table II shows that there is a clear decrease in recognition-time as a function both of frequency of experience and also of organized experience. Both of these decreases are highly significant. In Table III are shown the results of an analysis of variance involving the 15 paralog. The analysis revealed that the effects on thresholds due to individual paralog (structural or other differences peculiar to the paralog themselves) are insignificant.

*Reports.* An examination of the reports of the Ss revealed no clear evidence that, considered from the point of view of structure, any particular paralog was either more easily associated in the learning situation or more



readily recognized in the tachistoscope. Ss reported that some paralogos were more easily learned than others, and that some of the paralogos were more easily recognized; but further questioning revealed that it was the way in which the paralogos were treated in the learning situation, rather than any structural effect, that was identified as the contributing factor. In almost every case the reason given by the Ss for what they considered to be better performance could be paraphrased by the statement, "Paralog X seemed easier to see. I think it was the one that was always paired with Figure Y." The Ss, when asked which of the paralogos seemed easier to learn, usually were able to name at least one or two of the organized paralogos, but seldom

TABLE III  
ANALYSIS OF VARIANCE (TWO WAY CLASSIFICATION) OF THE THRESHOLD DATA  
ARRANGED ACCORDING TO INDIVIDUAL PARALOGS AND  
Ss' CLASSIFICATION FOR CORRELATED SCORES

Source of variance	Sum of squares	df	Variance estimate	F
Paralogos	71.85	14	5.132	1.179
Ss	576.02	29	19.863	4.566*
Residual	1766.15	406	4.350	
Total	2414.02	449		

\* Significant beyond the 1% level of confidence.

named any of the unorganized ones. One S recalled all five of the organized paralogos, but when asked to try to recall more, he said, "I can't. They kept changing on me." It was clearly evident from the reports that after learning the lists, when the paralogos had come to represent (in the experimental situation, at least) a definite shape, irrespective of its texture, they were both more easily recalled and more readily recognized in the tachistoscope. This finding lends further support to the interpretation based on the learning curves and the recognition-data.

The results show clearly that the recognitive thresholds for the paralogos were joint functions both of frequency of past experience with the paralogos and of the organizational character of that experience. The effect of frequency alone stands out clearly in the data, but those paralogos which were consistently associated with shape were more readily recognized than those which were inconsistently associated with shape when frequency was held constant. This finding follows deductively from the assumption that, with frequency controlled, organized trace systems are more stable than unorganized ones, and a stable trace system may lend stability to poorly organized or structured processes. It is possible to argue from an analysis of the learning curves derived from the first part of the experiment, that those



paralogs which we have termed 'organized' may also have been reinforced more often than those called 'unorganized' since they were anticipated correctly more often. This argument, however, would need to posit the assumption that correct anticipation of the paralog would be reinforcing to a new situation in which the paralog, previously a response, is now a stimulus. Furthermore, negative evidence of this situation has been found in a preliminary study by the author. The factor of differential reinforcement was controlled in a learning situation where items were dropped from each list immediately after the first correct anticipation of each paralog. Under this condition, results similar to those found in the present study were obtained.

Independent evidence for the validity of the assumptions made here is provided by a related experiment by Ortner.<sup>16</sup> Tachistoscopic thresholds for isolated and non-isolated items in serial lists of nonsense materials constructed after the method of Restorff were compared,<sup>17</sup> and differences in favor of the isolated materials were found. In this case isolation was the factor responsible for the differential trace stability, but again the results may be interpreted in terms of the communication between process and trace.

From the interpretation of these findings it is suggested that the trace theory of Gestalt psychology may provide a framework for the integration of the data on the rôle of motivational and affective forces in perception with the results of traditional work with affectively neutral materials. While at the present time the principles of the traditional psychologies of perception have provided only a rudimentary beginning, it is expected that the results of the modern studies will fit these general principles of trace theory; and it is conceivable (and to be hoped) that the principles will themselves be further refined in the process of fitting the new data to them.

#### SUMMARY

The studies of Bruner and others have provided empirical evidence for the rôle of a variety of 'dynamic' factors in perception. It is here proposed that the results of many of these recent studies may be understood in terms of the organized character of the trace systems involved rather than in terms of processes peculiar to motivational or affective variables. An attempt is made to relate the organizational principles of Gestalt psychology to association and recognition, and to demonstrate their effects in a general way.

<sup>16</sup> Astri Ortner, Nachweis der Retentionsstörung beim Erkennen, *Psychol. Forsch.*, 22, 1937, 59-88.

<sup>17</sup> Hedwig von Restorff, Über die Wirkung von Bereichsbildungen im Spurenfeld: Analyse von Vorgängen im Spurenfeld, *Psychol. Forsch.*, 18, 1933, 299-342.



Results are deduced for an investigation in which cognitive thresholds of affectively neutral nonsense materials are predicted to be a function of the organized properties of experimentally established trace systems.

An experiment is reported in which Ss learned to associate paralogs with geometric figures under conditions which provided for the development of (a) organized and (b) unorganized trace systems. Frequency of past experience was held constant and structural differences among the paralogs were counterbalanced. The results confirmed the deduction that cognitive thresholds of the paralogs which were experienced in a consistent, and therefore organized, manner were lower than the thresholds of the paralogs which were experienced in an inconsistent, and therefore unorganized, manner. Although the difference cannot be accounted for in terms of frequency alone, the importance of this factor was evidenced by the fact that the thresholds for both sets of paralogs were lower than those for a new set introduced for the first time in the tachistoscopic phase of the experiment. The results are interpreted to suggest that trace theory provides a basis for understanding the relation between the results of experiments with 'dynamic' variables and the traditional psychology of perception.



## THE RÔLE OF BRIGHTNESS IN PRIMARY SIZE-DISTANCE PERCEPTION

By E. LAURENCE CHALMERS, JR., Princeton University

Recent experiments upon the rôle of brightness in depth perception by Mueller and Lloyd,<sup>1</sup> Berry, Riggs, and Duncan,<sup>2</sup> and others have indicated that the threshold for the discrimination of depth increases as brightness is decreased. Further than this, Berry, Riggs, and Duncan have compared the thresholds for depth discrimination with thresholds for visual acuity where thresholds are in terms of the angle measured at a single eye. This comparison has shown that while both types of thresholds increase in magnitude as the brightness of the stimulus-objects is decreased, the threshold for visual acuity increases more rapidly until its magnitude is approximately that of the threshold for depth discrimination. This occurs at a brightness of 0.04 or 0.07 millilamberts. This finding led the authors to conclude, "that at the lowest illuminations the essential factor in . . . real depth discrimination is the retinal resolution of the single eye."<sup>3</sup>

The present experiment was designed further to investigate these findings within the setting of the problem of size-constancy. In the experiments cited the stimulus-objects were usually within a few feet of *O*. The task was either to place a comparison object at the same distance as one or more standard objects, or to indicate which of two stimulus-objects was nearer to or farther from *O*. In the present investigation *O* was asked to indicate when the size of a comparison-object was the same apparent physical size as a standard object. These standards were at distances of 10 to 120 ft. from *O*. Nothing other than the stimulus-objects was visible, and an attempt was made to eliminate the secondary cues. The brightness of the stimulus-objects was 25 foot lamberts for one-half of the judgments (above the level at which vernier or depth discrimination begins to decline rapidly) and 0.025 foot lamberts for the remaining judgments (below the lowest brightness in the Berry, Riggs, and Duncan experiment).

\* Accepted for publication October 3, 1952. This study is a continuation of research financed by a grant from the Rockefeller Foundation to the Department of Psychology of Princeton University.

<sup>1</sup> C. G. Mueller and V. V. Lloyd, Stereoscopic acuity for various levels of illumination, *Proc. Nat. Acad. Sci.*, 34, 1948, 223-227.

<sup>2</sup> R. N. Berry, L. A. Riggs, and Carl P. Duncan, The relation of vernier and depth discriminations to field brightness, *J. Exper. Psychol.*, 40, 1950, 349-354.

<sup>3</sup> Berry, Riggs, and Duncan, *op. cit.*, 354.



If one assumes that the results obtained by Mueller and Lloyd, and by Berry, Riggs, and Duncan are applicable to the present experiment, one might expect increased variability in *O*'s judgments as brightness is decreased. A second effect of decreased brightness stems from the postulate of Berry, Riggs, and Duncan to the effect that the retinal resolution of the single eye is the important factor in depth discrimination at low levels of brightness. From this postulate one might expect a shift in *O*'s judgments from a typical binocular performance (approximating size-constancy to distances of 30 to 80 ft. from *O*) to a performance typical of monocular vision (following visual angle beyond 10 ft.). The present research is primarily concerned with the verification of these expectations.

#### METHOD AND PROCEDURE

*Apparatus.* The apparatus used in this experiment was similar to that described in an earlier study.<sup>4</sup> *O* sat in a dark tunnel with his chin and forehead held firmly by a head-rest. Stimulus-panels could be presented directly in front of *O* at seven different distances. Each panel had an isosceles (length of base  $\frac{3}{8}$  of height) triangular opening through which *O* could see the illuminated surface of a shadow box 120 ft. away. These standard triangles were cut to different sizes in order that each one at its respective distance would subtend the same visual angle of  $1^{\circ} 11'$ . One of these standard triangles was lowered into viewing position at a time.

The comparison was a variable isosceles triangle (length of base also  $\frac{3}{8}$  of height). The brightness of this comparison was equated to that of the standard triangles with a Macbeth illuminometer. The base of the comparison triangle was  $2.5^{\circ}$  to the left of the standard triangles. For half the *Os* the distance of the comparison triangle was 60 ft. For the remaining *Os* the distance was 35 ft.

*Observers.* College students, unfamiliar with the apparatus or the purpose of the experiment, were used as *Os*. All were tested for acuity and depth discrimination at a distance of 10 ft. Only those with 20/20 vision were used in the experiment. Depth discrimination thresholds obtained at a distance of 10 ft. did not correlate significantly with the range of distances over which *O* demonstrated good size constancy in the experiment itself (rank order correlation =  $+0.20$ ,  $N = 12$ ). The preliminary test of depth discrimination was therefore not used as a criterion in the selection of *Os*.

*Procedure.* *O* was asked to tell *E*, who controlled the comparison triangle, when the comparison appeared to be the same physical size as the standard triangle. "For example," *O* was told, "if you place a measuring stick against one triangle and then against the other, both would read the same number of units."

Several viewing conditions were employed: (1) monocular judgments with the brightness of all stimulus-objects at 25 foot lamberts; (2) monocular judgments at 0.025 foot lamberts; (3) binocular judgments at 25 foot lamberts; (4) binocular judgments at 0.025 foot lamberts. The order of conditions for each *O* was determined from random number tables. Ten minutes of dark adaptation preceded

<sup>4</sup> E. L. Chalmers Jr., Monocular and binocular cues in the perception of size and distance, this JOURNAL, 65, 1952, 415-423.



the brightness of 0.025 foot lamberts. Five minutes of rest was allowed between all other conditions. Brightness levels below 25 foot lamberts were obtained by placing a neutral absorption filter between the stimulus-objects and  $O$ .

### RESULTS

(1) *Monocular vision.* The data of 8  $O$ s are shown in Table I. The entries represent the apparent physical size of the standard triangles as measured by the settings of the comparison triangle. If the visual angle of the standard triangles were the sole determinant of their perceived size and distance, the judgments would be the same magnitude for all standard tri-

TABLE I  
MONOCULAR VISION: HEIGHT (IN.) TO WHICH  $O$  SET THE COMPARISON  
TRIANGLE TO MATCH THREE STANDARD TRIANGLES

Each entry for  $O_3$  through  $O_8$  is the mean of five judgments; for  $O_1$ , the mean of two judgments; for  $O_2$ , the mean of three judgments.

Brightness (ft. lamberts)	Dis. (ft.)	Height (in.)	Comparison at 60 ft.				Comparison at 35 ft.			
			$O_1$	$O_2$	$O_3$	$O_4$	$O_5$	$O_6$	$O_7$	$O_8$
25	10	2.5	13.5	12.7	14.4	10.0	7.7	6.8	9.5	7.3
	15	3.75	14.0	14.0	14.8	11.2	8.0	8.1	9.7	7.6
	20	5.0	14.1	13.8	14.7	11.4	8.0	8.9	9.8	7.7
0.025	10	2.5	13.5	14.0	13.7	16.0	8.1	—	8.1	7.1
	15	3.75	14.3	14.5	14.0	15.6	8.8	—	9.1	7.6
	20	5.0	14.1	13.8	13.7	16.8	8.2	—	7.9	8.1

angles. Ideally the judgments would all be 15.0 in. with the comparison at 60 ft., and 8.75 in. with the comparison at 35 ft. At a brightness of 25 foot lamberts monocular judgments of the 15- and 20-ft. standards—and of all standards beyond 20 ft. (data not shown) are quite similar and thus appear to be determined solely by the visual angle of the standards. On the other hand, the mean apparent size of the 10-ft. standard is less for all  $O$ s than the mean size of all other standards, significantly so (5-% level of confidence) for  $O_2$  and  $O_6$ . A cue other than visual angle determines, in part, the apparent size-distance of the 10-ft. standard.

What is the effect of reduced brightness? Does the variability of  $O$ 's judgments increase? The answer is in the negative. No such trend is apparent in the data. The variability of the judgments (sum of the squared deviations from the mean) for more than half the means (11) is greater at a brightness of 25 foot lamberts than at 0.025 foot lamberts. Are there any other effects of reduced brightness? The answer is inconclusive. Reduced brightness results in slight and varied shifts of judgment. Of the 3 mean judgments from each of 7  $O$ s, 13 shift in the direction of a closer match to the visual angle of the standards, 4 shift away from visual angle, and



4 are unchanged. Four of these shifts are significant (5-% level of confidence). They are all of the judgments for  $O_4$  which shift in the direction of a closer visual angle match, and the judgments of  $O_3$  of the 15-ft. standard which shift away from visual angle. Thus it appears that under conditions of monocular regard any effects of reduced brightness are too small and too varied to permit any rigorous conclusions. The results of the following section on binocular vision will further help to explain what slight effects have been observed with monocular vision.

(2) *Binocular vision.* Table II contains the binocular data for the same eight  $O$ s. The entries represent the apparent physical size of seven standard

TABLE II  
BINOCULAR VISION: HEIGHT (IN.) TO WHICH  $O$  SET THE COMPARISON  
TRIANGLE TO MATCH SEVEN STANDARD TRIANGLES

Each entry for  $O_1$  is the mean of two judgments; for  $O_2$  and  $O_6$ , the mean of three judgments; for remaining  $O$ s, the mean of five judgments.

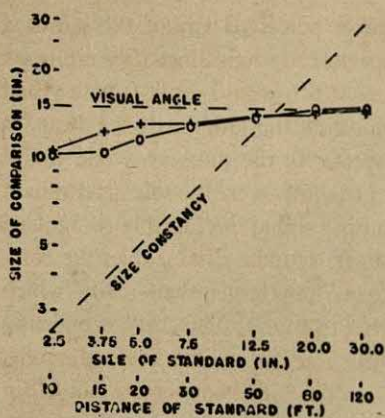
Brightness (ft. lamberts)	Dis. (ft.)	Height (in.)	Comparison at 6c ft.				Comparison at 35 ft.			
			$O_1$	$O_2$	$O_3$	$O_4$	$O_5$	$O_6$	$O_7$	$O_8$
25	10	2.5	10.5	9.9	4.4	4.8	2.5	3.0	3.6	3.1
	15	3.75	10.0	10.8	6.0	6.1	3.8	4.6	4.5	4.3
	20	5.0	12.6	11.1	6.7	7.8	4.0	5.9	5.3	5.1
	30	7.5	12.9	13.1	8.0	9.4	5.3	7.2	7.1	5.9
	50	12.5	13.4	14.4	14.8	11.5	7.3	6.1	12.4	11.3
	80	20.0	14.6	15.4	22.5	26.2	—	8.1	14.8	12.6
	120	30.0	14.4	16.1	26.1	26.9	7.6	7.9	12.3	12.8
0.025	10	2.5	11.0	10.3	9.2	8.3	4.3	6.4	3.1	4.3
	15	3.75	12.2	12.6	9.7	8.6	5.3	8.8	5.0	5.8
	20	5.0	14.2	12.5	10.7	10.4	5.3	9.6	6.0	6.3
	30	7.5	13.1	13.4	11.7	11.6	6.0	9.8	7.6	7.2
	50	12.5	14.2	14.6	14.0	14.7	7.2	8.9	9.7	10.3
	80	20.0	14.4	14.7	—	20.3	—	10.0	11.0	10.8
	120	30.0	15.3	14.8	14.0	20.0	8.0	9.8	10.7	11.7

triangles as measured by the settings of the comparison triangle. The data from this Table appear in Fig. 1. The data in the upper half of this Table were collected with the brightness of the stimulus-objects at 25 foot lamberts, and are indicated in Fig. 1 by small circles (O). The data in the lower half of this table were collected at a brightness of 0.025 foot lamberts, and are indicated in Fig. 1 by small plus signs (+).

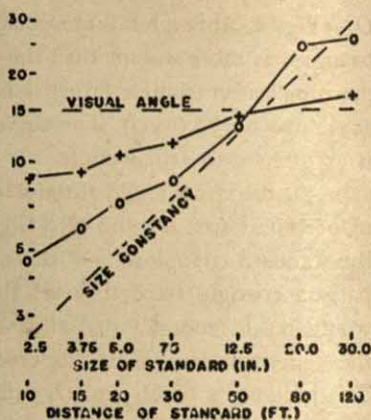
In an earlier article binocular size-distance perception was found to be mainly determined by the real physical size of stimulus-objects 10 to 50 ft. from  $O$ , and primarily determined by the visual angle of stimulus-objects 80 ft. and farther.<sup>5</sup> The distance at which the visual angle of the triangles becomes the principal determinant of their apparent physical size differs for each  $O$ . This is apparent in Fig. 1 of the present study.  $O_1$  and

<sup>5</sup> Chalmers, *op. cit.*, 420-421.

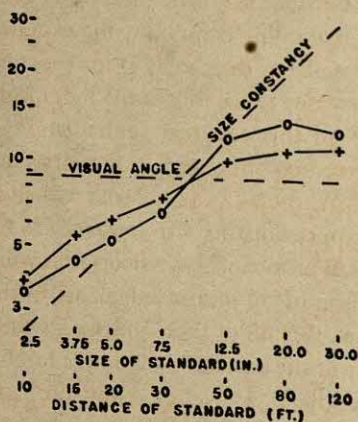




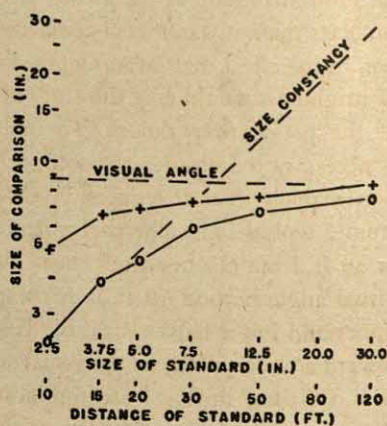
Data from  $O_1$  and  $O_2$   
(Dis. = 60 ft.)



Data from  $O_3$  and  $O_4$   
(Dis. = 60 ft.)



Data from  $O_5$  and  $O_6$   
(Dis. = 35 ft.)



Data from  $O_7$  and  $O_8$   
(Dis. = 35 ft.)

FIG. 1. APPARENT SIZE OF STANDARD TRIANGLES AS MEASURED  
BY COMPARISON TRIANGLES

° = brightness of triangles at 25 foot lamberts.  
+ = brightness of triangles at a 0.025 foot lamberts.



$O_2$  (Fig. 1, upper left) presumably never perceived any of the standard triangles as more distant than the comparison triangle since they rarely set the comparison triangle larger than a size which would result from a visual angle match (15.0 in.). The curve connecting the judgments for these  $O$ s is asymptotic to visual angle, *i.e.* asymptotic to the judgments one would expect if the visual angle of the standard triangles were the sole determinant of perceived size.  $O_3$  and  $O_4$  (Fig. 1, upper right) presumably recognized the standard triangles at 80 ft. and 120 ft. as more distant than the comparison triangle since they set the comparison larger than a size which would result from a visual angle match (15.0 in.). The curve connecting the judgments for these  $O$ s crosses the line representing visual angle. The judgments of  $O_5$  and  $O_6$  differ from the judgments of  $O_7$  and  $O_8$  in the same manner. To pool these data might be misleading to the reader, especially when one considers the effects of reduced brightness upon  $O$ 's judgments.

Before discussing Fig. 1 in greater detail it will be helpful to determine whether reduced brightness increases the variability of  $O$ 's judgments. As was the case with monocular vision the answer is in the negative. The variability of the judgments (sum of the squared deviations from the mean) for more than half of the means (31) is greater at a brightness of 25 foot lamberts than at 0.025 foot lamberts. This finding contradicts the expectation to the effect that larger depth discrimination thresholds at lower levels of brightness would take the form of increased variability in the judgments of the present experiment. To understand this apparent contradiction it is necessary to consider the second expectation suggested in the introduction; namely, that at reduced levels of brightness  $O$ 's judgments will shift from a typical binocular performance (approximating size constancy to 30 or 80 ft. from  $O$ ) to a performance typical of monocular vision (following visual angle beyond 10 ft.). A comparison of 54 means judgments in the upper and lower halves of Table II shows that 48 of these judgments shift toward a closer match to the visual angle of the standards at a lower brightness. Of these shifts 11 are significant at the 1-% level of confidence, and 9 more at the 5-% level. The effects of reduced brightness may be seen more readily in the graphs of Fig. 1. The shifts in the judgments of  $O_1$ ,  $O_2$ ,  $O_5$ , and  $O_6$  are in the direction of a larger setting of the comparison triangle. The shifts in the judgments of  $O_3$ ,  $O_4$ ,  $O_7$ , and  $O_8$  are in the direction of a larger setting with standards nearer than the comparison, and in the direction of a smaller setting with standards farther than the comparison.



These shifts or changes in *O*'s judgments as a result of reduced brightness might be better described as a regression to visual angle. Since the visual angle of stimulus-objects beyond 10 ft. is the sole determinant of primary monocular size-distance perception, the shifts in *O*'s judgments might also be described as a regression to monocular vision. Thus the postulate of Berry, Riggs, and Duncan is confirmed by the present data. It should be pointed out however that the regression of judgments that occurs as brightness is reduced is by no means complete at 0.025 foot lamberts. Even at this level of brightness the cues provided by binocular disparity and convergence play an important rôle in size-distance perception.

TABLE III

BINOCULAR VISION AT FOUR LEVELS OF BRIGHTNESS: HEIGHT (IN.) TO WHICH *O* SET THE COMPARISON TRIANGLE TO MATCH TWO STANDARD TRIANGLES

Each entry is a mean of five judgments; distance of the comparison triangle is 35 ft.

Dis. (ft.)	Height (in.)	Brightness (ft. lam- berts)	<i>O</i> <sub>3</sub>	<i>O</i> <sub>7</sub>	<i>O</i> <sub>8</sub>	<i>O</i> <sub>9</sub>	<i>O</i> <sub>10</sub>	Mean
10	2.5	25.0	4.6	3.6	3.1	3.9	3.3	3.7
		2.5	4.7	3.4	3.8	4.3	3.4	3.9
		0.25	4.6	3.7	4.2	4.3	3.7	4.1
		0.025	5.8	3.1	4.3	2.7	5.1	4.2
120	30	25.0	14.2	12.3	12.8	14.6	12.3	13.2
		2.5	14.3	13.8	12.2	12.5	12.0	13.0
		0.25	13.7	11.7	11.8	13.8	11.4	12.5
		0.025	12.6	10.7	11.7	14.1	9.5	11.7

It is now easy to understand why the expectation of increased variability of the judgments at reduced brightness was not confirmed. Reduced brightness appears to result in a change in the proportion of the available cues utilized in making a judgment. The variability of judgments at 0.025 foot lamberts compared with the variability at 25 foot lamberts involves a comparison of the variability of monocular acuity judgments with binocular size-distance judgments. The expectation of increased variability was based upon data from the threshold of acuity and the discrimination of depth without recognition of the shift in the use of cues. The expectation was therefore unwarranted.

All of the preceding data have been obtained at only two levels of brightness. Additional levels are necessary to obtain information about the course of the regression to visual angle. For this reason two additional *O*s and three former *O*s made judgments of the 10-ft. and 120-ft. standards at brightnesses of 0.025, 0.25, 2.5, and 25 foot lamberts. These data appear



in Table III. For  $O_8$  the initial reduction of brightness (from 25 to 2.5 foot lamberts) results in the greatest amount of regression while for  $O_3$  and  $O_{10}$  the regression is small until the lower levels of brightness are reached.  $O_7$  and  $O_9$  give more ambiguous judgments. The means obtained with the 10-ft. standard show equal amounts of regression when brightness is reduced by logarithmic steps from 25 to 0.025 foot lamberts. The means obtained with the 120-ft. standard show increased amounts of regression when brightness is reduced by logarithmic steps. Without additional data especially at brightness levels below 0.025 foot lamberts one can only conclude that the course of the regression to visual angle differs greatly from one  $O$  to the next.

Rank-order correlations between the amount of regression in judgments of the 10-ft. and the 120-ft. standards on the one hand and three criteria of depth perception at the highest brightness on the other are all positive (three of six correlations were significant at the 5-% level of confidence). Restated, these correlations are not surprising. They indicate that those  $O$ s whose initial judgments indicate better than average size constancy alter their judgments more as brightness is lowered than do  $O$ s with little depth perception, *i.e.*  $O$ s whose judgments at 25 foot lamberts are primarily determined by visual angle.

#### SUMMARY

Information about the rôle of brightness in primary size-distance perception may be found in experiments upon thresholds of visual acuity and of depth discrimination at reduced levels of brightness. Expectations from these experiments are (1) increased variability in the judgments as brightness is reduced and (2) a shift in the judgments from a typical binocular performance (judgments follow the real physical size of objects to distances of 30 to 80 ft. from  $O$ ) to a performance typical of monocular vision (judgments determined by the visual angle of stimulus-objects beyond 10 ft.).

Data have been presented from monocular and binocular size-distance judgments made at brightness-levels of 25 and 0.025 foot lamberts. An attempt was made to eliminate the use of any secondary cues in these judgments.

Data obtained under conditions of monocular regard are similar to, thought not as conclusive as the data obtained under binocular regard. Under conditions of binocular regard there is no evidence for the expectation that reduced brightness will cause increased variability in the judgments.



ments. This finding is more clearly understood after a consideration of the second expectation.

Under binocular regard the reduction of brightness results in what may be described as a regression to visual angle. That is to say, as brightness is reduced the visual angle of the stimulus-objects assumes greater importance relative to the visual cues provided by retinal disparity and binocular convergence. Since visual angle is the only cue for the monocular, size-distance perception of objects beyond 10 ft., the effect of reduced brightness might also be described as a regression to monocular vision. Thus the second expectation is confirmed. Additional data have shown that the regression to visual angle is most marked at different levels of brightness for different *O*s. More data and lower levels of brightness are needed for a better understanding of the course of this regression to visual angle.



## THE ORIENTING TASK IN INCIDENTAL AND INTENTIONAL LEARNING

By IRVING J. SALTZMAN, Indiana University

The early experiments on incidental learning were designed to determine whether that form of learning did occur and, if so, to what extent. Their results indicate quite conclusively that it does occur and, furthermore, that it is much less efficient than intentional learning.<sup>1</sup>

The usual procedure in studies comparing incidental and intentional learning is to give the Ss in the 'incidental-learning' group a task that does not require that the material presented be learned though it does require that it be observed. The Ss in the 'intentional-learning' group, on the other hand, are merely instructed to learn the material presented—they are not required to perform the orienting task of the first group. Giving or not giving explicit instructions to learn may not, therefore, be the only factor contributing to the difference between the results of the two groups. It is possible that the orienting task required of the incidental learners, and not of the intentional learners, may be a contributing factor. To date, however, the influence of this 'orienting' factor has been ignored. The difference between the scores of incidental and intentional learning has been attributed exclusively to the presence or absence of instructions to learn. The present study attempts to determine the rôle of the orienting task in the results of these studies.

### METHOD AND PROCEDURE

*Subjects.* Forty undergraduate students (men and women) served as Ss. They were randomly divided into two equal groups: Group I, an incidental-learning group, and Group II, an intentional-learning group. Every S was seated comfortably at a table and given a deck of 32 cards, each  $3 \times 5$  in., and the following instructions.

*Instructions.* Each of these 32 cards has a different number typed on it. Eight of the numbers are below 50 and even; eight are above 50 and even; eight are below 50 and odd; and eight are above 50 and odd. I wish you to sort the cards into these four categories as rapidly as you can. You will have several opportunities to sort the cards and I shall time you on each trial. I am interested in finding out how your speed changes with practice. Arrange the four piles of cards as shown on this card.

A card showing the arrangement of the four piles of cards was placed on the table in front of S and was left there for reference during the card-sorting. The cards with

\* Accepted for publication August 26, 1952.

<sup>1</sup> For a recent treatment of the topic of incidental learning see J. A. McGeoch and A. L. Irion, *The Psychology of Human Learning*, 1952, 210-215.



the even numbers above 50 were to be placed on the upper right and those with the even numbers below 50, on the lower right. The cards with the odd numbers above 50 were to be placed on the upper left and those with the odd numbers below 50, on the lower left. When *S* indicated that the instructions were understood,\* the first trial was begun. Three timed trials were given every *S* with a 10-sec. interval between successive trials. A duplicate set of cards was used on alternate trials. While *S* sorted one set of cards, *E* shuffled the duplicate set. The accuracy of each sorting was checked immediately after the trial was completed. If a card was misplaced, *S* was cautioned about future errors. The trials were timed by means of a stop watch.

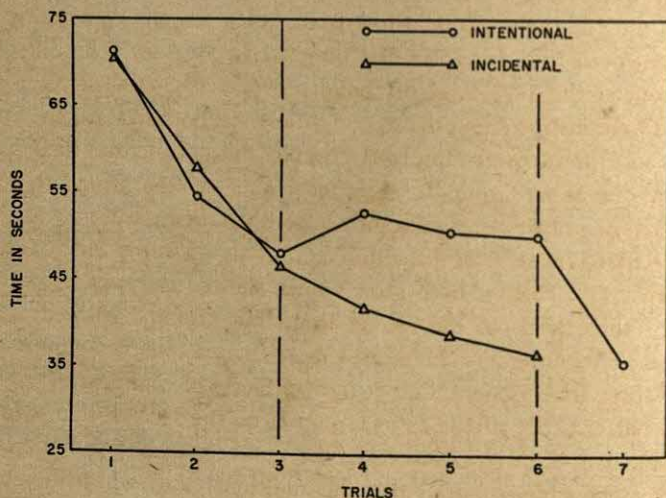


FIG. 1. MEAN SORTING TIME REQUIRED BY THE TWO GROUPS OF *SS* AT THE VARIOUS TRIALS

Following the third trial, the *SS* of Group I were given a 75-sec. rest-period, which was followed by three more sorting trials. The *SS* of Group II, following the third trial, were given an unexpected recognition-test for the numbers on the cards. They were given a sheet of paper on which were mimeographed 72 two-digit numbers, arranged in eight columns of nine. The 72 numbers were arranged in sequence and included the 32 that were on the cards plus 40 which were not on the cards. The *SS* were asked to circle as many of the numbers as they recognized as having been on the cards, 75 sec. being allowed them. Following the recognition-test the *SS* were given three more sorting trials in which they were asked to continue to sort the cards as rapidly as possible and at the same time to try to learn the numbers, since there would be another test.

Following the sixth trial, both groups of *SS* were given the recognition-test. The *SS* of Group II had been informed that the test would be given; the *SS* in Group I had not been so informed. The *SS* in Group I were dismissed following the test; the *SS* in Group II were dismissed after one additional sorting trial was given with the assurance that there would be no further tests.



*Results.* The mean sorting times on each trial for both groups are shown in Fig. 1. On the first three trials both groups were performing under identical conditions, *i.e.* neither group had been informed that a test was to be administered. The mean times for the first three trials combined are 59.4 sec. for Group I and 57.9 sec. for Group II. The difference between these means is not statistically significant ( $t = 0.25$ ).

On the second three trials, the performances of the two groups differed noticeably. The Ss in Group I, who merely received a 75-sec. rest period between trials three and four, continued to show an increase in speed. The mean time for their second three trials, 39.3 sec., is significantly lower than the mean time for their first three trials, 59.4 sec. The difference is significant at the 1-% level of confidence ( $t = 3.59$ ). The Ss in Group II, who were instructed on the second three trials both to learn the numbers and to sort the cards as rapidly as possible, did not increase their speed as much as the Ss in Group I. Their mean time on the second three trials (51.3 sec.) is, however, significantly lower than their mean time on the first three trials (57.9 sec.) and the difference is significant at the 5-% level of confidence ( $t = 2.13$ ). Both groups then, showed increases in speed from the first three trials to the second three, but Group I showed a greater increase. The difference in mean time between the two groups on the second three trials is significant at the 1-% level of confidence ( $t = 2.86$ ).

The performance of the Ss in Group II on the seventh trial, when they were asked to stop trying to learn the numbers and to sort the cards as rapidly as possible, suggests that the additional time which they took on the second three trials was spent in trying to learn the numbers. The mean time for these Ss on the seventh trial is 35.2 sec., which is not significantly different from the mean time of the Ss in Group I on trial six, 36.9 sec. ( $t = 0.63$ ). It is, however, greatly different from the mean time on their own sixth trial (50.3 sec.) and the difference is significant at the 1-% level of confidence ( $t = 6.67$ ).

Scores on the recognition-test were derived by subtracting from the number of correctly recognized numbers, the number that might be expected by chance. For example, if S circled 18 numbers, 15 of which were correctly recognized, his score would be 15 minus 8, or 7. (Four of every nine numbers circled were considered to be correct by chance.) The scores obtained are as follows: the mean score for Group II following the sixth sorting trial is 4.60; for Group I it is 4.49. The difference between these means is not statistically significant ( $t = 0.15$ ). The amount learned by the two groups is substantially the same. The mean score for Group II after three trials (without instructions to learn) is 3.52. This score is not sig-



nificantly different from the mean score of Group I after six trials without instructions to learn ( $t = 1.23$ ).

In the previous studies comparing intentional and incidental learning, the incidental learners were not given instructions to learn, but were required to perform an orienting task. The intentional learners, on the other hand, were given instructions to learn, but were not required to perform the orienting task. Therefore, the difference in learning scores obtained with these two conditions could be attributed to either one, or both, of these two factors: giving instructions to learn and requiring the performance of an orienting task. Invariably, however, the interpretation of the higher scores of the intentional learners has been in terms of the influence of the instructions to learn. The influence of performing the task has been consistently ignored. The present study was designed to investigate the rôle of the orienting task. The results of this study indicate that when both groups were required to perform the orienting task, the intentional learning scores were not higher than the incidental learning scores. It is concluded, therefore, that the higher scores of the intentional learners in the previous studies are at least in part due to not requiring the intentional learners to perform the orienting task.

Since the scores of the intentional learners in the present study were not higher than those of the incidental learners, the importance that has been attributed to instructions to learn might be questioned. It might be concluded from the results of the present study that when the only difference between the two learning conditions is the presence or absence of instructions to learn, we will fail to find a difference in the learning scores. It is felt, however, that a conclusion of such generality should not be drawn from these data. Another interpretation is available. In the previous studies comparing incidental and intentional learning, the *Es* controlled the rates at which the learning materials were presented. The rates apparently were slow enough for the intentional learners, who were not required to perform the orienting task, to carry out their instructions to learn. This was probably not the case in the present study where each *S* set his own rate by the speed with which he sorted the cards. It is suggested that although the intentional learners in the present study took more time than the incidental learners, they did not take enough additional time. The instructions to sort the cards as rapidly as possible probably caused them to rush. Accordingly, their scores are not higher than the scores of the incidental learners. In other words, it is suggested that an important parameter in studies comparing incidental and intentional learning is the rate of presentation of the learning material. It is felt that the magnitude of the difference between the intentional and



incidental learning scores depends upon the rate of presentation of the learning material, being greater with slow rates than with fast rates. If this is so, it is possible that the rates adopted by the Ss in the present study were much faster than the rates used in the other studies in this area. Consequently no difference in the learning scores between the two learning conditions was found. This line of reasoning can be checked experimentally by noting whether the difference between incidental and intentional learning scores changes with changes in the rate of presentation of the learning material.<sup>2</sup>

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<sup>2</sup> Such an experiment has been conducted by Miss Edith Neimark and the writer and the results were presented at the 1952 meetings of the Midwestern Psychological Association. A greater difference between the intentional and incidental learning scores was found with a slow rate than with a fast rate of presentation.



## THE PHENOMENAL VERTICAL AND HORIZONTAL IN BLIND AND SIGHTED SUBJECTS

By M. E. BITTERMAN and PHILIP WÖRCHEL, University of Texas

The relative importance of visual and postural determinants of the major axes of phenomenal space has been debated extensively. Gibson and Mowrer, while admitting the influence of visual factors, have maintained that postural cues are both genetically prior and decisive in the event of conflict between the two kinds of sensory data.<sup>1</sup> Asch and Witkin,<sup>2</sup> following Koffka,<sup>3</sup> have insisted that the phenomenal vertical is principally determined by the direction of lines in the visual field. Two classes of evidence have been examined in relation to these contradictory positions: (1) on the ability of *S* to discover the gravitational vertical in the absence of a visual framework; and (2) on the effect of experimentally produced conflict between visual and postural cues. Unfortunately, no unambiguous solution of the problem has emerged. Most recently, Gibson has withdrawn from the debate, assuming the coördinate development and functioning of visual and postural mechanisms.<sup>4</sup>

One may doubt, nevertheless, that the problem is as 'fruitless' as Gibson maintains or existing data as 'contradictory' as he suggests. The key to a more satisfactory resolution of the problem is to separate the issues of genetic priority and dominance which have been hopelessly confounded in previous treatments. When this separation of issues has been achieved, it becomes possible to defend the position that *postural cues are genetically prior* and that *visual cues eventually become dominant* in the orientation of normal people. The problem of orientation which confronts the organism is first understood in postural terms and later comes to be solved in terms which are dominantly visual. This modal shift is prompted by the highly articulated structure of everyday visual environments and the great precision

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<sup>1</sup> J. J. Gibson and O. H. Mowrer, Determinants of the perceived vertical and horizontal, *Psychol. Rev.*, 45, 1938, 300-323.

<sup>2</sup> S. E. Asch and H. A. Witkin, Studies in space orientation: I. Perception of the upright with displaced visual fields, *J. Exper. Psychol.*, 38, 1948, 325-337; II. Perception of the upright with displaced visual fields and with body tilted, *ibid.*, 445-477.

<sup>3</sup> Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 213-216.

<sup>4</sup> Gibson, The relation between visual and postural determinants of the phenomenal vertical, *Psychol. Rev.*, 59, 1952, 370-375.



of visual discrimination; it is justified by the close correspondence between visual and gravitational directions in the normal life of the individual.<sup>5</sup>

It has been demonstrated in a number of experiments that the upright posture provides a remarkably stable point of reference in the absence of a visual framework. A tilted *S* can return his body to the gravitational upright with great precision,<sup>6</sup> and a centrifuged *S* discriminates a vertical which is closely related to the resultant of centrifugal and gravitational forces.<sup>7</sup> Witkin and Asch have demonstrated that although an upright *S* is able to adjust a luminous rod to the gravitational vertical with considerable success, marked disorientation (inconsistency and inaccuracy) appears when *S* is required to make the same judgment with head or body tilted. These investigators maintain that the great disturbance which results from so minor a change points to "the rather limited usefulness of postural determinants."<sup>8</sup> It should be obvious, however, that (whatever the accuracy of gravitational orientation in the absence of a visual framework) vision alone prior to somesthetic experience, provides no basis at all for estimating the gravitational vertical; it is in this sense that postural cues may be assigned a genetic priority. Nevertheless, the deficiencies in postural orientation highlighted by the experiments of Witkin and Asch may be taken as evidence that the extremely successful orientation of sighted individuals stems from the dominance of visual factors.

The same investigators also provide impressive evidence for the prepotency of visual determinants under conditions of conflict.<sup>9</sup> Contradictory conclusions have appeared in the literature, but the evidence against visual dominance is not convincing. Of the three studies cited in Gibson's review as supporting the conclusion that judgments of the vertical are "little influenced by the lines in the visual field,"<sup>10</sup> one involved no visual framework (only the luminous rod to be adjusted by *S* as seen),<sup>11</sup> and a second involved a 'not very striking' visual 'cue' consisting only of "a pair of parallel lines 8 in. long and 1 in. apart at a very low level of visual intensity."<sup>12</sup> In the third study, by Passey,<sup>13</sup> a quite well structured visual framework was found to have little effect on the ability of tilted *Ss* to return themselves to the gravitational upright; but the procedure employed was such as to provide a strong set for postural

<sup>5</sup> An interesting analogy may be found in the logic of test-validation. An objective test, validated (*e.g.* given meaning) in terms of teachers' ratings, is used instead of the ratings because of its greater precision and convenience.

<sup>6</sup> C. W. Mann, N. H. Berthelot-Berry, and H. J. Dauterive, Jr., The perception of the vertical: I. Visual and non-labyrinthine cues, *J. Exper. Psychol.*, 39, 1949, 538-547.

<sup>7</sup> C. E. Noble, The perception of the vertical: III. The visual vertical as a function of centrifugal and gravitational forces, *J. Exper. Psychol.*, 39, 1949, 839-850.

<sup>8</sup> Witkin and Asch, Studies in space orientation: III. Perception of the upright in the absence of a visual field, *J. Exper. Psychol.*, 38, 1948, 611.

<sup>9</sup> Asch and Witkin, *op. cit.*, 325-337, 445-477; Witkin and Asch, Studies in space perception: IV. Further experiments on perception of the upright with displaced visual fields, *J. Exper. Psychol.*, 38, 1948, 762-782.

<sup>10</sup> Gibson, *op. cit.*, 371.

<sup>11</sup> Noble, *op. cit.*, 839-850.

<sup>12</sup> Mann, Berthelot-Berry, and Dauterive, *op. cit.*, 544-545.

<sup>13</sup> G. E. Passey, The perception of the vertical: IV. Adjustment to the vertical with normal and tilted visual frames of reference, *J. Exper. Psychol.*, 40, 1950, 738-751.



cues and the Ss reported deliberate attempts to ignore the visual framework. It is best in an experiment of this kind to have *S* vary neither his own tilt nor that of the framework, but to use a visible rod which moves independently of both. The use of a visible rod make it impossible for *S* to avoid visual stimulation (in the same way that it is impossible for him to avoid somesthetic stimulation). In any event, the factor of set is difficult to control when the method of conflict is employed, and other approaches to the problem of dominance should be sought.

In the experiment here reported, the ability of congenitally blind Ss and sighted Ss deprived of vision to discriminate the gravitational vertical and horizontal was compared. The logic of the experiment is straightforward: If the orientation of sighted Ss is normally dominated by postural cues, depriving them of vision should not place them at a disadvantage, and the two groups should perform at the same level of accuracy. If, however, the orientation of sighted Ss is normally dominated by vision, their postural discrimination should not be highly developed and their performance in the experimental situation should be inferior to that of the blind.

*Subjects.* The Ss of the experiment were 22 totally blind students (blind since birth) at the Texas State School for the Blind and a like number of sighted students. The ages of the Ss ranged from 9 to 24 yr., with a mean of 16.1. There were 13 boys and 9 girls in each group, blind and sighted, who were paired for age and sex.

*Apparatus.* The aluminum rod used for indicating the vertical (*V*) and horizontal (*H*) was 36 in. long and 0.5 in. wide. It was pivoted at its center on a shaft 8 in. in length at the other end of which was a dial calibrated in degrees. The dial had two zero-points, one representing *V* and the other *H*. Since the dial turned with the rod against a stationary pointer, the displacement of the rod from *V* or *H* could be read directly in degrees. Clockwise deviations (relative to *S*) from *V* or *H* were designated as *positive* and counterclockwise deviations as *negative*. The height of the shaft on which the rod was pivoted could be varied from 36-66 in.

For determinations with *S* tilted, an apparatus was constructed consisting of two 1 × 12 in. boards (78 in. long) fastened together at the top to form an inverted 'V' with each arm inclined 42° from the vertical. The arms rested in a heavy base which kept them securely in position. Attached perpendicularly to each of the inclined planes was a wooden footrest.

*Procedure.* Each *S* was securely blindfolded in a small anteroom before being led into the experimental room which contained the apparatus. In this respect, as in all others, blind and sighted Ss were treated in identical fashion. Under *Condition I*, *S* stood erect and made 32 settings of the rod, 16 each for *V* and *H*. Half the determinations were made with the right hand, the pivot of the rod being aligned with *S*'s right shoulder; half the determinations were made with the left hand, the pivot being aligned with the left shoulder. The meanings of *V* and *H* were explained and the *V* and *H* positions of the rod were illustrated. *V* was defined as 'straight up and down' and *H* as 'parallel to the floor—both ends of the rods are the same distance from the floor.' The initial position of the rod on each trial was systematically counterbalanced. Under *Condition II*, the first series of determinations was repeated with



*S* tilted  $42^\circ$  from *V*. There were 16 determinations of *V* and 16 of *H*. For half the determinations of each axis *S* was tilted to the left and used the right hand; on the remaining trials *S* was tilted to the right and used the left hand. The pivot of the rod was always aligned with the shoulder of the hand used for the setting on any given trial.

*Results.* The accuracy of each *S* was expressed as an average error—the 16 determinations of each axis of space under each condition were averaged algebraically. The accuracy of each group was expressed as an average deviation from the true *V* or *H*—the mean of the individual error-scores

TABLE I  
AVERAGE DEVIATIONS OF BLIND AND SIGHTED *SS* FOR SETTINGS OF *V*  
AND *H* UNDER EACH OF THE TWO CONDITIONS

Condition	<i>V</i>			<i>H</i>		
	Blind	Sighted	Diff.	Blind	Sighted	Diff.
I	0.8	1.7	0.9	0.9	0.9	0.0
II	1.8	1.9	0.1	1.3	1.9	0.6
Diff.	1.0	0.2		0.4	1.0	

(taken without regard to sign for the purpose of reflecting magnitude of error as distinct from direction). From the average deviations presented in Table I it may be seen that the performance of the two groups was very much the same. Accuracy was somewhat higher under Condition I than under Condition II and higher for judgments of *V* than for judgments of *H*, but none of the differences (tested by Wilcoxon's nonparametric method for paired replicates)<sup>14</sup> remotely approached statistical significance.

Further inspection of the data revealed, however, that the variability of individual judgments increased in a systematic manner from Condition I to Condition II; in Condition II there was a marked tendency for errors to be positive (clockwise) when *S* was tilted to the left and negative (counterclockwise) when *S* was tilted to the right. This feature of the performance of each *S* was expressed in an index of lateral discrepancy—the mean of the settings when *S* was tilted to the left *minus* the mean of the settings when *S* was tilted to the right—for *V* and for *H*. Since *S* made settings with the right hand when tilted left, and with the left hand when tilted right, the index of discrepancy involved a factor of dextrality as well as a factor of orientation. To permit independent evaluation of the influence of dextrality, lateral discrepancies were computed also for Condition I—the mean of the

<sup>14</sup> Frank Wilcoxon, *Some Rapid Approximate Statistical Procedures*, American Cyanamid Co., Stamford, Conn., 1949, 1-16.



settings made with the right hand *minus* the mean of the settings made with the left.

Table II shows the mean lateral discrepancies for *V* and *H* under the two conditions. The values for Condition I are small and statistically insignificant, but the corresponding values for Condition II are larger and each is significant beyond the 1-% level (Wilcoxon's method for paired replicates). As may be seen from Table II, each discrepancy for Condition II is significantly larger than the corresponding discrepancy for Condition I (Wilcoxon's method for paired replicates). These results suggest that the discrepancy-score is an index of disorientation rather than dextrality.

It may also be seen from Table II that the lateral discrepancies under

TABLE II  
LATERAL DISCREPANCIES OF BLIND AND SIGHTED Ss FOR SETTINGS OF  
*V* AND *H* UNDER EACH OF THE TWO CONDITIONS

Condition	<i>V</i>			<i>H</i>		
	Blind	Sighted	Diff.	Blind	Sighted	Diff.
I	0.1	0.5	0.4	1.5	0.8	-0.7
II	3.2	5.5	2.3*	8.1	11.7	3.6†
Diff.	2.1*	5.0†		6.6†	9.1†	

\* Significant beyond the 5% level by Wilcoxon's test for paired replicates.

† Significant beyond the 1% level by Wilcoxon's test for paired replicates.

Condition II were significantly greater for the sighted Ss than for the blind, both in the settings of *V* and the settings of *H* (Wilcoxon's method for paired replicates). From these results it may be concluded that the blind are better oriented to the principal axes of space than are sighted Ss deprived of visual stimulation. If the superiority of the blind is attributed to years of practice in getting about in the world without the help of vision, it is reasonable to attribute the inferior performance of the sighted Ss to the fact that under ordinary circumstances their orientation has been dominated by vision. The present results are in accord with those obtained by Asch and Witkin in the study of orientation in conflict situations.

*Summary.* The experiment here reported bears on the controversy over the relative importance of visual and postural cues in the spatial perception of normal individuals. Orientation to the vertical and horizontal in blind Ss and in sighted Ss deprived of vision was studied. In an upright position both groups performed at the same high level; with body tilted the performance of both groups deteriorated, with significantly greater disorientation being manifested by the sighted Ss than by the blind. The latter result is interpreted as further evidence for the dominance of vision in the orientation of normal individuals.



## STUDIES IN VICARIOUSNESS: MOTOR ACTIVITY AND PERCEIVED MOVEMENT

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The study presented here is concerned with the construct of vicariousness as developed within the framework of the sensory-tonic field-theory of perception.<sup>1</sup> This theory is organismic. It attempts to overcome the limitations of a purely sensory interpretation of perception and its essential tenet is that organismic states are part and parcel of perception. Numerous physiologists (Sherrington, Magnus, Stein, Metzger, and others) have shown that sensory stimulation affects muscular tonus.<sup>2</sup> Any stimulation, whether it comes through extero-, proprio-, or intero-ceptors, is therefore sensory-tonic in nature.

The conception that any stimulation is sensory-tonic aims to eliminate the difficulties in understanding interaction between processes (sensory, motor, and visceral) which traditional psychology has viewed as separate entities. One of the logical consequences of this conceptualization is that there is 'functional equivalence' between sensory and muscular factors with respect to a perceptual end-product. It has been demonstrated, for instance, that visual perception of the vertical is affected equivalently by auditory stimulation and by direct muscular changes.<sup>3</sup> Such functional equivalence between sensory and muscular factors implies that one factor can substitute, or act vicariously, for the other factor.<sup>4</sup>

The concept of vicariousness has been extended to include that of vicarious channelization. This means that available energy may be released through different channels.<sup>5</sup> It is postulated that sensory-tonic processes may come to expression in

\* Accepted for publication September 30, 1952. This investigation was supported by a grant from the National Institute of Mental Health of the National Institutes of Health, Public Health Service.

<sup>1</sup> Heinz Werner and Seymour Wapner, Sensory-tonic field theory of perception, *J. Personal.*, 18, 1949, 88-107; Toward a general theory of perception, *Psychol. Rev.*, 59, 1952, 324-338.

<sup>2</sup> For an historical review see Werner and Wapner, *op. cit.*, *J. Personal.*, 94 f.

<sup>3</sup> Wapner, Werner and K. A. Chandler, Experiments on sensory-tonic field theory of perception: I. Effect of extraneous stimulation on the visual perception of verticality, *J. Exper. Psychol.*, 42, 1951, 341-345.

<sup>4</sup> This aspect of vicariousness with reference to an end-product has recently been stressed in a monograph by Brunswik and in Bertalanffy's discussion of equifinality, See Egon Brunswik, The conceptual framework of psychology, *Encyclopedia of Unified Science*, Vol. 1, 1952, No. 10; and L. von Bertalanffy, Theoretical models in biology and psychology, *J. Personal.*, 20, 1951, 24-38.

<sup>5</sup> We are aware of the shortcomings in the psychologist's use of the concept of energy, which commonly implies quantifiability. At the present state of our experimentation we introduce the concept to indicate that psychophysical activity has a source; we do not, however, explicitly state the nature of this source. Physiologizing psychologists might understand its nature in terms of central excitatory states; those who would confine themselves to psychological analysis might think of it in terms of tension or cathexis. For us, it has mainly the heuristic value of defining object-body relationships.



terms of muscular-tonic activity, viscerotonic activity, and perceptual activity. One should, for instance, expect that, if one mode of response is blocked, activity may be channelized into another mode.

An area which lends itself well to the study of the problem of vicarious channelization is that of motion. Evidence is accumulating which points to a vicarious relationship obtaining between bodily and perceptual motion.<sup>6</sup> An early experiment of Mach's illustrates the point. He had an S turn his eyes to the extreme left, where they were fixed by means of putty. When S attempted to look toward the right he saw objects displaced toward the right. This observation points to an antagonistic relationship between perceptual motion and bodily movement. It is confirmed by the following: if the eyes are moved over a point in space, the point appears stationary; if, however, the movement is obstructed, the innervation initiated for changing fixation finds no outlet in eye movement and is channelized into perceiving motion.

The generality of this problem becomes evident from the fact that systematic attempts to understand human behavior have utilized notions akin to that of vicarious channelization. Rorschach's interpretation of movement-responses to ink blots is related to the present problem. He assumes, as the following quotation reveals, an antagonistic relationship between the perception of motion and S's general motility.

The (degree of) motility observed in the subject is not a measure of the kinesthetic influences playing on him while interpreting the figures. On the contrary, the individual who is influenced by kinesthetic factors in the test is stable in his general motility, the mobile person is influenced little by a sense of movement in the figures. Such empirical results of the experiment can be repeated at any time though they lack theoretical foundation. The more kinesthesias, the less physical activity; the more physical activity, the less kinesthesias. Kinesthetic engrams therefore act as inhibitors of physical activity; motor activity inhibits kinesthetic engrams.<sup>7</sup>

Rorschach, in offering these observations, neither attempts to prove them experimentally nor to explain them theoretically.

The most comprehensive use of the notion of transformation of energy has been made by psychoanalysis. Transformation of energy appears to be the basis for the dynamics of cathexis which Rapaport recently discussed in terms of tension-discharge models.<sup>8</sup>

One of the aims of this study is to lift the notion of channelization or transformation of energy out of its conjectural status and to demonstrate its amenability to experimental test.<sup>9</sup>

<sup>6</sup> For a review of this evidence, see Werner, Motion and motion perception: A study in vicarious functioning, *J. Psychol.*, 19, 1945, 317-327.

<sup>7</sup> Herman Rorschach, *Psychodiagnostics*, 1942, 25.

<sup>8</sup> David Rapaport, The conceptual model of psychoanalysis, *J. Personal.*, 20, 1951, 56-81. One of the few neuropsychiatrists who have been keenly aware of this problem and its implications is Schilder. He finds that "transfer of tonic energy" from one muscle to another muscle is clearly demonstrable in cerebellar patients. For instance, by moving the legs of a patient Schilder induced movement in the arms; if these arm movements were held in check by fixation of the shoulder, the movements appeared in the head region instead. "Such positive reactions" Schilder remarks, "go on according to laws not so different from the laws psychoanalysis has discovered." See Paul Schilder, *Brain and Personality*, 1951, 18.

<sup>9</sup> One method of testing the channelization hypothesis was recently reported by Korchin, Meltzoff, and Singer. These investigators inhibited motor activity preceding the presentation of a Rorschach card. Under these conditions, the number of move-



## THE EXPERIMENT

As stated above the hypothesis underlying the present experiment is that of the vicarious relationship obtaining between perception of motion and muscular involvement. If *S* is confronted with drawings representing objects which suggest motion, it is hypothesized that there will be an inverse relation between the frequency of movement responses and the degree of motor involvement.

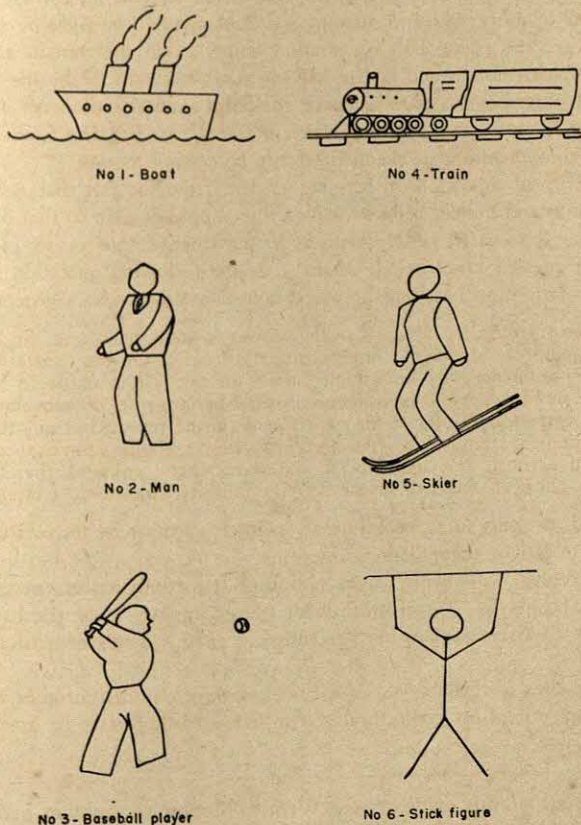


FIG. 1. DRAWINGS EXPOSED TO *S* FOR 20 M. SEC.

*Procedure.* Six drawings (see Fig. 1) were used. The pictures, simple black-on-white line drawings, are 18 in. square. They were flashed one at a time, on a screen in a darkened room, for 20 m.sec.

The pictures were presented to a control group (17 *Ss*), and an experimental

ment responses were more numerous than under conditions where motor activity was not inhibited. See S. J. Korchin, Julian Meltzoff, and J. L. Singer, Motor inhibition and Rorschach movement responses, *Amer. Psychol.*, 6, 1951, 344. A pilot study involving the methodology used in the experiment to be reported was carried out by Eugene Gollin at Clark University.



group (12 Ss). The Ss in the experimental group stood erect at a distance of 6 ft. from the screen, looking at a fixation-point in the center of the area in which the line drawings were to be exposed. The exposure of each drawing was preceded by a 20-sec. period of pushing against a push-board. The push-board apparatus (Fig. 2) consisted of a spring-opposed movable, horizontal board *a*, at chest level, and a stationary foot-platform *b*; both *a* and *b* were mounted on a wooden frame *c*. The horizontal board and the foot-platform were so arranged that the S was tilted at about a 60° angle and had to exert arm pressure counteracted by foot pressure. After a 20-sec. interval of pushing, S assumed an erect position and the picture was flashed

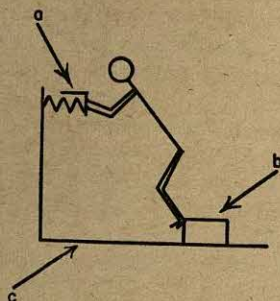


FIG. 2. DIAGRAMMATIC SKETCH OF THE PUSH-BOARD APPARATUS

on the screen. The Ss of the control group, like those of the experimental group, stood erect while viewing the pictures; however, their observations were not preceded by pushing.

This method was adopted after many failures. The difficulty to be overcome was that of bringing the S into a condition of general motor involvement without distracting him during the course of his observations. It was finally decided to introduce motor involvement in terms of an after-effect.<sup>10</sup> In this way motor involvement is induced without concomitant distraction from a set toward motor activity. Thus while the experimental and control group have the same stance, the degree of motor involvement is greater in the former than in the latter.

The Ss were requested to report fully what they perceived when the picture was flashed. No hint was given by E as to the aim of the experiment.

*Evaluating the responses.* Analogous to the Rorschach technique, the verbal responses of S were used as indicators of the presence or absence of the perception of motion. The responses were grouped into three classes defined as follows.

(1) *Active motion (MA).* A response was judged as indicating active motion (MA) if S stated that the whole figure or a part of the figure was seen as moving. These statements always contain either a verb or a participle of action. For example:

<sup>10</sup> This is analogous to the so-called Kohnstamm effect described and experimentally studied by Rupprecht Matthaei, *Nachbewegungen beim Menschen*, *Archiv. f. d. ges. Physiol.*, 202, 1924, 88-111; 204, 1924, 587-600. This phenomenon is there described as follows: If one stands close to a wall with the arm hanging down, and then presses the back of the hand against the wall for 5 to 10 sec., an after-effect occurs when one steps away from the wall. The arm automatically rises away from the side of the body.



"A train is going to the right," "A man swinging a baseball bat," "There was smoke coming out of the stacks," "The baseball was coming at him."

(2) *Postural dynamics (MP)*. The second type of response, postural dynamics (MP), occurred only in those drawings which depicted human figures (Pictures 2, 3, 5 and 6 of Fig. 1). Examples are: "A man holding up a weight," "His hands are holding a bat."

(3) *Motion not indicated (NM)*. The large majority of our verbal statements did not indicate motion (NM). They merely consisted in naming the pictorial objects and in listing their component parts. For instance: "A boat on the sea. It has port-holes. A smokestack is facing the right"; "A skier. He has no hair or features."

The responses were scored independently by two judges. The scoring was done

TABLE I  
SIGNIFICANCE OF DIFFERENCE BETWEEN MEAN NUMBER  
OF RESPONSES INDICATING ACTIVE MOTION (MA)

Group	No. Ss	Mean	M <sub>Diff.</sub>	t	P
Control	17	3.2			
Experimental	12	1.5	1.7	3.82	<.01

without knowledge whether the responses were those of Ss belonging to the control or the experimental group. Inter-judge agreement was high (91.4% for MA judgments; 94.0% for MA + MP judgments; and 97.7% for NM judgments). The few disagreements between the judges were resolved by conference.

Each response was judged to be in one of the three categories. Since there were six pictures, each S's score for any of the three categories could range from 0 to 6. In a few instances S's response could not be obtained.

*Results and conclusions.* Table I presents the mean number of MA responses for the experimental and control groups. The mean number for the experimental group, 1.5, is significantly smaller than the mean number for the control group, 3.2.

TABLE II  
SIGNIFICANCE OF DIFFERENCE BETWEEN MEAN NUMBER OF RESPONSES INDICATING  
ACTIVE MOTION AND POSTURAL DYNAMICS MA+MP

Group	No. Ss	Mean	M <sub>Diff.</sub>	t	P
Control	17	4.8			
Experimental	12	2.8	2.0	5.38	<.01

Because of the dynamics implicit in the postural-gestural responses, MP, one might regard them as being close to those clearly indicating perceptual motion. Thus, a second analysis was done grouping together the MA and MP responses. The results of this analysis are presented in Table II. Again, by applying this dual criterion (MA + MP), one finds that there is a significant difference between the two groups, i.e. the experimental group shows a significantly smaller number of these responses.

Another approach to treating the data is by analyzing the mean number of NM responses rather than by dealing with the responses of motion. As one would expect,



Table III shows that the experimental group produced a significantly greater number of NM responses in comparison with the control group.

To conclude, the results of this experiment are in support of the hypothesis of

TABLE III  
SIGNIFICANCE OF DIFFERENCE BETWEEN MEAN NUMBER OF  
RESPONSES NOT INDICATING MOTION (NM)

Group	No. Ss	Mean	M <sub>Diff.</sub>	t	P
Control	17	.9			
Experimental	12	2.9	2.0	6.00	< .01

vicarious channelization. In accordance with expectation, the introduction of motor involvement has the effect of significantly decreasing perceptual movement as measured by verbal responses to pictorial material.



## FIGURAL AFTER-EFFECTS IN KINESTHETIC SPACE

By JACK NACHMIAS, Swarthmore College

The empirical phenomenon of figural after-effects in visual space has been shown to have, among others, two important characteristics: (1) the inspection of any visual contour (the *S* or satiation contour) displaces the apparent location of a contour subsequently seen (the *T* or test-contour) in such a way that the *T*-contour appears to have receded from the locus previously occupied by the *S*-contour.<sup>1</sup> (2) The absolute magnitude of this displacement at first increases as the geometrical distance between the *S*- and *T*-contours increases, and then decreases as that distance is further increased.<sup>2</sup> This second characteristic is of considerable importance because it enables us to account—at least in principle—for some of the instances of after-effects produced by curved or tilted lines.<sup>3</sup>

Evidence suggesting that similar after-effects exist in kinesthesia comes from experiments by Ponzo<sup>4</sup> and by Gibson.<sup>5</sup> They found that moving the fingers along a tilted (or curved) edge produces an apparent opposite tilt (or curvature) in a straight edge subsequently traversed. More recently, Köhler and Dinnerstein showed that the width of a strip, as perceived by moving the thumb and fingers along its parallel edges, grows or shrinks, depending on whether a narrower or wider strip has been previously 'inspected.'<sup>6</sup> All three of these experiments used a technique requiring *O* to move his hand, and thus involved the temporal integration of sense impressions, an element usually absent from visual figural after-effects investigations.

The two new experiments to be reported in this paper were performed to show that kinesthetic after-effects can be induced by a method more closely analogous to that employed in visual experiments. This method is based on the subjective comparison of the stationary position—relative to the body—of the two hands. Furthermore, the second experiment attempted to determine whether such kinesthetic after-effects display the second characteristic of visual after-effects. *i.e.*, whether the magnitude of the displacement passes through a maximum.

### EXPERIMENT I

In the first experiment only one hand was used during the satiation-period in an endeavor to determine whether kinesthetic after-effects were induced.

\* Accepted for publication September 23, 1952. The author is indebted to Dr. Wolfgang Köhler for suggesting this problem and for his guidance throughout the work.

<sup>1</sup> Wolfgang Köhler and Hans Wallach, Figural after-effects, *Proc. Amer. Phil. Soc.*, 88, 1944, 269-357.

<sup>2</sup> B. H. Fox, Figural after-effects: "satiation" and adaptation, *J. Exper. Psychol.*, 42, 1951, 317-326.

<sup>3</sup> Köhler and Wallach, *op. cit.*, 324.

<sup>4</sup> Mario Ponzo, La methode des variations continues des stimuli, *J. de Psychol.*, 30, 1933, 617-638.

<sup>5</sup> J. J. Gibson, Adaptation, after-effects, and contrast in the perception of curved lines, *J. Exper. Psych.*, 16, 1933, 1-31.

<sup>6</sup> Köhler and Dorothy Dinnerstein, Figural after-effects in kinesthesia, *Misc. Psychol. A. Michotte*, 1947, 196-220.



*Materials.* S's hands rested on horizontal metal bars clamped onto vertical metal bars. The vertical bars were marked at 1-in. intervals. A level was used to check the position of the horizontal bars.

*Observers.* Twenty undergraduates, naïve with respect to kinesthetic after-effects, served as *Os*. Every *O* was given only one satiation-period and asked to make only one judgment.

*Procedure.* *O* was asked to shut his eyes as soon as he entered the experimental room. During the satiation-period, one of his hands was placed for 60 sec. on a bar 4 in. above or below the test-bar; the other hand remained at his side. For the test, both hands were placed simultaneously on the same horizontal bar, and *O* was asked

TABLE I  
SHOWING FOR EVERY EXPERIMENTAL CONDITION THE NUMBER OF *Os* REPORTING  
THE VARIOUS EFFECTS OBTAINED IN EXPERIMENT I

Condition		Effect		
Hand satiated	Test of position	Expected	opposite	none
right	above	3	1	1
right	below	3	1	1
left	above	5	0	0
left	below	5	0	0
Total		16 (80%)	2 (10%)	2 (10%)

to report whether his hands now appeared to be at exactly the same height, and if not, which felt higher. Many *Os* changed their verbal report one or more times. On the assumption that, as in vision,<sup>7</sup> after-effects in kinesthesia take a little time to develop, only the first relatively stable impression was recorded.

For all the *Os*, the test-bar was located the same distance from the floor (4½ ft.). Ten *Os* had their *left* hands satiated, 10 their *right* hands; half in each sub-group were satiated on a bar 4 in. above the test-bar, and half on a bar 4 in. below it. No attempt was made to control either the distance that short and tall *Os* had to raise their hands, or the exact position in which the hands were held.

*Results.* If kinesthetic after-effects behave like their visual analogs, the satiated hand should appear displaced during the test away from the previously satiated locus. For example, with the right hand satiated, and the satiation-bar below the test-bar, the right hand would be expected to appear higher than the left when both are placed on the same bar.

From Table I it can be seen that 16 of the 20 *Os* reported an effect in the expected direction. The probability of such an event occurring by chance alone is less than 0.025, according to the one-tailed sign-test.<sup>8</sup> The existence of an after-effect in kinesthetic space having the first characteristic of visual after-effects—displacement of a test-contour away from the locus previously occupied by the satiation contour—has thus been demonstrated.

<sup>7</sup> Köhler and Julia Fishback, The destruction of the Müller-Lyer illusion in repeated trials: II. Satiation patterns and memory traces, *J. Exper. Psych.*, 40, 1950, 398-410.

<sup>8</sup> W. J. Dixon and F. J. Massey, *Introduction to Statistical Analysis*, 1951.



## EXPERIMENT II

In the second experiment an attempt was made to discover whether the magnitude of kinesthetic after-effects passes through a maximum, *i.e.*, whether they display the second characteristic of their visual analogs.

Although no simple method was found for measuring the absolute amount of displacement produced by any one satiation position, the *direction* of the difference between displacements from two positions can be ascertained in the following manner. Both hands are satiated simultaneously, with each hand kept at a different height; *e.g.* in Fig. 1, one hand ( $S_n$ ) is at distance  $d_n$  from its future test-position, while the other hand ( $S_f$ ) is at distance  $d_f$  from its test-position. In the test-period, the two hands are placed on the same test-bar ( $T-T'$ ). Now if the size of kinesthetic after-effects

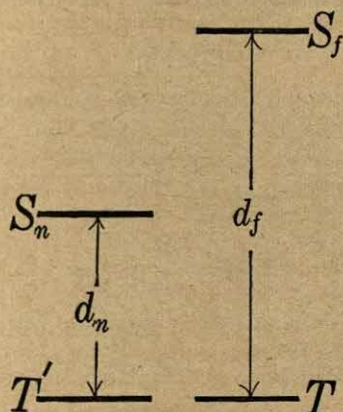


FIG. 1. SATIATION- AND TEST-POSITIONS IN EXPERIMENT II  
The distance  $d_n$  was varied from 2 to 10 in., and  $d_f$  from 4 to 15 in.

passes through a maximum, then for some values of  $d_n$  and  $d_f$ , the hand at  $T$  should be more affected than at  $T'$ , *i.e.* the right hand in this instance should feel *lower* than the left. As either both distances ( $d_n$  and  $d_f$ ) are increased, or as only  $d_f$  is increased, the *direction* of the difference in the strength of the effects must reverse—in this instance, the right hand should appear to be *higher* than the left.

**Materials.** The materials used in this part of the study were the same as those described in Experiment I.

**Observers.** Seventy-two undergraduates were used. As before, every  $O$  was satiated and tested only once.

**Procedure.** The procedure was the same as in Experiment I, with the following exceptions. During the 60 sec. satiation-period, both hands were satiated simultaneously, but on bars at different heights. Altogether five combinations of the critical distances  $d_n$  and  $d_f$  were used. The distance (see Fig. 1 and Table II)  $d_n$  was varied from 2 to 10 in. and  $d_f$  from 4 to 15 in.

Under each condition, an equal number of  $O$ s were satiated with the right hand above the left, and with the left hand above the right; similarly, an equal number



were satiated below and above the test-bar. Since only a small number of *O*s (8 or 16) were tested at each condition the results were not fractionated.

*Results.* Table II shows that no evidence was obtained to support the assumption that the size of kinesthetic after-effects passes through a maximum, as is the case with visual after-effects: the number of *O*s reporting a greater displacement from *S<sub>n</sub>* never exceeded the number reporting the opposite effect. It should be noted, however, that the proportion of *O*s reporting a differential effect in either direction declines as one or both satiation bars were set relatively far from the test-bar. Thus the proportion of 'no difference' judgments rose from 0.19 with Condition A to 0.37 with Condition E. This trend is not statistically significant.

There is, consequently, no convincing reason to believe that the various conditions of satiation were not equivalent so far as their effect is concerned. On this assumption, the results from all five conditions may be pooled. The frequencies so ob-

TABLE II

SHOWING FOR EVERY EXPERIMENTAL CONDITION THE NUMBER AND PERCENTAGE OF *O*s REPORTING THE VARIOUS EFFECTS OBTAINED IN EXPERIMENT II

Condition	Distance (in.)			Displacement greater		No difference
	<i>d<sub>n</sub></i>	<i>d<sub>f</sub></i>	<i>d<sub>f</sub>-d<sub>n</sub></i>	<i>S<sub>t</sub></i>	<i>S<sub>n</sub></i>	
A	2	4	2	13 (81%)	0	3 (19%)
B	2	10	8	7 (86%)	0	1 (14%)
C	4	10	6	10 (63%)	2 (12%)	4 (25%)
D	4	15	11	7 (44%)	2 (12%)	7 (44%)
E	10	15	5	7 (44%)	3 (19%)	6 (37%)
Total				44 (61%)	7 (10%)	21 (29%)

tained revealed that the proportion of *O*s (0.61) for whom the more distant satiation position produced the greater displacement was significantly greater than 0.5 ( $X^2 = 3.6$ ).

The results of Experiment II are compatible with one of the following hypotheses: (1) The size of after-effects in kinesthetic space *does not* pass through a maximum as the geometrical distance between test- and satiation-bars is increased. (2) It *does* pass such a maximum, but the particular distances employed in this experiment were not such as to bring this out.

There remains an entirely different kind of alternative explanation. It may be that when the hands are rested at different heights, an adaptation takes place, *i.e.* the perceived difference between the heights gradually diminishes; the apparent difference in height which is reported when the hands are subsequently placed at objectively the same height, would only be a manifestation of this adaptation.

According to this last principle, no after effects whatever would result from satiation of one hand alone. Since after-effects were obtained in Experiment I, the principle of adaptation cannot be the only one operative in kinesthetic after-effects induced by the technique employed in these experiments.



## STUDIES IN VICARIOUSNESS: DEGREE OF MOTOR ACTIVITY AND THE AUTOKINETIC PHENOMENON

By ALFRED E. GOLDMAN, Clark University

As elaborated by Krus, Werner, and Wapner,<sup>1</sup> the construct of vicarious channelization underlying this investigation is an outgrowth of the sensory-tonic field-theory of perception.<sup>2</sup> By vicarious channelization is meant that available energy may be released through different channels. In keeping with this general statement two specific hypotheses pertaining to the relation of motion and motion-perception can be formulated: (1) Under conditions of inhibition of motor expression perceptual activity is expected to increase; (2) Conversely, if motor expression is increased, perceptual activity is expected to decrease.

*Procedure.* The autokinetic phenomenon—that is the perceived movement of a stationary point of light in a dark room—was chosen as a means of testing the hypotheses advanced above. Of the many possible measures of the autokinetic phenomenon, two were selected because they were most objective, *i.e.* reaction-time and duration. By reaction-time is meant the interval from exposure of the light to the first report of movement. The assumption is that reaction-time measures readiness to perceive motion and thus gives an indication of one aspect of channelization of perceptual activity. Accordingly, a higher degree of readiness to perceive is reflected in shorter reaction-time. By duration is meant the duration of the first autokinetic motion cycle, *i.e.* the interval between the first report of movement and the cessation of movement. The assumption is that greater perceptual activity is reflected in longer duration.

The stationary point of light was observed by *S* under three experimental conditions: Immobilization (*I*), Free situation (*F*), and Mobility (*M*).

In Condition *I*, *S*'s immobilization was accomplished by the instruction not to move, and by strapping *S* to a specially constructed chair which prevented movement of the limbs, trunk and head. Immobilization took place 10 min. prior to and during the presentation of the stimulus. In Condition *F*, *S* was not strapped to the chair; he sat quietly except for the slight body movements which usually occur under such conditions. In Condition *M*, *S* was required to move both arms continually in a prescribed manner. This movement was maintained for 2 sec. prior to and during the exposure of the stimulus.

In all three test conditions *S*'s head was held in place by means of a head-rest.

*S* was requested to report: (1) the onset of apparent movement; (2) the cessation of the movement; (3) the directions and shifts of movement in terms of posi-

\*Accepted for publication October 22, 1952. This investigation was supported by a research grant to Clark University from the National Institute of Mental Health of the National Institutes of Health, Public Health Service.

<sup>1</sup>D. M. Krus, Heinz Werner, and Seymour Wapner, Studies in vicariousness: Motor activity and perceived movement, this JOURNAL, 66, 1953, 603-608.

<sup>2</sup>Werner and Wapner, Sensory-tonic field theory of perception, *J. Personal.*, 18, 1949, 88-107; Toward a general theory of perception, *Psychol. Rev.*, 59, 1952, 324-338.



tions of the hands of a clock. In addition S was encouraged to report everything else he noticed.

To acquaint Ss with the situation and the autokinetic phenomenon, two trials were given prior to the experiment proper. There were five trials in each of the three experimental conditions.

Each of the Ss (12 men and 12 women) was tested in the three experimental conditions. Two men and two women were tested in one of the six possible sequences

TABLE I  
SIGNIFICANCE OF DIFFERENCE AMONG TEST CONDITIONS: REACTION-TIME

Source of variance	df	F-tests			
		men		women	
		mean square	F	mean square	F
order	2	28.99	1.30	57.74	2.99
individuals	11	140.77		263.29	
sequence	5	78.70	3.52*	118.02	6.11†
individuals within					
sequence	6	192.50	8.62†	384.19	19.90†
conditions	2	198.64	8.89†	155.49	8.05†
error	20	22.34		19.31	
Total	35				

Means (sec.)			
	'immobilization'	'free'	'mobility'
men	6.6	7.6	14.1
women	8.8	11.3	15.9

		t-tests	(mean differences)
		men	women
		'free'	'mobility'
'immobilization'	1.0	7.5†	2.5
'free'		6.5†	7.1†
			4.6*

\* Significant at or below 5% level of confidence.

† Significant at or below 1% level of confidence.

of the three experimental conditions: I,F,M; I,M,F; etc. A modified latin-square factorial design was used.

*Results. (1) Reaction-time.* Table I summarizes the analysis of variance and tests of significance for the three experimental conditions. In accordance with expectation, for both men and women, the shortest reaction-time occurred for Immobilization; in the Free situation the reaction-time was longer; and for Mobility it was the longest. The reaction-time under Immobilization and in the Free situation did not differ significantly; but under Mobility it differed significantly from each of the other two conditions.

(2) *Duration.* According to our hypothesis, an increase of motor activity should



be accompanied by a decrease in duration of movement. This expectation was, in general, borne out by the results. Duration was longest under Immobilization, shorter for the Free condition, and shortest for Mobility (Table II). For men, the mean duration for Immobilization differed significantly from the durations obtained under the other two experimental conditions. Under Free and Mobility conditions the durations did not differ significantly from each other. For women, the relation between the scores for the three conditions was in the expected direction though the differences were not significant.

TABLE II  
SIGNIFICANCE OF DIFFERENCE AMONG TEST-CONDITIONS: DURATION

Source of variance	df	F-tests			
		men		women	
		mean square	F	mean square	F
order	2	235.66	.47	107.72	1.06
individuals	11	1226.05		224.67	
sequence	5	610.20	1.21	248.95	2.46
individuals within sequence	6	1739.25	3.45*	204.44	2.02
conditions	2	3078.68	6.10†	65.67	.65
error	20	504.70		101.35	
Total	35				

Means (sec.)			
	'immobilization'	'free'	'mobility'
men	68.7	49.0	37.0
women	24.7	21.5	20.2

		t-tests	(mean differences)
men			
'free'	'mobility'		
'immobilization'	19.7*	31.7†	3.4
'free'		12.0	4.5
			1.1
women			
'free'	'mobility'		
'immobilization'	19.7*	31.7†	3.4
'free'		12.0	4.5
			1.1

\* Significant at or below 5% level of confidence.

† Significant at or below 1% level of confidence.

(3) *Complexity.* An additional analysis was carried out pertaining to another aspect of autokinetic activity; namely, the complexity of the pattern of apparent motion. Complexity is defined in terms of number of shifts in direction of movement. Thus movement in a straight line or curved line is considered less complex than an angular, zig-zag motion. We assume that greater complexity of the pattern reflects greater perceptual activity. S's continuous report of direction of motion provided a means of measuring complexity. The responses were scored as follows: one point was assigned for a continuous direction of motion; for each change in direction an additional point was assigned up to a maximum of five points. A score of complexity was obtained for each trial and the scores for the five trials within a test-condition



These reports are concerned with such properties of the perceptual movement as velocity, magnitude, and kind of motion. It is interesting to note that of the 20 Ss who reported on relative velocity, 15 stated that the movement was fastest under Immobilization, whereas only 4 judged it most rapid under the Free condition and only one reported that the speed was greatest under Mobility. Fifteen Ss were able to judge with a high degree of certainty the relative extent of apparent distance covered by the light. These reports provide a result analogous to that obtained for velocity: the light was seen as going farthest under the condition of Immobilization



by 11 Ss, under the Free condition by 3, and under Mobility by 1 S.<sup>3</sup>

*Conclusions.* The results of this study indicate that the greater the degree of motor involvement (1) the longer the time of appearance of autokinesis, (2) the shorter the duration of its first uninterrupted phase and (3) the less complex is its pattern of movement. These results support the postulate of vicarious channelization, according to which an inverse relation is expected between the amount of perceptual movement and motor activity.

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<sup>3</sup> There are other aspects of motion implied in 'Ss' reports which might be investigated in future research. For instance, their reports may be categorized in terms of degree of forcefulness, definiteness of direction, etc. Some of the Ss described the light as "not seeming to go anywhere; like a rising balloon," "like a puff of air could blow it away." Contrast this with movement descriptions such as "clearly directed," "definite," "action going through space." It is possible that the application of such categorical analysis might yield systematic relationships between degree of motor involvement and energies of the perceived motion.



## INTENTIONAL AND INCIDENTAL LEARNING WITH DIFFERENT RATES OF STIMULUS-PRESENTATION

By EDITH NEIMARK and IRVING J. SALTZMAN, Indiana University

In all of the studies comparing intentional and incidental learning, except one recently reported by Saltzman,<sup>1</sup> intentional learning has been found to be more efficient than incidental learning. In contrast with the results of the previous studies, Saltzman failed to find a difference between groups on a recognition-test for numbers after one of the groups had been instructed to learn the numbers (intentional learning) and the other group had not been so instructed (incidental learning). The procedure used by Saltzman differed in two major respects from that usually employed by investigators in this area. First of all, the intentional learners were required not only to learn the numbers, but also to perform the same orienting task required of the incidental learners. This orienting task consisted of sorting the numbers, typed on cards, into four mutually exclusive categories as rapidly as possible. Secondly, *E* did not control the rate at which the numbers were presented; every *S* set his own rate by the speed with which he performed the orienting task. Saltzman concluded that the greater efficiency of intentional learning over incidental learning found in the previous studies is not entirely due to giving the intentional learners instructions to learn, but is in part due to not requiring them to perform the orienting task. He suggested, furthermore, that the rate of presentation of the learning material is an important parameter affecting the magnitude of the difference between the scores of intentional and incidental learning.

### THE EXPERIMENT

The purpose of the present study was to investigate the relationship between rate of presentation of the materials to be learned and the magnitude of the difference between the scores of incidental and intentional learning (a) when the intentional learners perform the orienting task, and (b) when they do not.

*Procedure.* The *Ss* (180 undergraduate students) were randomly divided into three groups of 60: Group I, an intentional learning group in which the *Ss* were not required to perform the orienting task; Group II, an incidental learning group; and Group III, an intentional learning group in which the *Ss* were required to perform the orienting task. Each group was further divided into three subgroups of 20 *Ss* each. For one subgroup in each group, the learning material was presented at a rate of one item every 2 sec.; for another subgroup in each group, it was presented at a rate of one item every 3 sec.; and for the third subgroup in each group, it was presented at a rate of one item every 6 sec. The learning material was a list of 14 two-digit numbers selected from a table of random numbers. The list was presented to each *S* four times by means of an electrically operated

\* Accepted for publication October 23, 1952.

<sup>1</sup>I. J. Saltzman, The orienting task in incidental and intentional learning, this JOURNAL, 66, 1953, 593-597.



drum. A 10-sec. period intervened between successive presentations of the list. A different random order of the numbers was used on each of the four presentations.

*Instructions: Group I.* The Ss in Group I were read the following instructions:

We wish to see how fast you can learn numbers. The numbers you are to learn will appear one by one in this slot. There are 14 numbers ranging from 11 to 99. A short rest-period will follow each series of numbers. There are four series. You are to try to learn as many numbers as you can.

*Group II.* The Ss in Group II were read the following instructions:

We are interested in finding out the best way to code numbers. Some numbers will appear in this slot one by one. As soon as a number appears, we wish you to circle that number on a coding matrix. There are 14 numbers ranging from 11 to 99. We have four different kinds of matrices that we wish to try, so we will repeat the procedure four times, using a different matrix each time. There will be a short rest-period after each matrix is used. During the rest-period you will be able to inspect the next matrix to see how the numbers are set up.

The matrices were mimeographed sheets containing the numbers 11 to 99 arranged in sequence. On one matrix the numbers were arranged in four columns, on another, in five columns, on the third, in nine columns, and on the fourth, in 10 columns. On two of the matrices the numbers were arranged horizontally; on the other two matrices the numbers were arranged vertically.

*Group III.* The Ss in Group III were read the following instructions:

We wish to see how fast you can learn numbers. There will be 14 numbers ranging from 11 to 99. A short rest-period will follow each series of numbers. There will be four series. When a number appears in this slot, you are to circle it on this sheet. During the rest-period you will be given another sheet with the numbers arranged somewhat differently. You are to repeat the same procedure with this, and so on for the four sheets. Remember, you are to learn as many of the numbers as you can.

Summarizing, the Ss in Group I were not required to circle the numbers; they were merely asked to learn the numbers. This is the traditional procedure used

TABLE I  
MEANS OF DERIVED RECOGNITION-SCORES

Group	Rate of presentation (in sec.)		
	2	3	6
I	5.24	6.39	6.56
II	4.78	4.33	4.26
III	3.96	5.18	6.41

in studies on intentional learning. The Ss in Group II were not asked to learn the numbers; they were merely required to circle the numbers. This again is the traditional procedure in incidental learning. The Ss in Group III were required to circle the numbers and were also asked to learn the numbers. This is the procedure used recently by Saltzman.

Immediately following the fourth presentation of the numbers, all the Ss were given a recognition-test. The 14 numbers of the learning series plus 42 additional two-digit numbers were randomly arranged in four columns on a mimeographed



sheet. *S* was given 2 min. in which to circle all the numbers he recognized as having appeared on the drum.

*Results.* Scores on the recognition-test were derived by subtracting from the number of correctly circled numbers, the number that might be expected to be correct by chance. For example, if *S* circled 12 numbers, 10 of which were correctly circled, his score would be 10 minus 3 ( $12/4 = 3$ ), or 7. (One of every four numbers circled was considered to be correct by chance.) Means of the derived scores were computed for each group and are shown in Table I. An analysis of variance yielded an *F* of 9.935 for group differences and an *F* of 4.629 for presentation rate differences. Both of these values are significant at the 1-% level

TABLE II  
DIFFERENCES IN MEAN SCORES BETWEEN GROUPS AT EACH RATE OF PRESENTATION  
Rate of presentation (in sec.)

Groups	2	3	6
I-II	0.46 $t=0.77; P<.50$	2.07 $t=3.24; P<.01$	2.30 $t=3.74; P<.01$
II-III	0.82 $t=1.59; P>.10$	-0.86 $t=1.28; P\approx.25$	-2.15 $t=2.93; P<.01$
I-III	1.28 $t=2.14; P<.05$	1.21 $t=2.18; P<.05$	0.15 $t=0.02; P>.90$

of confidence. Table II contains the differences in mean scores between groups at each of the presentation rates. The *t*-values for the differences and their corresponding probabilities are also shown. At the fastest rate of presentation (one number every 2 sec.) none of the differences is significant at the 1-% level of confidence. At this fastest rate, then, neither group of intentional learners is more efficient than the group of incidental learners. At the next rate of presentation (one number every 3 sec.) the mean score of Group II is not significantly different from the mean score of Group III, but it is reliably smaller than the mean score of Group I. At this slower rate of presentation, then, intentional learning with performance of the orienting task is not more efficient than the incidental learning, but intentional learning without performance of the orienting task is more efficient than the incidental learning. The difference between the two conditions of intentional learning is not significant at the 1-% level of confidence. At the slowest rate of presentation, one number every 6 sec., the mean score of Group II is significantly smaller than the mean of both Group I and Group III. At this slowest rate of presentation, then, intentional learning, either with or without performance of the orienting task, is more efficient than incidental learning. Again, the difference between the two conditions of intentional learning is not statistically significant.

Inspection of Table I shows that the mean scores of both groups of intentional learners increase and the mean scores of the group of incidental learners decrease as the rate of presentation decreases. Only the difference between the 6- and the 2-sec.



rates for Group III is, however, significant at the 1-% level of confidence. Table III shows the differences in mean scores between presentation rates for each of the three groups. The table also shows the  $t$ -values and their corresponding probabilities.

*Discussion.* The results of this study support the suggestion made by Saltzman that the rate of presentation of the learning material is an important parameter in studies comparing the efficiency of incidental and intentional learning.<sup>2</sup> When the

TABLE III  
DIFFERENCES IN MEAN SCORES BETWEEN RATES OF PRESENTATION FOR EACH GROUP

Presentation rates (in sec.)	Group		
	I	II	III
6-3	0.17	-0.07	1.23
	$t=0.30; P \approx .75$	$t=0.01; P < .90$	$t=1.74; P > .10$
6-2	1.32	-0.52	2.45
	$t=2.17; P < .05$	$t=0.90; P < .40$	$t=3.61; P < .01$
3-2	1.15	-0.45	1.22
	$t=1.91; P > .05$	$t=0.71; P > .50$	$t=2.21; P < .05$

learning material is presented at relatively fast rates, the scores of incidental and intentional learning do not differ significantly. This is true whether or not the intentional learners are required to perform the orienting task. When the learning material is presented at relatively slow rates, the scores of intentional learning, either with or without the intentional learners performing the orienting task, are significantly higher than the scores of incidental learning. Any statements about the relative efficiency of intentional and incidental learning must, consequently, be qualified in terms of the rate at which the learning material is presented.

<sup>2</sup> Saltzman, *op. cit.*, 593-597.



## VERBAL TRANSFER OF OVERLEARNED FORWARD AND BACKWARD ASSOCIATIONS

By E. RAE HARCUM, The Johns Hopkins University

Given association A—B, subsequent tendencies for stimulus A to elicit response B and for B to evoke A are by definition forward and backward associations, respectively. Research on forward associations has produced the Bruce-Wylie paradigm of transfer, which predicts positive transfer when an old response is made to a new stimulus, and negative transfer when a new response to an old stimulus is learned.<sup>1</sup> The present study tests the hypothesis that backward associations also exert a verbal transfer effect which can be predicted from the Bruce-Wylie paradigm.

Backward associations have been inferred previously from perseveratory errors in serial learning, from faster learning for reversed orders of derived serial lists, and from direct count of subsequent responses to stimuli originally imbedded in a series. Using the last technique, Hermans<sup>2</sup> and Wohlgenuth<sup>3</sup> found that subjects set to learn forward associations gave responses attributable to backward association approximately one-third as often as forward associations. Therefore, in the present experiment the transfer effect of backward relative to forward association was expected to be about one-third.

*Method: Subjects.* The Ss, university students, were divided randomly to five groups. They learned by the anticipation method a practice list and two experimental lists of eight paired-associate nonsense syllables. Those not responding correctly to 3 or more syllables on any one trial of the practice list within 15 trials were dropped from the study. Final N was 10 Ss in each group. The learning criterion for the experimental lists was two successive errorless presentations.

*Materials.* The syllables consisted of two consonants and a vowel. They were projected from a continuous film strip within a cabinet onto a milk glass screen built into the front of the cabinet. Frames were indexed into the projector at a 2.5-sec. rate, but were visible for only approximately 1.8 sec. since the projection light was off during indexing. One blank frame separated successive syllable pairs and four blank exposures separated successive presentations of the same list. S rested 2 min. between lists.

*Procedure.* The relationship of the syllable components in the first or pre-training list to the syllables in the second or test-list determined the experimental condition. In Group C I the pre-training list was unrelated to the test-list. This may also be called the U, or unrelated group. The second list of Group C II employed responses

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\* Accepted for publication October 22, 1952. My thanks are due Dr. James E. Deese for his supervision of this study and Mr. Ray Hyman for his advice concerning the statistics.

<sup>1</sup> J. A. McGeoch, *The Psychology of Human Learning*, 1942, 613.

<sup>2</sup> T. G. Hermans, A study of the relative amounts of forward and backward association of verbal material. *J. Exper. Psychol.*, 19, 1936, 769-775.

<sup>3</sup> A. Wohlgenuth, On memory and the direction of associations, *Brit. J. Psychol.*, 5, 1913, 447-465.



unrelated to any syllables in the pre-training list, but the stimuli in the two lists were identical. This is the  $S=S$  or identical stimulus-condition. Group C III contained identical responses but unrelated stimuli. It is the  $R=R$  or identical response-condition. The three C-groups were used merely for control, while the two remaining groups (Groups E I and E II) were given the experimental conditions.

In Group E I the response-items of the pre-training list became the stimulus-items for the second list. The other components were unrelated. This group may be designated the  $R=S$  condition in which an old response becomes the new stimulus for subsequent learning. If backward association occurs in the pre-training list, then the response-items in that list are in effect responses to forward associations,

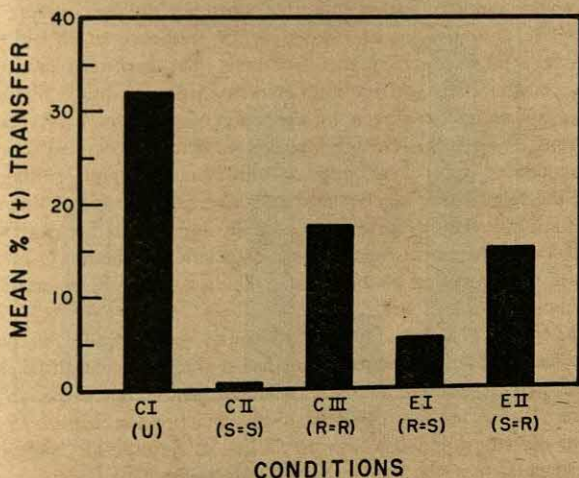


FIG. 1. MEAN PERCENTAGE POSITIVE TRANSFER COMPUTED FROM TRIALS TO CRITERION.

but stimuli in backward associations. When these syllables are used as stimuli for forward associations in the test-list, then they can be considered old stimuli to which new responses must be learned and negative transfer can be predicted. In Group E II the stimulus-items of the pre-training list became responses in the second list and the other items were unrelated. This may be called the  $S=R$  condition, where the old stimulus becomes the new response. Assuming backward association in the pre-training list, one can predict positive transfer by reasoning similar to that used with Group E I.

Percentage of transfer was computed from trials to criterion and from total errors. The formulas used to compute transfer were among those suggested by Gagné, Foster, and Crowley permitting transfer scores with a range of  $\pm 100\%$ <sup>4</sup>. Results reported are from statistical analysis on an arc sine transformation of original percentage of transfer scores.

<sup>4</sup> R. M. Gagné, Harriet Foster, and Miriam Crowley, The measurement of transfer of training, *Psychol. Bull.*, 45, 1948, 97-130.



*Results.* Results are essentially the same for trial (Fig. 1) and for error transfer scores (Fig. 2). Immediately noticeable is the positive transfer in all groups—particularly in Group C I. This is undoubtedly due to a large practice effect. Compared to Group C I, the other groups exhibit negative transfer. This would be predictable from the Müller-Schumann law of associative inhibition.<sup>5</sup> Having learned any of these syllables in a different context makes it more difficult to learn new associations to that syllable than to learn completely fresh material. The deviation from the Bruce-Wylie model is due probably to overlearning in the present experiment. These lists may be considered overlearned in comparison to Bruce's study in which the pre-training lists were presented for 0, 2, 6, or 12 trials.<sup>6</sup> His data indicate a decrease in negative transfer effects as the pre-training list is

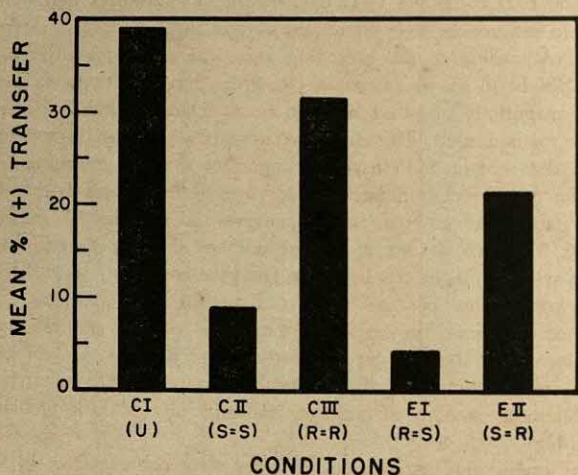


FIG. 2. MEAN PERCENTAGE POSITIVE TRANSFER  
COMPUTED FROM TOTAL ERRORS.

better learned. Irion has recently pointed out the transient nature of negative transfer and how it sometimes dissipates with greater degrees of learning.<sup>7</sup>

In both figures, Groups C II and E I show approximately equal amounts of positive transfer. Similarly, Groups C III and E II exhibit nearly equal positive transfer. Notice that there are greater facilitating effects of transfer in Groups C III and E II in comparison to the transfer effects in Groups C II and E I. This relative difference corroborates the Bruce-Wylie paradigm and supports the present hypothesis.

The difference between Groups C I and C II in Fig. 1 is significant at the 1-% level, but it is not significant in Fig. 2. There is heterogeneity of variance between these groups in Fig. 2.

A difference between either of the E-groups and Group C I would demonstrate

<sup>5</sup> G. E. Müller and F. Schumann, Experimentelle Beiträge zur Untersuchung des Gedächtnisses, *Z. Psychol.*, 6, 1894, 81-190: 256-339.

<sup>6</sup> R. W. Bruce, Conditions of transfer of training, *J. Exper. Psychol.* 16, 1933, 343-361.

<sup>7</sup> McGeoch and A. L. Irion, *The Psychology of Human Learning*, 1952, 338.



backward association according to the present hypothesis. The difference between Groups E II and C I is not significant. The large difference between Groups C I and E I is significant, however, at the 4-% level in Fig. 1 and the 1-% level in Fig. 2. The interpretation of this latter value is doubtful considering heterogeneity of variance between these distributions.

The variance of transfer scores computed from errors (Fig. 2) were significantly different between Groups C I and C II, C I and C III, C I and E I, C III and E II, and between E I and E II. Although there were no significant differences between the variances of the trial-scores (Fig. 1), they varied in the same direction as the error scores.

In Fig. 2, Groups C II and E I considered together were significantly different from C III and E II combined ( $p = 0.029$ ), and the variances were homogeneous. No other mean differences were significant beyond the 5-% level.

*Discussion.* According to the preceding argument a transfer effect of backward associations has been shown. Contrary to prediction, however, no demonstrable difference in magnitude of effect between forward and backward associations was found by the method used. This strongly suggests an alternative hypothesis to account for the data obtained. Perhaps the important variable governing verbal transfer is not the S—R relation between previous learning and later learning, but whether an item from previous learning serves as stimulus or response in the later learning. The two groups in this experiment showing least positive transfer have stimulus-syllables in the test-list taken from the previously learned list. The two groups showing greater positive transfer have the response-items in the new learning carried over from the earlier learning. This suggests that *the ease of learning association A—B depends upon whether S is familiar with A or B*. Since the Bruce-Wylie paradigm can also explain this data, further research is needed. Degree of learning, amount of practice, and difficulty of task probably are important variables.

Differences among the distribution of scores were not expected. In Group C I, where the material in the test list was unfamiliar to S transfer was, however, greatest and variability least. The variability in Group E II was slightly larger than in Group C I. It can be inferred from this that the relatively small variability in Group E II was caused by transposed items that were less well learned in the pre-training list. This is reasonable since stimulus-items must merely be recognized and not reproduced. They were not, moreover, used later in the same position as they were learned on the pre-training list. Also, the variance of Group E I, where the transposed item was a response on the pre-training list, is larger than Group C II, where the transposed item was a stimulus. In Group C III, where the response in the pre-training list is transferred as a response in the test-list, the greatest variability is seen.

In Groups C II and E I, where transferred items are stimuli in the test-list, there is a suggestion of bi-modality. This result, if valid, would suggest that, for some individuals, stimulus-familiarity facilitates learning and, for others, it retards learning.



## MANIPULATIVE COMPLETION OF BISECTED GEOMETRICAL FORMS BY NURSERY SCHOOL CHILDREN

By ARTHUR I. SIEGEL, Institute for Research in Human Relations, and  
HALIM OZKAPTAN, Fordham University

Schiller and Hartmann recently presented a series of bisected geometrical forms to 50 graduate students with instructions to "make some changes in their arrangement by altering the position of one (piece) or the other."<sup>1</sup> Their results indicated that from a total of 3,750 adjustive placements, the most 'perfect' completion possible (*i.e.* minimal perimetry) was obtained in 3,288 or 87.6% of the cases. They postulated, as an explanatory principle, that "the minimal perimeter, the symmetry,

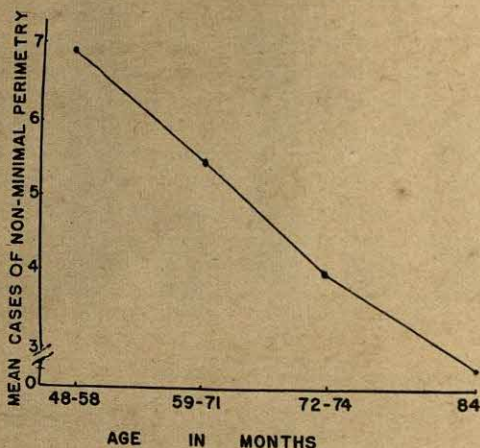


FIG. 1. MEAN NUMBER OF CASES OF NON-MINIMAL PERIMETRY PLOTTED AGAINST AGE IN MONTHS

and the esthetic values suggest 'the good gestalt' and the principle of 'closure.' " An alternative and more parsimonious explanation in terms of past conditioning and familiarity may, however, be advanced. It is possible that their adult Ss produced figures of minimal perimetry because of past experience and conditioning to 'closed' and symmetrical forms. If this, is so, we should expect that nursery school children who presumably have less past visuo-associate experiences and familiarity with these types of figures would produce fewer figures of minimal perimetry than did the graduate students of Schiller and Hartmann. The present experiment was designed to investigate this hypothesis.

*Procedure.* The 16 geometrical forms used by Schiller and Hartmann were em-

\* Accepted for publication October 30, 1952.

<sup>1</sup> Paul Schiller and George Hartmann, Manipulative completion of bisected geometrical figures, this JOURNAL, 64, 1951, 238-246.



played as stimulus-objects in the present study. Our forms deviated from the original, however, in that we colored each form with crayon. Twenty-six nursery school children (7 at age 4 yr., 16 at 5 yr., 2 at 6 yr., and 1 at 7 yr.) were used as *Ss*. The experiment was performed in the nursery of the Bethesda Community School.<sup>2</sup> *E* first played various table top nursery games with *S*.<sup>3</sup> After establishing rapport, he instructed *S* as follows: "We are going to play with these colored pieces of cardboard. You can make anything you like." As *E* said this, the pieces of the first stimulus-form were placed on the table in front of *S*. The cut edges were never allowed, however, to face each other. After *S* had made a figure, the pieces were taken from him and he was told, as the pieces of the second stimulus-form were placed before him: "Now make something with these." This procedure was continued until all 16 of

TABLE I  
THE PERCENTAGE OF CLOSURE OBTAINED WITH DIFFERENT TYPES OF FIGURES  
Nature of cut and size of pieces

Figure	straight		broken		curved		wavy		toothed		Mean.
	eq.	uneq.	eq.	uneq.	eq.	uneq.	eq.	uneq.	eq.	uneq.	
Compact											
circle								76.9			76.9
triangle*		19.2					80.7				49.9
ellipse		80.7									80.7
square					73.1						73.1
hexagon			30.8				76.9				53.9
pentagon			46.9						76.9		61.9
oblong†									80.7		80.7
mean all compact											64.2
Complex											
star (4, 6 points)		15.4			73.1						44.3
star (3, 5 points)					61.5						71.1
mean all stars					80.7						57.7
cross		7.7									7.7
amorphous‡					53.8						53.8
mean all complex											48.7
General average	7.7	38.4	38.9	65.3	73.1	78.8	76.9		78.8		58.4

\* Isolateral.

† Golden section.

‡ Oak leaf.

the forms had been presented. A preconceived, random, serial order of presentation was employed for the 16 forms.

*Results.* Out of 416 adjustive placements made, minimal perimetry was obtained in 243 placements (58.4%). The remaining 173 instances included: (a) matching without producing the smallest possible circumference, and (b) approximation along the actual bisecting line without actual contact. The difference between our results which indicated 58.4% 'closure' for nursery school children and the Schiller-Hartmann results of 87.6% 'closure' for graduate students is significant beyond the 1% level of significance.

Fig. 1, in which age is plotted against the mean number of times non-minimal perimetry was obtained, indicates that as age increased the mean number of times

<sup>2</sup> We wish to acknowledge our indebtedness to the personnel of the Bethesda Community School, Bethesda, Md., for their cooperation in supplying us with the *Ss* and the space in which to conduct the study.

<sup>3</sup> Halim Ozkaptan served as *E* throughout the study.



non-minimal perimetry was obtained decreased. It is unfortunate that children under four years of age were unable to consummate the procedure, in spite of the best efforts of the experimenter, and regardless of the degree of rapport established (in one case a nephew-uncle relationship). The S under 4 yr. of age who made any adjustive motions whatever merely superimposed the pieces.

In contrast to Schiller and Hartmann, our Ss manipulated the 'compact' figures into 'closed' forms more often than 'complex' figures. Our difference, however, is not statistically significant. The results of the present experiment also indicate, in agreement with Schiller and Hartmann, more closure for non-straight than for straight cuts, and, in contrast to them, more closure for parts of unequal than of equal size.

*Discussion.* The results indicate a significant difference between the percentage of manipulative adjustments producing minimal perimetry of bisected geometrical forms by nursery school children, as compared with graduate students, and also indicate that as the age of our Ss increased, the mean number of cases of minimal perimetry increased. This lends support to our hypothesis that 'closure' is a function of past conditioning and familiarity. We propose that Schiller and Hartmann's adult Ss were more familiar, due to their richer visual-associative background, with good 'gestalts' than with poor ones. It is possible that these associations have been reinforced throughout life and generalized to the experimental situation. It is interesting to note that Schiller and Hartmann indicate a significant difference in closure between their circle and their amorphous figure. We were able to confirm this in children. This last point, as well as the fact that the nursery school children produced more cases of minimal perimetry with 'compact' than with complex figures, lends support to our previous hypothesis of familiarity and conditioning. This follows, because we would expect that nursery school children, by nature of their past experience and nursery school training, would be familiar with 'compact' and regular figures, rather than 'complex' and irregular figures. It is also interesting to note that the 5-yr. old child used by Schiller and Hartmann showed 78% closure or was one standard deviation below their group mean.



## NOTES AND DISCUSSIONS

### METHODOLOGY FOR STUDYING FIGURAL AFTER-EFFECTS AND PRACTICE EFFECTS IN THE MÜLLER-LYER ILLUSION

On the basis of recent findings by the author, certain controls are required in two kinds of experiments.<sup>1</sup> One kind of experiment is often used to study figural after-effects; the other deals with 'practice effects' in the Müller-Lyer illusion.

(1) *Figural after-effects.* A common method of measuring figural after-effects is to place an outline inspection-figure on one side of a fixation-point. After the inspection-figure has been viewed for a time, it is removed and two outline test-figures are shown. One test-figure is inside, outside, or congruent with the previous locus of the inspection-figure, while the other test-figure is symmetrically placed on the opposite side of the fixation-point. If the two test-figures differ in apparent size, the difference is considered a measure of the figural after-effect. In other words, the entire difference is considered due to previous stimulation by the inspection figure. The basic assumption of this method of measuring figural after-effects is that apparent size is equal in opposite halves of the visual field when no inspection-figure is used. Köhler and Wallach, in their extensive study of figural after-effects, recognized the basic assumption of this method of measurement and considered the assumption valid.<sup>2</sup> They recommend the use of test-patterns which are symmetrical with respect to the main axes of space, stating that "Under these conditions intrinsic inhomogeneities of the visual field play but a minor rôle."<sup>3</sup> Several more recent studies have used this same methodology.<sup>4</sup>

Marks,<sup>5</sup> Weitz and Post,<sup>6</sup> and Weitz and Compton,<sup>7</sup> however, all ob-

<sup>1</sup> K. T. Brown, Factors affecting differences in apparent size between opposite halves of a visual meridian, *J. Opt. Soc. Amer.*, 43, 1953, 464-472.

<sup>2</sup> Wolfgang Köhler and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. Phil. Soc.*, 88, 1944, 269-357.

<sup>3</sup> *Ibid.*, 272.

<sup>4</sup> Köhler and Julia Fishback, The destruction of the Müller-Lyer illusion in repeated trials: II. Satiation patterns and memory traces, *J. Exper. Psychol.*, 40, 1950, 398-410; W. C. H. Prentice, On the size of the figural after-effect with varying distances, this JOURNAL, 63, 1950, 589-593; A. S. Luchins and E. H. Luchins, On the relationship between figural after-effects and the principle of prägnanz, this JOURNAL, 65, 1952, 16-26.

<sup>5</sup> M. R. Marks, A further investigation into the Köhler effect, this JOURNAL, 62, 1949, 62-74.

<sup>6</sup> Joseph Weitz and Dorothy Post, A stereoscopic study of figural after-effects, this JOURNAL, 61, 1948, 59-65.

<sup>7</sup> Weitz and Bertita Compton, A further stereoscopic study of figural after-effects, this JOURNAL, 63, 1950, 78-83.



tained reports of differences in apparent size both with and without the inspection-figure. Marks called both kinds of differences in apparent size "K-effects," or figural after-effects, but only differences in apparent size which are produced by an inspection-figure can be called figural after-effects. Since Weitz and Post obtained the same general results both with and without inspection-figures, they expressed doubts concerning the reality of figural after-effects. Weitz and Compton likewise state that, "It appears, then, that our results cannot be called 'figural after-effects.'"<sup>8</sup>

The differences in apparent size found by all three of these studies can be explained, however, if it is assumed that each study dealt with two separate phenomena. One phenomenon is differences in apparent size which are found without inspection-figures, while the other is the true figural after-effect. When no inspection-figure was used, only the former phenomenon could have been measured, but measurements with the inspection-figure would have been influenced by both phenomena. In these three studies the Ss merely reported whether one of the test-objects appeared smaller than, larger than, or equal in size to the other test-object. Thus there was no adequate method of distinguishing, in a given S, between differences in apparent size which exist without inspection-figures and differences in apparent size which may properly be called figural after-effects.

In the author's recent study, measurements were made of differences between apparent size in opposite halves of a visual meridian passing through the fixation-point. These differences in apparent size were called half-meridional differences (*HMDs*) and they were measured by equating the apparent lengths of lines in opposite halves of a visual meridian. Measurements were made on six Ss for both the horizontal and vertical meridians, using the right eye, left eye, and both eyes. Under each of the six conditions of measurement, some Ss had statistically significant *HMDs*. These *HMDs* varied among the Ss in both direction and magnitude. Thus there are differences in apparent size between opposite halves of the visual field, even when no inspection-figure is used. The existence of *HMDs* casts no doubt on the reality of figural after-effects, because figural after-effects have been demonstrated clearly by both Prentice and Hammer, using after-effects of figure displacements which did not involve *HMDs*.<sup>9</sup> Particularly clearcut figural after-effects were also obtained by Hochberg and Bitterman,<sup>10</sup> who

<sup>8</sup> *Ibid.*, 83.

<sup>9</sup> Prentice, The relation of distance to the apparent size of figural after-effects, this JOURNAL, 60, 1947, 617-623; E. R. Hammer, Temporal factors in figural after-effects, *ibid.*, 62, 1949, 337-354.

<sup>10</sup> J. E. Hochberg and M. E. Bitterman, Figural after-effects as a function of the retinal size of the inspection-figure, this JOURNAL, 64, 1951, 99-102.



measured *HMDs* and took them into account.<sup>11</sup> Thus figural after-effects and *HMDs* are different phenomena which can be demonstrated independently.

The method of measuring figural after-effects recommended by Köhler and Wallach, however, is contaminated by *HMDs* and hence contains large errors. One reason why these errors are large is that *HMDs* are large relative to the figural after-effects which have been reported. The largest figural after-effects published to date are those of Köhler and Wallach.<sup>12</sup> They report an average figural after-effect for six *Ss* of 10.0%, when the percentage values are recomputed by the same method used for computing the percentage values of *HMDs*. On the other hand, when the author measured binocular *HMDs* in the horizontal meridian, which corresponds to the conditions used by Köhler and Wallach, the average *HMD* regardless of sign was 6.6%. It might be objected that the results of these two studies are not comparable because Köhler and Wallach used black circles on a white ground, whereas the author used white lines on a black ground. However, unpublished measurements of *HMDs* have also been made using a method identical in all essentials to the method of Köhler and Wallach for measuring figural after-effects,<sup>13</sup> except that no inspection figure was employed. A separate group of eight *Ss* was used, and *HMDs* were also found with this method. These *HMDs* were statistically significant in five *Ss*, variable among the *Ss* in direction, and as large as 4.7%. The average *HMD* for all eight *Ss*, regardless of sign, was 1.8%. A second reason why the errors in figural after-effect measurements are large is that *HMDs* occur in different directions in different *Ss*. Thus the *HMDs* of some *Ss* will make the figural after-effect appear too large, while in other *Ss* the opposite will occur. In some cases the *HMD* could even be larger than the figural after-effect and in the opposite direction. This would cause the figural after-effect to appear negative, and negative figural after-effects have been reported by Weitz and Post. Hence it cannot be assumed, under the conditions prescribed by Köhler and Wallach, that intrinsic inhomogeneities of the visual field play only a minor rôle. When measuring figural after-effects under these conditions, *HMDs* must be measured and taken into account.

(2) *Practice effects in the Müller-Lyer illusion.* Many studies have

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<sup>11</sup> The existence of *HMDs* was not indicated in this study. Only the average *HMD* for all 10 *Ss* was reported and this average value was not significantly greater than zero. Since significant *HMD* occur in opposite directions in different *Ss*, however, an average value is not suitable for demonstrating *HMDs*.

<sup>12</sup> *Op. cit.*, 283.

<sup>13</sup> *Ibid.*, 283 f.



demonstrated that the Müller-Lyer illusion usually diminishes with repeated testing.<sup>14</sup> Early investigators generally considered the Müller-Lyer illusion to be the result of an error of judgment. Thus they attributed the reduction in magnitude of the illusion to the gradual assumption by the *S* of a more objective attitude in judging the lengths of the two halves of the Müller-Lyer figure. Köhler and Fishback, however, believe the practice effect is due to a more rapid development of satiation on the more enclosed side of the figure.<sup>15</sup> The resultant asymmetrical satiation then causes the enclosed side to increase in apparent length relative to the other side. Both of these theories attribute the entire practice effect to the specific shape of the Müller-Lyer figure. Thus both theories assume that the relation between apparent size in opposite halves of the visual field does not change with repeated trials unless the Müller-Lyer figure is used.

In the author's measurements of *HMDs*, simple straight line stimuli were used. Nevertheless, under all six conditions of measurement, the *HMDs* of some *Ss* were found to change with repeated testing in a trendlike manner. These trends were often quite marked and they continued in the same direction for as long as eight weeks of testing. Thus the apparent size relation between opposite halves of the visual field sometimes changes with repeated testing, even when straight line stimuli are used. This means that changes which occur in the Müller-Lyer illusion must be regarded as a special case of a general phenomenon, rather than as an entirely distinctive phenomenon. Furthermore, changes in the Müller-Lyer illusion which have been found with repeated testing cannot be assumed to be due entirely to the shape of the Müller-Lyer figure.

From the standpoint of methodology, effects of repeated testing which are due to the shape of the Müller-Lyer figure can be studied by making concomitant tests on the same *S* with both the Müller-Lyer pattern and a similar pattern which does not have the distinctive angles of the Müller-Lyer figure. The magnitude of the Müller-Lyer illusion at any given time of testing will then be the difference between the two kinds of measurement. Thus the effect of repeated testing which is due to the shape of the Müller-Lyer

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<sup>14</sup> C. H. Judd, Practice and its effects on the perception of illusions, *Psychol. Rev.*, 9, 1902, 27-39; The Müller-Lyer illusion, *Psychol. Rev. Mon. Suppl.*, 7, 1905, 55-81; E. O. Lewis, The effect of practice on the perception of the Müller-Lyer illusion, *Brit. J. Psychol.*, 2, 1908, 294-306; C. E. Seashore *et al.*, The effect of practice on normal illusions, *Psychol. Rev. Mon. Suppl.*, 9, 1908, 103-148; Köhler and Fishback, The destruction of the Müller-Lyer illusion in repeated trials: I. An examination of two theories, *J. Exper. Psychol.*, 40, 1950, 267-281; see also Köhler and Fishback, footnote 4.

<sup>15</sup> See Köhler and Fishback, footnotes 4 and 14.



figure can be found by plotting the difference between the two obtained curves. Another method would be to measure only the Müller-Lyer illusion, but each point on the curve would be the average of a number of measurements. Half of these measurements would be made with the illusion-figure in one orientation, while the other half would be made with the figure rotated 180°. This method would likewise eliminate *HMDs* from each point on the curve, thus eliminating any change in *HMDs* which may occur with repeated testing. In addition, this method should prevent any possibility of the development of differential satiation by the Müller-Lyer figure. Thus an experiment using this method should provide a crucial test of the hypothesis of Köhler and Fishback regarding the cause of practice effects in the Müller-Lyer illusion. If a practice effect is still found, it must be due to something other than either differential development of satiation by the Müller-Lyer figure, or changes which occur in *HMDs* with repeated trials. If no practice effect is found, its absence must be due to the elimination of one or both of these factors. In the latter case, a second experiment should be performed using the first methodology mentioned above. This method eliminates changes in *HMDs* which occur with repeated trials, but it does not eliminate differential development of satiation by the Müller-Lyer figure. The results of the two experiments would then show which factor causes the practice effect in the Müller-Lyer illusion, or whether both factors contribute to the effect.

It is not believed likely that the practice effect with the Müller-Lyer illusion can be reduced entirely to changes which occur in *HMDs*. This belief is based primarily on the fact that the Müller-Lyer illusion always tends to become smaller at the beginning of testing, if any change occurs, whereas *HMD* sometimes become smaller and sometimes larger. There are many similarities, however, between the changes which take place with the Müller-Lyer illusion and *HMDs*. In both cases the changes are often pronounced and trendlike, but some *Ss* do not show any change. Furthermore, in those *Ss* where the constant error has been reduced to zero, the trend sometimes continues and becomes an increasingly large constant error in the opposite direction. Still another point is that both kinds of trends sometimes continue in the same direction in the absence of specific practice. This can be demonstrated, once a trend has developed, by ceasing measurements for a time and then retesting. All these findings for the Müller-Lyer illusion have been established and discussed in the studies of Köhler and Fishback, while the findings for *HMDs* have been established in the author's recent study. Since changes in *HMDs* are sometimes quite marked, it seems certain that the results of practice with the Müller-Lyer illusion have been influ-



enced by changes in *HMDs*. Since there are so many similarities between the two phenomena, it also seems likely that some characteristics of changes in the Müller-Lyer illusion are caused by factors which are not specific to the illusion figure. In other words, some characteristics of changes in the Müller-Lyer illusion may not be due to the shape of the figure, but to unknown factors which cause apparent size relations between opposite halves of the visual field to change with any figure. Thus it is necessary to eliminate changes in *HMDs* when studying practice effects due to the shape of the Müller-Lyer figure.

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### THE ELASTIC EFFECT: AN OPTICAL ILLUSION OF EXPANSION

During a recent automobile trip, the last part of which was made after midnight on U. S. Route 1 between Richmond, Va., and Washington, D.C., I noticed the illusion described here. As I was a passenger in the rear seat, I did not have the care of driving nor any other claims on my attention; topics of conversation had by then run low and the highway was dark except for the part under the car's lights. Since the sway of the rapidly driven car made sleep impossible there was nothing for me to do but to look down the highway.

The broken lines, about 25 ft. in length, spaced at similar intervals, which marked the lanes of the divided highway, were freshly painted white and contrasted markedly, under the illumination of the car's head lights, with the newly black-topped road. Since I sat in the right rear seat, I had a clear and unobstructed view of these lines between the heads of the two occupants of the front seat. Suddenly, as I was idly looking at the intruding broken lines, I observed that they seemed to grow in length as they came toward and vanished under the hood of the car. The growth was slow when a line was observed far down the highway but it increased rapidly as the line neared the car. The expansion was greatest just before a line vanished under the car. It was greater at the near than the far end of the line, particularly just before it passed from view, when the near end seemed to spring or to stretch forward suddenly to meet the oncoming car.

I 'played' with the illusion for some time, observing the expansion at this and at that point and comparing it at different points along the intruding path. Finally the following questions occurred to me: Why had I not seen the illusion before? Why, after so many years of traveling by automobile, was I now seeing it? Why had others not seen and reported it?



Why, since it seemed to me to be compulsory, was it not common-place?

Perhaps, after all, I was only imagining it, or it might be due to the peculiarities of my defective vision. To satisfy myself upon these points, I called my traveling companions' attention to the phenomenon. All of them saw it immediately although none had ever before seen it. Since then I have brought the illusion to the attention of many people. Everyone without exception has seen it as soon as his attention is directed to it. Only one of the many to whom I mentioned the illusion reported that he had observed it before.

The illusion is compelling once it has been observed. It occurs during the day as well as during the night; with orange, yellow, and tawny division lines as well as with white; with other lines and even objects on or along the highway as well as with the division lines; at slow as well as at rapid driving speeds; and with short as well as with long broken lines. The illusion, particularly the expansive snap at the end just before the line passes under the car, is more striking at rapid speeds than at slow and it is less pronounced with very short division markers—*e.g.* 6-in. lines placed at intervals of 50 ft.—than with those of medium (30–40 ft.) length.

I have no answer to the questions asked above. Perhaps, however, since many illusions are not observed until attention is drawn to them, it may be that the peculiar collocation of events necessary for the observation of the illusion described here has seldom occurred. When riding in an automobile, the driver is, or should be, attending to the road. He is not in a position to note the phenomenon. His passengers have many more insistent and worthy objects of attention than the passing division lines, *e.g.* the passing scene, the topics of conversation, and their own thoughts. They should not be expected to note the illusion under those conditions. It is only when the passing scene is blanked out by darkness, when conversation is stilled, when one's thoughts are a vacuum, and sleep, which would ordinarily follow, is impossible, that the highway division lines would become objects of attention—and even then the illusion would be noted only if the observer were psychologically sophisticated and had the insight to check his observations with others and also the desire and initiative to report his observations when he found that they were authentic.

The explanation of the illusion is simple, being, as I believe, principally a matter of visual angle. The angle subtended by the line when it is seen at eye-level far down the road is small, hence the line appears short. As the line comes nearer to the car, it falls further and further below the eye-level, the visual angle steadily increases, and the line appears proportionately longer. There are doubtless other contributing factors which research alone



may reveal, but, in the main, changes in visual angle will prove, as I now believe, to be the basic factor.

I have chosen to call this phenomenon an illusion of optical expansion because it seems to be akin to the optical patterns of expansion described by Gibson in his phenomenological discussion of motion in the visual field.\* The illusion described here is concerned with the linear expansion of a line—of an object—rather than the motion of a point but it is sufficiently like the phenomena Gibson discusses to be related to them.

KARL M. DALLENBACH

### A DEMONSTRATION OF 'SUBJECTIVE' COLORS ON TELEVISION

A course in elementary psychology, recently given by the writer over television, offered an opportunity to devise demonstrations that could be presented on this new medium of instruction.<sup>1</sup> Among the demonstrations successfully attempted was Fechner's 'subjective' colors.

Since colors are perceived from slowly rotating black and white disks, Fechner's phenomenon should, as it was thought, be observed on the black and white TV-screen. The disk recently described by Hess,<sup>2</sup> and several from among those described by Erb and Dallenbach<sup>3</sup> were, therefore, projected to test the conjecture.

The projected image, which appeared on the monitoring sets in the studio, was examined by all of the personnel present. Some saw color immediately, others saw it after a short interval, but some were never able to see it.

When the TV-camera is focused upon a disk rotating clockwise slowly, the movement seen on the TV-screen is clockwise and the colors are saturated, the inner circle (on the disk described by Hess) being red, the outer, blue. When the disk is rotated more and more rapidly—still in a clockwise direction—the movement on the TV-screen reverses; it becomes counter-clockwise. Though the colors are still saturated, the inner circle is now blue and the outer red.

The demonstration has been repeated twice: once before a group of about 100 people so seated that they could see one of six monitoring screens and a second time over an open telecast. Though no formal check was made

\* J. J. Gibson, *The Perception of the Visual World*, 1950, 121.

<sup>1</sup> E. L. Stromberg, College credit for television home study, *Amer. Psychol.* 7, 1952, 507-509.

<sup>2</sup> E. H. Hess, A practical demonstration of subjective colors, this JOURNAL, 63, 1950, 259.

<sup>3</sup> M. B. Erb and K. M. Dallenbach, 'Subjective' colors from line patterns, this JOURNAL, 52, 1939, 229, esp. disks h. j, and k.



either time of the number who saw color, nor of the colors seen, it was evident from the comments received that the colors seen tended to follow Helmholtz's description,<sup>4</sup> viz. the reds tended to vary between reddish yellow and reddish brown and the blues to be bluish green—probably because of the reduced illumination of the projected image.

Western Reserve University

ELEROY L. STROMBERG

### THIRTY-THIRD ANNUAL MEETING OF THE WESTERN PSYCHOLOGICAL ASSOCIATION

The Western Psychological Association held its thirty-third annual meeting Thursday through Saturday, June 18-20, 1953, in the new Health Sciences Building of the University of Washington at Seattle. Eighty papers were given and several conferences were held during the three-day period. Total registration was 253.

At the annual banquet Friday evening the presidential address, "Virtue Rewarded and Vice Punished," was read by Edward C. Tolman since the president, Ruth Tolman was absent because of illness. The paper argued for harmony or coöperation between the 'pure' scientist and the applied, or the theoretician and the professional practitioner in psychology.

Special meetings held in conjunction with the meeting were: a luncheon for members of Psi Chi, sponsored by the University of Washington chapter; a luncheon for members of the Society of Projective Techniques, Bruno Klopfer presiding; a get-acquainted luncheon for school psychologists, sponsored by members of APA Division 16; a regional meeting of the Division of Clinical and Abnormal Psychology, arranged by Kate Kogan and William Grove. On Friday afternoon members of the Association and guests were entertained at a tea given by Dean and Mrs. Edwin R. Guthrie.

Local arrangements were under the direction of Allan Katcher of the University of Washington. Carl Dickinson assisted by providing a placement service. The Program Committee was made up of Clare Thompson, VA Hospital, Palo Alto; C. W. Telford, San Jose State College; Arthur Coladarci, Stanford; Arthur Burton, Agnew State Hospital; Leo Postman, University of California, Berkeley, and the Secretary.

The following officers were elected for 1953-54: President, Nancy Bayley, University of California, Berkeley; President-elect, Neil Warren, University of Southern California; Secretary, Leona Tyler, University of Oregon; Treasurer, George Horton, University of Washington.

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<sup>4</sup> Herman von Helmholtz, *Treatise on Physiological Optics*, Eng. trans., ed. by J. P. Southall, 2, 1924, 255-261.



The 1954 annual meeting will be held on May 20-22, at Long Beach under the joint sponsorship of the University of California at Los Angeles, the University of Southern California, Occidental College, and Long Beach State College.

San Jose State College

RICHARD KILBY

### CHANGES IN THE DATES OF PUBLICATION

Beginning with the next volume of the JOURNAL (1954, Volume 67), the dates of publication of the separate quarterly issues will be on the 15th of March, June, September, and December. This change is necessary because the JOURNAL cannot, under present conditions, as the past few years have shown, meet its present schedule. The new one, however, can and will be met.

This marks the first change in the JOURNAL's month of publication since 1895 (Volume 7)—though the separate issues were renumbered in 1902 (Volume 13) to coincide with the calendar year. It is, therefore, with some regret that this break with the past is made. The advantages of the change, *i.e.* prompt publication, outweigh, however, tradition. If the new dates of publication prove, after a fair trial, to be unsatisfactory to the JOURNAL's contributors and subscribers, further changes will be made and with less hesitation now that tradition has been broken.

K. M. D.

### ERRATUM

Lines 10, 11, and 12 in the July 1953 issue of this JOURNAL (Vol. 66, page 417) should read "Since the best estimation of the true-score-equivalent of what this *S* thinks is one full turn is 22.45,  $1.12 \times 22.45 = 25.14$  is the predicted true-score for the subsequent trial."

K. M. D.

### David Katz: 1884-1953

There are scientists who, early in their careers, find the theme of their work in some basic ideas to whose systematic development and application they devote a lifetime of effort. Others are explorers, guided by a catholic curiosity and challenged by the many questions, observations, and ideas that cross their paths. David Katz, who was born on October 1, 1884, and died on February 2, 1953, in Stockholm, was an explorer. Theories did not act for him as impulses; they served him to explain the phenomena that had captured his imagination. In one of his last writings, an introductory chapter to a *Handbuch der Psychologie*, he made the following char-



acteristic assertion: "It can hardly be expected that theoretical psychology will ever attain the importance in relation to its experimental sister which theoretical physics has in relation to experimental physics, for the reason that psychical happening cannot be subsumed under so small a number of principles as can physics."<sup>1</sup>

The range of the topics that attracted his attention was large indeed. His readiness to respond to every opportunity or request became most evident when the advent of the Nazi-regime forced the forty-nine-year-old professor to give up the security of his homeland and work out a new existence. At that time, Katz had for fourteen years comfortably occupied a newly established chair of psychology and education at the State University of Mecklenburg in Rostock. Once before, world events had interrupted his academic routine. During the First World War he had served with a sound-ranging crew and acquainted himself with the techniques of auditory localization, which he later applied to animal experiments.<sup>2</sup> In 1918 he had been called to study the psychological aspects of amputation and thus collected the data for his monograph on the "phantom limb."<sup>3</sup> Food shortage during and after the war had given the incentive to a book on hunger and appetite,<sup>4</sup> in which he developed his thoughts on motivation and demonstrated the effect of vegetarian and meat diets on school children by means of Galton's composite photographs.

After his departure from Germany, new environments provided new targets. In a recently written autobiographic sketch,<sup>5</sup> he reported that at T. H. Pear's laboratory in Manchester he undertook research on "the tongue as a primitive sense-organ" and that in 1935 he used the facilities of the London Zoological Gardens, offered to him by Julian Huxley, to work on the feeding habits of monkeys and on their behavior under poor lighting conditions. At the request of the British Research Association of Flour Millers he studied the effect of the gluten content of flour on touch sensations and also tried to find out why bread consumption had declined in England. Later he lectured on massage at the invitation of the Chartered Society of Massage and Medical Gymnastics.

There was, however, method in the variety of Katz's interests. As one looks through the record of his life-work, certain subjects and principles are found to recur. For instance, perceptual constancy, to which he devoted many experiments in his classical investigation of color, is referred to in

<sup>1</sup> *Op. cit.*, 1951, 55.

<sup>2</sup> *Animals and Men*, 1937, chap. 5.

<sup>3</sup> *Zur Psychologie des Amputierten und seiner Prothese*, 1921, 1-119.

<sup>4</sup> *Hunger und Appetit: Untersuchungen zur medizinischen Psychologie*, 1932, 1-70.

<sup>5</sup> *History of Psychology in Autobiography*, IV, 1952, 189-211.



his early study on children's drawings<sup>6</sup> and also played a rôle in his weight-lifting experiments with amputees. His concern with kinesthesia led to his monograph on touch<sup>7</sup> and publications on the vibratory sense<sup>8</sup> and the musical perception in the deaf,<sup>9</sup> the same interest reverberates in the work on bread and massage and in measurements of handwriting by means of his "scriptochronograph."<sup>10</sup>

At the time when Katz started out on his career, psychology was still in the process of emancipating itself from philosophy. The field of perception was partly occupied by the physiologists. There was not a single chair of psychology in Germany. Men like Katz's teacher, G. E. Müller, taught psychology in the disguise of philosophers. Interest in the new discipline was aroused in Katz, a young science student, by Baumann's lectures on the immortality of the soul, and he conceived his work on color in rebellion against Müller's almost exclusively physiological treatment of visual sensations. Encouragement to deal with perceptual experiences as legitimate facts of science came from the phenomenology of the philosopher Husserl. Katz's careful observations and descriptions of 'film color,' 'surface color,' transparency and illumination bear the stamp of Husserl's teachings.

Spadework had still to be done in almost every area. One is reminded of the relative youthfulness of ideas now taken for granted by undergraduates when one reads in *Hunger und Appetit* that as late as 1932 Katz had to free himself from a purely physiological approach to motivation by means of a "Two-Components-Theory," which asserted the importance of field conditions. "By fitting the state of hunger into a network of functional relations the main problem is made to appear much more intricate but also much more attractive than it was taken to be by the physiologist who thought he had to consider nothing but the (objective) state of the hungering organism."

The decisive development in Katz's thinking is reflected on the title page of his work on color. In the German edition of 1930 one reads under the main title, *The Structure of the World of Color*: "Second, completely revised edition of: 'The Modes of Appearance of Color and Their Modification by Individual Experience.'"<sup>11</sup> In a passage not contained in the

<sup>6</sup> Ein Beitrag zur Kenntnis der Kinderzeichnungen, *Zsch. Psychol.*, 41, 1906, 241-256.

<sup>7</sup> Der Aufbau der Tastwelt, *Zsch. Psychol.*, Ergbd. 11, 1925.

<sup>8</sup> Ueber den Vibrationssinn, 1927, 1-278.

<sup>9</sup> With G. Révész, Musikgenuss bei Gehörlosen, *Zsch. Psychol.*, 99, 1926, 289-324.

<sup>10</sup> Der Skriptochronograph, *Miscellanea psychologica Albert Michotte*, 1947, 31-38, 634-636; also *Quart. J. Exper. Psychol.*, 1, 1948, 53-56.

<sup>11</sup> Der Aufbau der Farbwelt, 1930, 2nd ed. of *Die Erscheinungsweisen der Farben und ihre Beeinflussung durch die individuelle Erfahrung*, *Zsch. Psychol.*, Ergbd. 7, 1911, 1-425.



abridged English translation<sup>12</sup> Katz stated that, since the publication of the first edition in 1911, he had come to lay less stress on individual experience and to emphasize nativistic factors more strongly. "The beginnings of this change are contained already in the first edition to a considerable degree, but twenty years ago the entire approach of research was still largely under the spell of empiricist interpretation. Even Hering, otherwise the celebrated pioneer of nativism, trod the empiricist path when it came to explaining the phenomena of color constancy. It was not simple for a younger author to withdraw from this influence at once and entirely."<sup>13</sup>

Aided by the perceptive writings of Adhémar Gelb, Katz showed the dependence of constancy upon the configuration of the total visual field. This was his most conspicuous but not his first move toward gestalt psychology. He felt that already in his work on the sense of touch he had broken away completely from atomistic views. Later, his "avidity theory" of appetite dealt with the process of satiation as "an instance of self-regulation in the organism, directly resulting from its inner and outer physico-chemical conditions."<sup>14</sup> Even so, he never belonged to the inner circle of the Berlin school. He sympathized with gestalt principles, used some of them, was suspicious of others, doubted that the theory could be applied universally, and sometimes misunderstood it, as, for instance, when he wondered how individual differences could ever be accounted for in gestalt terms, or when he took the law of Prägnanz to imply purposefulness. In any case, from the time he assumed a professorship at the University of Stockholm in 1937, he and his students tended to experiment along gestalt lines. His book *Gestalt Psychology* (1950), first published in Swedish in 1942, gives an elementary introduction to preëmigration gestalt work and reports on some of his own more recent contributions, such as his "gestalt law of mental work," according to which "in any given type of task, the details of the work process—such as its progress and reliability—are determined by the nature of the total task of which they are parts."<sup>15</sup>

Color and touch—experiences related to emotional warmth and direct contact with reality—are the fitting symbols of the man and psychologist David Katz. Profiting from the early freedom of the young science, he tried his hand in many fields. In writing and talking about psychology he still had the enviable privilege of spicing research reports with anecdotes, reminiscences, chance observations. In his experiments he himself was his

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<sup>12</sup> *The World of Colour*, 1935, 1-300.

<sup>13</sup> *Ibid.*, 453.

<sup>14</sup> *Hunger und Appetit*, 1932, 56.

<sup>15</sup> *Op. cit.*, 105.



best subject. While pursuing his studies, some of which have now found their place in many textbooks, he never enjoyed the advantages of a well-equipped laboratory, a lack, which forced him, as he put it, "to make a virtue out of necessity."

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RUDOLPH ARNHEIM

### **Gustav Kafka: 1883-1953**

Gustav Kafka was born in Vienna on July 23, 1883, and there attended elementary school and later the school organized by the Schotten monks.<sup>1</sup> He learned French and English at home and as a child became fully conversant with both languages. In 1902 he entered the University of Vienna, where he studied law for one semester and then shifted his studies to philosophy and psychology. After a semester at G. E. Müller's laboratory in Göttingen, where he became acquainted with Géza Révész and David Katz, Kafka matriculated at Leipzig where in 1904 he received the doctor's degree from Wundt for a thesis entitled *Ueber das Ansteigen der Tonerregung*. In 1905 he went to Munich to continue his studies under Theodor Lipps. Later he worked there under Erich Becher and was appointed professor at Munich in 1915. He took part in the First World War as an Austrian reserve officer. Towards the end of that war, he and his friend Géza Révész, then at the University of Budapest, were commissioned to set up a psychotechnical service for the Austro-Hungarian Army. In 1923 Kafka succeeded Karl Bühler as professor of psychology, philosophy, and pedagogy at the Technische Hochschule in Dresden, but in 1935 political difficulties and ill health combined to force him to resign prematurely. Just before its close, the Second World War added to his misfortunes by the destruction of his home and all his property in an air raid.

The collapse of the war led not to his academic reinstatement but at first to hunger and dire distress. In 1947, however, he received an appointment as professor of philosophy and psychology at the University of Würzburg, where he continued to work until his second and final retirement in the summer of 1952. In his seventieth year, on February 12, 1953, death overtook him suddenly in his newly acquired home in Veitshochheim near Würzburg.

All his life the principal part of Kafka's interest and working power was concerned with the history of philosophy, ancient philosophy, systematic

<sup>1</sup> This note was translated into English from the German by Dorothy Cohen of the Harvard Psychological Laboratories and was then adjusted as to idiom by the Editor.



philosophy, and especially natural philosophy, epistemology and ethics. He achieved a reputation in these fields through a series of monographs which he either wrote or edited. Eventually he learned how to promote psychology as the real concern of his life. He introduced the doctrine of the phases of developmental psychology into the history of philosophy.<sup>2</sup> As logician he concerned himself with the clarification of the concept of type<sup>3</sup> and with the problem of the *Verstehens*.<sup>4</sup> He dealt with the methodological problems and goals of psychology with the breadth of a genuine philosophical orientation, as is evident in his addresses at the Amsterdam Congress of 1948<sup>5</sup> and, shortly before his death, at the Munich Congress in 1949.<sup>6</sup> In concrete investigation Kafka was concerned all his life with comparative, developmental and cultural psychology, taken in their broadest senses. His three-volume work<sup>7</sup> extended his treatment far beyond the confines of animal psychology, which he had treated earlier in another book (1914), going beyond the psychology of primitives, children, language, religion, art, society and vocations, to the borders of psychopathology, criminal psychology, dream psychology and sexual psychology. From this, his chosen field, Kafka recently isolated the problem of races, treating the topic in still another book.<sup>8</sup>

Outstanding among his many papers are those which show Kafka as a critic of fundamentals and methods, e.g. his criticism of behaviorism,<sup>9</sup> the 'world' and the 'surrounding world,'<sup>10</sup> the basic problems of the psychology of expression,<sup>11</sup> the concept of play,<sup>12</sup> psychological counseling and psychotherapy,<sup>13</sup> and those other articles in which he searchingly revealed and developed new points of view on concrete investigation. His most

<sup>2</sup> See especially Gustav Kafka, *Geschichtsphilosophie der Philosophiegeschichte*, 1933, 1-66.

<sup>3</sup> Zur Revision des Typusbegriffes, *Zsch. Psychol.*, 144, 1938, 109-133.

<sup>4</sup> "Verstehende Psychologie" und Psychologie des Verstehens, *Arch. ges. Psychol.*, 65, 1928, 7-40.

<sup>5</sup> Die metaphysischen Voraussetzungen der Psychologie, *Proc. X. International. Congr. Philos.*, 1948, 918-921.

<sup>6</sup> Forschungsaufgaben der Psychologie in der Gegenwart, *Philosophische Jahrbuch*, 1952.

<sup>7</sup> *Handbuch der vergleichenden Psychologie*, 1922, 3 vols.

<sup>8</sup> *Was sind Rassen?*, 1949.

<sup>9</sup> Die Bedeutung des Behaviorismus für die vergleichende Psychologie und Biologie, *Ber. XII. Kongr. deutsch. Gesellsch. Psychol.*, 1932, 213-255.

<sup>10</sup> The change in the concepts of "world" and "surrounding world," *J. Gen. Psychol.*, 14, 1936, 438-460.

<sup>11</sup> Grundsätzliches zur Ausdruckspsychologie, *Acta Psychol.*, 3, 1937, 273-314.

<sup>12</sup> Anmerkungen zum Begriff des Spieles in Anschluss an J. Huizingas "Homo ludens," *Zsch. Psychol.*, 154, 1942, 287-318.

<sup>13</sup> Psychagogik und Psychotherapie, *Acta Psychol.*, 8, 1951, 25-34.



recent researches include two original contributions.<sup>14</sup>

Originality, wit, sagacity, a sense of the problem, and sensitivity are manifest in all of Kafka's work. He was a rare teacher. Human warmth and kindness marked his behavior and defined his relations to his students and his colleagues, despite his tendency to display also the 'basic emotions,' a tendency of which he himself was ironically aware. Indeed irony, self-irony and humor were among his prominent characteristics.

His bequest as a psychologist was contained in the already mentioned Munich lecture on investigations.<sup>15</sup> Here we read that his psychology, which he would raise above the level of mere accumulation of facts, can not dismiss the question of the nature of the soul; that the neglect of this assumption must bear the major blame for the confusion in contemporary psychology and for the inconclusiveness of attempts to systematize psychology; that three false assumptions inherited from the English philosophy of enlightenment must be corrected; namely, the pure passivity of soul, the original isolation of phenomena, and the universal validity of the laws of casuality; and that Gestalt theory can never be wholly satisfactory because of its restriction to the concepts of perception and ideation. Finally Kafka opposed to the catchword 'depth psychology' (which he had already criticized), a 'psychology of height' (Höhenpsychologie), that is to say, a psychology of the spirit, a psychology which he considered both possible and necessary in opposition to the dying demands of psychologism. Insofar as Kafka was representative of a movement that one can identify with a phrase, it could be said that he stood for a psychology of structure, founded on philosophy, a universal psychology of the soul and spirit in which the notion of totality is obvious.

Only a short time before his death Kafka and V. E. von Gebattel founded a *Jahrbuch für Psychologie und Psychotherapie* with a program for the 'psychology of height.' For some time he had been co-editor of Révész's *Acta Psychologica*. In 1948, having previously been secretary of the Deutsche Gesellschaft für Psychologie, he undertook the reestablishment of that Gesellschaft and founded a section in the American Zone of occupation. From August 1951 to his death he was president of the revived society. He was a member of the Bayerische Akademie der Wissenschaften and not long before his death he became dean of its faculty.

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<sup>14</sup> Ueber das Erlebnis des Lebensalters, *ibid.*, 6, 1949, 178-189; Ueber Uraffekte, *ibid.*, 7, 1950, 256-278.

<sup>15</sup> See footnote 6.



**Karl Marbe: 1869-1953**

On January 2, 1953, Karl Marbe, Professor Emeritus of Philosophy (which includes psychology) in the University of Würzburg, died at the age of eighty-three years. Although he often had pleaded for the separation of psychology from philosophy in German universities, he himself wanted to be philosopher as well as psychologist. He was the author of several books and papers on philosophical questions. His chief concern was the problems of probability and of *Uniformity in the World*, the title he gave a two-volume work. He held that uniformities in nature, in both mentality and culture, cause certain discrepancies between empirical findings and the probability calculus. These and related statistical questions kept him busy up to the end of his life. He also thought that unexpected mental or behavioral uniformities lie at the root of various so-called parapsychological phenomena. Marbe's philosophy was strictly empiristic. Fact-finding, he believed, claims primacy over theorizing—in psychology no less than in any other science.

Marbe was born to German parents in Paris on August 31, 1869. He studied psychology with Münsterberg, Martius, Ebbinghaus, and Wundt and took the degree of doctor of philosophy at the University of Bonn in 1893. After having worked at Bonn and with Binet in Paris, he went to Külpe who had just been appointed professor at Würzburg, where Marbe became *Privatdozent* and, later on, associate professor. In 1905 he took over the chair of philosophy and psychology at the Academy of Social and Commercial Sciences at Frankfurt, the forerunner of the University. In 1909 he returned to Würzburg as Külpe's successor. There he worked until 1934 when the Nazis compelled him to retire. He had always hated nationalism in any form. From 1926 on he also lectured at the *Handels-Hochschule* at Nuremberg.

Marbe published 18 books and more than 100 articles. From his laboratories came about 180 books and papers, written by his assistants, lecturers, students and other co-workers. Of a periodical, *Fortschritte der Psychologie und ihrer Anwendungen*, founded by him in 1913, five volumes were issued.

His research started with classical problems of experimental psychology, with relations of stimuli and sensations, with the fusion of successive visual and auditory stimuli, with stroboscopy and with fluctuations of sensations near the stimulus-threshold. An apparatus constructed by him for varying colored sectors of revolving disks during rotation is still in general use.



Towards the beginning of this century Marbe turned to new problems. Speech and language and the problem of thinking came into the foreground. Together with the linguist Thumb, Marbe tried to explain the origin of certain verbal expressions by the results of word-association tests. They were the first to distinguish unusual, individual word-responses to stimulus-words from responses common to many people, and to measure the degree of communality. Kent and Rosanoff later on developed the same technique on a larger scale, pointing to the diagnostic value of individual responses, whereas Thumb and Marbe emphasized the linguistic importance of the common responses. They also found a close relation between the degree of communality and the time taken by the response.

With a surprisingly simple technique Marbe investigated the rhythm of prose language in works of fiction and in letter writing and its bearing on esthetics. To study the melody of speech he developed his 'soot method' (*Russmethode*), using flames that are set in vibration by sounds and recording their vibrations by smoke rings on moving paper. This method also was used for recording the tones produced by heart activity and for measuring reaction-time. When, however, new devices using photographic recording and oscillographs were developed, Marbe's excellent method was displaced.

Marbe's work on judgment (1901) was the first contribution to the psychology of thinking from the Würzburg School, research employing the method of systematic experimental introspection. He did not find any psychological characteristic applying to all judgments and concluded, therefore, that judgment is not a psychological but a logical concept. It may be that this result was due to the easy, even trivial problems of judgment given the subjects for solution or perhaps to the as yet crude state of introspection. Nonetheless the method helped Marbe to reveal those conscious states which he called *Bewusstseinslagen*, sometimes, but misleadingly, translated as "mental sets." Oddly enough, the relation of these *Bewusstseinslagen* to William James' thoughts or feelings of relation and thoughts or feelings of tendency was never noticed.

Marbe wanted the new method to be developed towards greater accuracy and measurability. What Külpe and his co-workers contributed to the psychology of thinking seemed to Marbe to run contrary to his own intentions. Thus he withdrew and abandoned experimental introspection. Years later, in 1915, he was led to a vigorous outburst against Külpe in *Fortschritte der Psychologie*.

As a psychologist who placed fact-finding before everything else, Marbe



could not be enthusiastic about the turn that psychology in Germany had taken after 1910. Endless discussions on *Gestalt* and *Ganzheit*, on *verstehende* and *geisteswissenschaftliche* psychology seemed to him to impair the effectiveness of psychologists, and his fear that psychology might fall back into an era of bare speculation was not unfounded. Marbe reacted to the new situation by ignoring it and by adding applied psychology as a new field of work to his previous ones. He continued research on mental uniformity and on such parapsychological phenomena as thought-transfer and dowsing. In addition he dealt with aptitude tests in general, with tests for drivers, surgeons, and dentists and with the psychological aspects of insurance, railway management, accidents, and advertising. His studies on accidents resulted in disclosing the accident-prone type. Marbe gained great reputation by his work on legal or forensic psychology and by his activity as psychological expert in law courts. In dealing with applied psychology he realized the importance of individual attitudes (*Einstellung*) and individual differences in shifting attitudes (*Umstellung*). This work brought him back to general psychology and on to the problems of personality.

Marbe himself twice has given account of his life and work, once in Murchison's *A History of Psychology in Autobiography*, III (1936) and later in Abderhalden's *Selbstbiographien von Naturforschern* (Leopoldinisch-Karolinische Akademie der Naturforscher, 1945).

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### Richard Maria Pauli: 1886-1951

Richard M. Pauli was born May 12, 1886, and died March 22, 1951. His way to psychology was typical of that of many of the famous German and American psychologists in the beginning of the twentieth century. He set out as a student of philosophy, with the Kantian, Liebmann, as his teacher. Soon he had become an assistant to Külpe, who had left Wundt's laboratories and had gone to Würzburg. Pauli's years in Würzburg were marked by his work and friendship with the physiologist, Max von Frey, known for the exact methods which he introduced into the measurement of touch. Thereafter Pauli's own work was characterized by his avid desire for the exact measurement and the application of mathematical methods to anything that can be measured in psychology. This attitude was balanced by Pauli's religious tendency to do reverence to the unfathomable, but never did he let his religion limit him in approaching as far as possible toward the limits of exact knowledge.



Pauli was a born pedagog. As a student, he became aware of the lack of a standardized set of experiments designed to give the beginner the feel of the standard instruments, as well as the general idea of the nature of psychological problems and methods. In 1919 his psychological 'practicum' was published by Gustav Fischer in Jena. It has run through many editions, has increased in content and size, and has been used in many German psychological institutes.

The fruits of his exact work are manifold. In 1913 he built the *Reizhebelapparat*, a device providing for the exact simultaneity of two or more stimuli. With this method, he and his pupil, Alois Mager, could definitely solve the 2000-yr. old psychological problem of true simultaneity of disparate percepts. The answer was that there is no such simultaneity, and that the story of Caesar dictating three letters at the same time is not psychologically true. Notwithstanding all the work that had been done by Wirth in Wundt's laboratory on the range of consciousness, this was perfectly new ground.

Pauli's theoretical papers dealt with a new biological interpretation and demonstration of the Weber-Fechner law. He based his generalizations on his own and his many pupils' investigations of taste, especially the taste of sweetness.

His greatest satisfaction was that some of his pupils continued to show the same love of exactitude. Among many others, we may mention Heinrich Schriever, at present Director of the Physiological Institute of the University of Mainz. Schriever's papers on algosimetry opened up new ranges of the Weber-Fechner law.

Pauli recognized no other differences between his pupils than talent, conscientiousness, and seriousness of purpose. This was the reason why his outward success was curtailed, once the National Socialists took over in Germany. He had never been willing to submit to the theories, the fashions of the times, and was, therefore, considered politically suspect. Persecutions could not bend, however, a man so strong in his religious feelings.

In his last years he turned to a new application of exact measuring methods. He became an adherent of graphology, which he built up as natural science, comparing graphology, particularly the graphology of the handwriting of figures, with the results of the Kraepelin-Pauli graph of work. An enormous material was piled up, the evaluation of which is delayed by Pauli's untimely death.

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**Martin Luther Reymert: 1883-1953**

Martin Luther Reymert, Director of the Mooseheart Laboratory for Child Research, was born in Holmestrand, Norway, November 10, 1883. He graduated from the Oslo Gymnasium in 1903, from the Army Officers School in 1904, and from the State Normal School in 1905. In 1906 he passed the *Examen Philosophicum* at the University of Oslo. During the ten year period 1906-16 he engaged in graduate study and teaching in Norway except for the years he had to devote to military service. He came to the United States in 1916 as a Fellow of the American Scandinavian Foundation and took graduate work in psychology at Clark University, where he received the Ph.D. degree in 1917.

Reymert was an Honorary Fellow of the American-Scandinavian Foundation and a Fellow in Psychology at the University of Iowa in 1918-19, and in 1919-20 he was a Research Associate at the Iowa Child Welfare Research Station. After this appointment he went back to Norway and was made Assistant Professor of Experimental Psychology at the University of Oslo and Lecturer on Educational Psychology at the State Agricultural College of Norway. He returned to the United States in 1925 and was appointed head of the Department of Psychology of Wittenberg College and Director of the new psychological laboratory which he was principally instrumental in establishing.

In 1930 Reymert went to Mooseheart, Illinois, an institution for children run by the Loyal Order of the Moose. There he established the Mooseheart Laboratory for Child Research and remained its Director until his death. The other institution owned and operated by the Loyal Order of the Moose is Moosehaven, in Orange Park, Florida, a model community for old people. Here in 1949 he established the Moosehaven Research Laboratory, with Dr. R. W. Kleemeier as Director. In 1951 he was appointed Director of Research for the Loyal Order of Moose. This Directorship covered both the Mooseheart Laboratory for Child Research and the Moosehaven Research Laboratory. In the former laboratory he and his staff periodically tested all of the children of the Institution and advised them regarding their studies and their various problems of adjustment. The results of their work have been of great value to the officers of Mooseheart. At Moosehaven research has been undertaken on the numerous phases of aging.

Reymert's interest in child welfare did not stop at the gate of Mooseheart. He was a member of the advisory board of St. Charles (Illinois) School for Boys, the Illinois Division for Youth and Community Service, and the Big Brothers and Sisters Association of Illinois.



The *Scandinavian Scientific Review*, a quarterly journal published in English, was started by Reymert in 1921, and he was Editor-in-Chief until 1926. While at the University of Oslo during the period 1920-25, he standardized group and individual intelligence tests for Norway. He was also Chairman of the Symposium on Mental Tests at the Ninth International Congress of Psychology at Yale in 1929.

Reymert considered his organization of the two Symposia on Feelings and Emotions one of his greatest contributions to psychology. The first Symposium took place at Wittenberg College in 1928 on the occasion of the opening of the new psychological laboratory. The second Symposium was held in 1948 under the auspices of Mooseheart and the University of Chicago. The papers of these Symposia were published in two volumes edited by Reymert.

Dr. Reymert died of carcinoma on June 2, 1953. He was a man of great energy and enthusiasm. He was an excellent organizer and executive as well as a devoted scientist, and his loss will be keenly felt, especially in the field of child welfare.

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HERBERT S. LANGFELD



## BOOK REVIEWS

Edited by M. E. BITTERMAN, University of Texas

*History of American Psychology.* By A. A. ROBACK. New York, Library Publishers, 1952. Pp. xiv, 426.

This book is ostensibly a geopolitical history of psychology since it takes America for its field. That might seem a strange choice when science has become so international, but Roback notes that "France, Germany, Scotland, England and Italy can be proven to have developed psychologists of different casts" (p. 4), and therein lies his justification, even if he does insist with some vehemence that nationality and nativity are not apt to be as important as certain other factors (like Jewishness) in describing a scientist (e.g. p. 296). Actually, however, Roback is forced to go abroad for the roots of American psychology. He praises Münsterberg's experimental work at Freiburg, insists (incorrectly, I think) on the admiration of the British psychologists for McDougall before he emigrated, and devotes as much space to Freud, who was in America only briefly in 1909, as he does to anyone else except James and Münsterberg. That is right. The geopolitical classification can serve only for gross orientation.

A case can be made for there being a peculiarly American psychology, a functional psychology. Those who argue thus have at hand an explanation of why German psychology got changed at the hands of its importers—James, Ladd, Hall and Cattell. They see a community between these men and Dewey and Baldwin, and later Thorndike and Woodworth, and ultimately the behaviorists and the testers; but they leave Titchener out, regarding him as part of the German tradition. Only geographically was Titchener an American. Politically and as a person he was intensely English, but professionally he was German, Roback to the contrary notwithstanding.

Roback's loyalty—for he seems to have no feeling that the historian should try to suppress his value-judgments—is all for mentalism, for those who know that "all of nature is not physical or physicalistic" (p. 333), and that credo leads him to excoriate behaviorists and to applaud James, Münsterberg, McDougall, Titchener and the Gestalt psychologists. Nor does he seem to notice that only one of these heroes of "American" psychology is American born.

There is, moreover, another way in which American psychology fails to provide Roback with a unitary theme. The experimental period, from William James on, simply does not find the sources of its being in pre-experimental American psychology. Roback describes the psychological thought and writings of the two centuries from Samuel Johnson's *Elementa Philosophica* of 1752 right on down to the present. Under his hand the principal characters become clear. In the order of their appearance they are: Samuel Johnson and Jonathan Edwards (influenced by Locke); John Witherspoon and S. S. Smith (presenting the Scottish view of mind); Benjamin Rush (the early medical psychologist of 1812); Burton and Upham (the early textbook writers); Rauch (who introduced German views and was in 1840



first to put the word *Psychology* into a book title); Schmucker, Hickock, Mahan, Wayland, and Haven (who relied upon German science and dominated the scene until James and Dewey entered upon it). Roback's account of this period is clear and informative and we are fortunate to have it in English so easy to read. Jay Wharton Fay has already written this story, but I find Roback's treatment clearer and more sophisticated and we owe him our gratitude. My point is merely that the first quarter of this volume does not make the remainder more intelligible. The ancients of American psychology were not the prophets.

After hearing about the ancients you get to the founders: James, Stanley Hall, Cattell, Baldwin, Titchener and Münsterberg, plus McDougall who arrived from abroad long after the founding was over.

Of these men McDougall with his *horme* is Roback's no. 1. Roback says that McDougall "belongs to the titans in psychology, despite the general belittling attitude toward him on the part of younger men. . . . In the opinion of the writer, McDougall has been and will remain perhaps for a long time yet, the foremost English-speaking psychologist, with no exception" (p. 253). This surprising pronouncement is one of many unexpected value-judgments which this volume contains, judgments which I can neither catalog nor criticize in the space available. In the present instance I content myself with quoting Roback's exact words because I think that nearly every British or American psychologist who was professionally active before 1920 will find them preposterous.

James is Roback's no. 2. McDougall was "the foremost English-speaking psychologist," but James is "the greatest American psychologist" (p. 253). James and Münsterberg get the most space, and Roback presents the paradoxes and contradictions of James with so much mixed praise and apology that one sees why it is James' greatness must always be ascribed in part to his engaging humanity.

I expected Münsterberg to be no. 3, but no. "Titchener stands higher in our estimation than Münsterberg, for whom the lure of glamor meant so much" (p. 198). It is not my judgment, however, that Roback has managed a true assessment of Titchener. Titchener did not, I think, ever dominate American psychology; he withdrew from it because it would not defer to his values. Nor did rivalry between Cornell and Harvard "make scientific history" in psychology "for two decades or more" (p. 180). The main battle was elsewhere. Nor did Titchener admire the American phase of Münsterberg, as the letter which Roback quotes shows, if you read it very carefully (pp. 199f.); he disliked all the phases of the "lure of glamor." Nor was Titchener, as psychologist, British. He modelled his professional life on the German pattern. (For Roback's view on this matter, see his pp. 181-186, 211f., 301; for mine, see various places, including this JOURNAL, 38, 1927, 489-506.)

Thus Münsterberg becomes Roback's no. 4, although the world, Roback says, "in the years immediately preceding World War I, rated Münsterberg as the foremost psychologist of his time, barring Wundt" (p. 200). That again is an *ipse dixit* difficult to accept. That world, which would have known enough about Wundt to rate him first, would not have rated the popularizing Münsterberg second. On the other hand, the Americans who were not psychologists, the nonprofessional world, might easily in these years have put Münsterberg first, without knowing Wundt existed.

Stanley Hall is no. 5 and Cattell is no. 6.



Then come the schools. Functionalism—Dewey, Angell, Judd, with Titchener off-stage complaining. Behaviorism—Mostly Watson, with a flash-back to James Rush whom Roback has discovered as an American behaviorist of 1865. Dynamic psychology—Freud, Morton Prince, the admired McDougall, the somewhat belittled Woodworth, Lewin, Tolman, Troland, who had he lived, "would take his place among the first ten American psychologists" today (p. 274), a little of Gordon Allport, a little of Harry Murray. (The list seems to me to be overweighted with Harvard connections.) Psychoanalysis—Freud and the standard mention of European analysts. Titling the book "American psychology" does not keep it so. Gestalt psychology—a splendid account of the movement, marred by the assertion that it is essentially a Jewish contribution. Operationism—a severe criticism of it by intuitionist Roback. Factorial analysis—the English Spearman, the American Thurstone, the Scottish Thomson. General semantics—Korzybski in solitary state. Thomistic psychology of the Catholic writers—a wise, mature, understanding account (cf. esp. p. 353), which contrasts strangely with Roback's bitternesses in behalf of Jewish influence and against objective psychology in the other parts of the volume.

Now about Roback's contention that Gestalt psychology is a Jewish contribution. This is no new idea with him for he wrote about the matter in 1929 in his large *Jewish Influence in Modern Thought*. In his present volume he inserts a six-page polemic on the importance of saying, when one writes on the history of thought and science, which men are Jews or part-Jews, in order that the contribution of Jews to the culture may not be suppressed. Roback notes how often the nativity and nationality of great men are stated and he asks for justice. I do not think, however, that he makes his case, certainly not in respect of Gestalt psychology.

In the first place, let it be observed that he seeks support in biased selection. He picks out all the Jews he can find in Gestalt psychology and ignores most of the non-Jews. Thus, right at the start he is in trouble, for, of the three recognized representatives of Gestalt psychology, Wertheimer, Koffka and Köhler, we have one and a half Jews (Wertheimer, Koffka/2) and one and a half non-Jews (Köhler and Koffka/2). He brings in von Ehrenfels ("partly Jewish") and Husserl ("a Moravian Jew") and leaves out Hering, who was more important to the Gestalt psychologists than Husserl, and Stumpf who surely had about as much and also about as little influence as von Ehrenfels. The phenomenological tradition in psychology—Goethe, Purkinje, Hering, Stumpf, is not a piece of Jewish culture. The genesis of field theory—Faraday, Maxwell, Planck, Einstein—was not thoroughly Jewish merely because it led to Einstein. No; Roback has missed here the basic principles of the design of experiment and of scientific control. All he says is that the incidence of Jews in Gestalt psychology is greater than their incidence in non-Gestalt psychology. That fact could be explained on the ground that Jews trust one another and are more easily influenced by one another, so that you would expect greater community among Jewish psychologists than among psychologists at large.

An even greater difficulty with Roback's argument lies in the fact that he fails to say anything whatsoever about what there is common to Gestalt psychology and to Jewish culture—or to any particular Jewish culture. Empty correlations do not take one far. Honor by association leaves the question *why* unanswered. You can, after a fashion, say why phenomenology thrived in Germany and why functional psychology prospered in America. But why would not a Gentile have had that compelling insight



that caused Wertheimer to get off the train at Frankfurt in August 1910 when he had meant to go on to the Rhine, get off and, stroboscope in hand, start the line of thought that became Gestalt psychology?

The book, I have said, is well written. The flow is smooth, the reading easy. Once in a while there is an unintelligible sentence (cf. pp. 10, 44). Occasionally there is a strange figure of speech: "a thorough-going behaviorist would not think of mixing so many ill-digested ingredients in one caldron" (p. 243). But the worst defect of composition lies in the placing of the portraits, for the publisher has inserted them with no regard to sense at all. The great behaviorists appear to be Noah Porter and Morton Prince. McDougall and Thorndike illustrate Gestalt psychology, whereas Wertheimer and Koffka, along with the good Scottish James McCosh, seem to show you what Canadian Thomistic psychologists look like. It would have cost no more to have used a little discretion. And what is Stevens doing in the anechoic chamber when across the page Münsterberg dies on Radcliffe's platform?

Perhaps I, who have also written with some care a history of modern western psychology, am bound to assess the deviation of Roback's assured judgments from mine as if they were his errors; so perhaps someone with less assurance about what really happened should have reviewed this book, for self-assurance is egoistic prejudice and not neutrality. Let us, however, agree on this point. Psychology profits from having its history examined and put into words—good clear words, like Roback's—and an expression of difference of view is always intellectually healthy. If the reader can not decide between Roback and me on these moot points, perhaps posterity can. Shall we wait and see?

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*The Psychology of Human Learning.* By JOHN A. MCGEOCH. Second Edition. Revised by ARTHUR L. IRION. New York, Longmans, Green & Co., 1952. Pp. xxiv, 596.

The appearance of a revised edition of McGeoch's *Psychology of Human Learning* is an event of the first importance in the psychological world. For ten years the first edition of this book has been the definitive treatment of the factual material on human learning. Between two and three thousand articles have appeared in the field of learning since the first edition went to press, creating, as a result, an evident need for revision and reassessment. Irion had the commendable temerity to attempt the task, and the result is before us.

On the whole, this is a revision and not a new book. The greatest novelty of the second edition is a twenty-five page chapter on conditioned-reflex learning. A second new feature is a single chapter early in the book in which certain theoretical issues are brought together. Thus, with new introductory and concluding chapters, there is about one-sixth of the book which does not follow the original plan. The remainder of the revision, except for certain rearrangements, takes the form largely of emendations and interpolations in the original text. It is a mark of Irion's skill that the new material has been integrated so expertly with the old that only by a careful comparison of the two versions can the changes be seen.

The addition of a chapter on conditioning reflects, of course, the influence of Hull. The *Principles of Behavior* did not appear until the year after McGeoch's death, so that the treatment of Hull in the earlier edition was at the "empirical" level. That is now changed. For Irion, the role of reinforcement has become "one



of the most apparent facts of learning." It has passed entirely from the realm of hypothesis or speculation; it becomes a kind of general law of learning on which new speculations are based. When reinforcement achieves this status, some account needs to be given of the background of our present ideas about conditioning. Hence its introduction here, and its treatment in a manner altogether conventional.

The chapter on "theoretical considerations" will also be quite acceptable to most psychologists. Theory, according to Irion, is a set of guesses about the relations between two operationally defined variables. A few principles of learning have great generality. Intervening variables are good. Reinforcement is good. Secondary reinforcement is one of the most powerful concepts in the repertoire of theorists. The law of frequency needs looking into. These are the considerations in capsule-form.

Critical comment on a book which seeks to summarize our current knowledge about the process of learning is necessarily aimed as much at the present state of our knowledge as it is at the treatment afforded this field by the particular author. With this *caveat*, let it be said that the strongest impression which this book makes is one of an amazing provincialism. The psychology of learning has turned in on itself as it has grown. To a discouraging extent this is a book by a 'learning' psychologist for 'learning' psychologists. How much is this a result of the scholarly interests of the author? How much is it a reflection of the milieu in which this book was written?

A few statistics will help to make the point clear. A sample of eighty articles was examined, written by forty-three people whose names appear for the first time in this revision. These articles appeared almost entirely in a small number of American journals of the experimental stripe. The *Journal of Experimental Psychology* accounts for by far the largest number, followed by the *Journal of Comparative and Physiological Psychology*, this JOURNAL, the *Review, Bulletin*, and a scattering in the Murchison journals. There is next to nothing from the clinical and abnormal field, practically nothing from social psychology, and a single reference from the educational field. Extrapolating to the entire book, it is hard to believe that in the last ten years there have been written no more than five articles in the whole field of education worthy of mention in a text on human learning.

Much the same impression is gained from a detailed examination of the chapter on conditioned-response learning which presents only a cursory survey of the field. Perhaps the treatment was meant to be cursory, but it is out of step with the rest of the book. There is no reference to the work of Liddell, or of Gantt, and but a single reference to Brogden. The name of Skinner appears only as the person for whom the Skinner-box is named. Instrumental (*i.e.* operant) conditioning occurs, according to Irion, when the act to be learned is instrumental in securing the reinforcement. Shades of our ante-operational forefathers! By what means the animal knows that a response is "instrumental" we are not told. The conditioned response is presented here as it appears in Hull's *Principles of Behavior* and in the particular studies done to elaborate that point of view. The reader gets a somewhat foreshortened picture.

It is tempting to document this provincialism with numerous other instances that occur throughout the book. Views at variance with the general orientation of the book are slanted in conformity with well-known stereotypes. Thus, Thorndike's "belongingness" becomes a somewhat special case of transfer of training, and Köhler's "insight" is simply an odd case of unusually rapid learning. One final



example deserves special mention. In discussing the bilateral transfer of improved sensory discrimination, credit is given to an obscure paper published in 1933 with no reference to the fact that A. W. Volkmann's treatment of this problem was the earliest experimental work on transfer, and that in 1858!

A second persistent difficulty with the second edition is the lack of integration of McGeoch's theoretical views (based very largely on a modernized associationism) with Irion's strong preference for reinforcement theory. Actually, of course, McGeoch was often quite eclectic, but the experiments he chose to perform and the emphasis in his review of experimental findings left little doubt about his allegiance to Robinson, Carr, and Ebbinghaus. Explanations in line with this viewpoint are still scattered throughout the second edition, but superimposed are views taken variously from Hull, Hulls, and Hulls. At no point does the author come to grips with these rather fundamental differences, not only between McGeoch and Hull, but among the various versions of Hull.

The treatment of the problem of transfer in the second edition is an excellent illustration of this difficulty. For McGeoch, transfer depended upon associative links—identical elements if you will—and anything for which associations could be presumed could, to that degree, be transferred to other, associated situations. In the present volume, Irion includes all of the examples for which McGeoch used "transfer" as an explanation, and adds to the list. It reaches a bewildering length: learning to learn, warm-up effects, transfer from one class of material to another, stimulus-generalization, response-generalization, associative inhibition, transfer of general factors, of modes of attack, and of set, with some of these items summed up under the rubric of non-specific transfer! So far as this reviewer can detect, the only restriction on this list is that certain terms are proscribed for followers of the McGeoch-Hull tradition: formal discipline, transposition, understanding, and the transfer of general 'principles.'

An important restriction at the explanatory level arises here. Following Hull, these instances of transfer are to be subsumed under the concept of stimulus-generalization as conceived by Pavlov—quite a jump from the delightfully loose and easily argued "common bonds" which McGeoch's theory required. Such bonds were based upon empirical associations. Now, in contrast, we seem to be thrown back on nativistically determined stimulus-continua. It is a brave man indeed that sets out in the wilderness of transfer equipped with this one principle as his guide and compass.

A good case can be made out for the view that what is needed is not a simple theory but more incisive experimentation. Transfer evidently depends on a great many complex relations among the learning materials. It does not help the designer of a gunnery trainer to be told that Pavlovian generalization is the Alpha and Omega of transfer. Unless he makes a shrewd estimate of the demands placed upon the successful gunner and an equally shrewd estimate about what his trainer does, he will be in trouble. To use an analogy, the modern psychologist finds himself stoutly maintaining that evolution works without knowing the first principles of taxonomy. Irion's theoretical treatment of the problem of transfer makes it abundantly clear that we are not going to get very far in this area until we face up to the fact that the substance of what a person learns has some influence on how he learns it.

There are other theoretical difficulties which impress one on reading straight through a book of this kind. One is the curious confusion of principles involved



in, let us say, the explanation of reminiscence. Ten years ago most psychologists would probably have accepted an interference theory of forgetting as pretty close to the truth, but today the interference or inhibition has become a very pervasive thing. It turns up so frequently that obviously there must be some means of 'forgetting' inhibition which will be on the same footing as the mechanism of forgetting the original material. Only now, curiously, we are back with the discarded view that something, in this case inhibition, fades out with the passage of time, and if such a conclusion is justified with regard to inhibitory forces, may it not be equally valid for some portion of the decrement in positive reaction-tendencies? When psychologists deal with one set of problems, such as extinction and spontaneous recovery, they seem to come down on one side of the fence. When they deal with another, such as retroactive inhibition, they come down on the other side. Irion leaves the reader more than a little puzzled about whether he sides with McGeoch or with Hull.

Another area of confusion is that which surrounds the concept of secondary reinforcement. It need hardly be said that reinforcement theory must lean heavily on secondary reinforcement when it is extended to human learning. In the present text, secondary reinforcement appears as an explanation of Thorndike's debatable "spread of effect," of the temporal gradient of reinforcement, and of incidental learning. It is also discussed in the more conventional framework. One looks, therefore, for the justification of so ubiquitous a concept. It is disappointing to find a statement of the *principle of secondary reinforcement* which ends up, "A number of interesting questions may be raised concerning the mode of operation of secondary reinforcements. Probably the most significant question concerns their fundamental nature" (p. 242). The "principle," then, is little more than a convenient generalization plus a very nagging series of questions.

Once more the ghosts of ancient views rise up to haunt us. McGeoch is still permitted to say, "A minimum of satisfying consequences following a response may be sufficient for fixation when the transfer described is present. This minimal consequence or effect might be provided by the sheer operation of an already established habit or of a transferred set. The performance of well-formed modes of acting can in itself become a motivating condition, and the carrying out of the performance a satisfying consequence" (p. 285). Wherein, one wonders, lies the magic that makes the rose with just this name smell so sweet.

To sum up, let me return to the point made earlier. It is most difficult to assign responsibility for many things which this book is and is not. The provincialism of which this review complains is certainly shared by many of the 'learning' psychologists being produced today, but better writers escape it. Hovland, for instance, in his chapter for the *Handbook of Experimental Psychology* finds fifty-one references from the educational literature worthy of comment. The same favorable comparison holds for citations from foreign literature. Irion suffers, then, because he shares certain contemporary prejudices about scholarship. He shares the view that only experiments done with certain particular materials and techniques are worthy. He shares the contemporary view that his account of learning explains most current problems in psychology, as witness some of the truly remarkable claims made in the first chapter of the book. He shares the currently popular confidence in a all-inclusive theory. On this ground, then, if submitted to a majority vote of those who work in the field today, this book will doubtless win approval.

The second edition is a worthy successor to McGeoch's earlier venture. For com-



pleteness within its chosen field, for thoroughness, for usefulness in gaining access to the literature on learning, this book has no near rival in the field today. It is good that the revision was undertaken, and it is good that it is done. R. I. P.

Harvard University

EDWIN B. NEWMAN

*The New Man in Soviet Psychology.* By RAYMOND A. BAUER. Cambridge, Harvard University Press, 1952. Pp. xv, 229.

The study of the changing images and concepts of Man at different periods and in different places on this earth has always been fascinating. Ordinarily, the farther away they are in space and time, the sharper appear the outlines of such changes, the clearer their origins and effects, and the less threatening their implications. Dr. Bauer has had the courage to undertake such a study without the benefit of long historical perspective, at a time when geographical distance has lost its significance, and when the implications are anything but non-threatening. In *The New Man in Soviet Psychology* he has brought together the results of his scholarly and systematic examination of Russian publications with the material gathered under his direction by the Harvard Refugee Interview Project in Europe and in this country. His purpose is to demonstrate the change which the image of man has undergone in Soviet thinking. The nature of this change, its origin in the political and economic developments of Russia in the twenties, and its implications for the events since the mid-thirties, are traced out carefully and in detail. Despite the wealth and complexity of its content, the book is written clearly and with almost dispassionate objectivity, which makes it all the more impressive, if not actually frightening, document.

In the opening chapter the author points out some of the mutually co-determining relationships existing in any society between the social and political system under which it operates and the prevailing basic assumptions about human nature. The psychologist, as a member of a given society, partakes of these assumptions and they are in turn reflected in his choice of problems and methods of study, as he selects and ignores in accordance with implicit and often altogether unconscious cultural values and confines. By contrast, in a controlled society such as the USSR, the choices are directed explicitly by central agencies with well-defined goals. For this reason the study of the psychology emanating from present-day Russia can give us valuable insights into the goals of her political leaders. As a corollary, the very problems investigated and theories expounded demonstrate the foci of their impact on the social system and the specific mechanisms by which they can be made, first, to influence social developments and, subsequently, to rationalize the results.

A brief discussion of "two kinds of Marxism" provides the larger frame for the two sets of concepts of man to be discussed throughout the book. Dr. Bauer describes the historical roots of the political philosophies underlying mechanical and dialectical materialism, and demonstrates how the results of economic decisions based on the former prepared the way for (and eventually necessitated the change-over to) the latter. In this section, as throughout the book, the naïve reader cannot help but be impressed by the apparently overwhelming need of the Soviet system—in whatever form—for explicit theoretical formulations. Furthermore, given the striving for theoretical consistency, we marvel at the mechanics, e.g. the official controversies, congresses, and so forth, for the achievement of the desired end.



The chapter on "the patterns of social change" is one of the most interesting in the book, because it illustrates concretely the meaning of the change from the mechanical to the dialectical point of view for two major social institutions—law and education. Both are fundamentally concerned with human nature, its formation, and the results of such formation, and in both we see parallel developments. The earliest decrees concerning both institutions were based on a Rousseau-like conception of man as inherently good, misshapen only by society, in this case a capitalistic class-society. By contrast, the decrees and pronouncements of the mid-thirties reflect a very different concept of man—one which is much like Weber's description of the protestant; man is seen as the conscious and deliberate cause of his own actions for which he must therefore be responsible. What happened in the interval?

It may sound simple-minded to say that the latter viewpoint came to be accepted because the former had been found lacking in its application, yet Bauer demonstrates that this is exactly what occurred. Man can be considered inherently good only as long as there is a society which can be blamed for man's short-comings. During the early years of the Soviet regime the capitalistic class society was held responsible for inhibiting and misguiding man's spontaneous development; the individual committing a crime was not guilty—an evil society had forced him into evil ways. A generation later, however, the situation was different. Under a very permissive educational system children had been allowed to "develop spontaneously." Juvenile delinquents as well as adult social deviants had been treated quite leniently, with explicit emphasis on reëducation rather than punitive retribution for misdeeds. The capitalistic class-society could no longer be held responsible for crimes which were still being committed, for the ever-increasing rate of juvenile delinquency, or for the lack of scholastic achievement which began seriously to hamper industrial progress; nor could the new classless society be blamed. The implication was two-fold: In the first place, energetic steps had to be taken to rectify this situation by proper training. Secondly, and basically much more important and interesting, man could no longer be considered innocent. Instead, transformed into a purposive builder of the new socialist society, he consciously decides on his course of action and hence is responsible for his wrong-doing.

Against this general background of socio-political change Bauer proceeds to analyze in the remainder of the book the correlative changes that took place in the field of psychology and to indicate carefully some likely future developments. In the early twenties psychology in the USSR had to struggle for the mere right to exist. It was doubted that psychology could ever meet the methodological requirements of exact quantitative and objective science, and it seemed that the study of psychological matters precluded the accepted materialistic outlook. Little wonder that only behavioristic psychologies could emerge in the mid-twenties to be accorded scientific status and official recognition. Lip service continued to be paid to the importance of the genetic substratum, but the basic outlook was a radical environmental determinism which, for instance, interpreted the finding of systematic differences in test-intelligence among children of different subcultures as proof of the overwhelming importance of environmental influences. Consciousness became a very suspect article which smacked methodologically of introspection and subjectivism, philosophically of idealism and free will, psychologically of deliberate action, legally of the responsibility of the individual. Human personality was seen as the result of a history of passive and



mechanical adaptations to environmental pressures, the basic motivation being the maintenance of a similarly mechanical equilibrium. Individually directed conscious action could only interfere with the smooth running of such a mechanism. Interestingly enough, however, unconscious determinants of behavior were readily accepted for the same reason that consciousness could not be. The action of unconscious factors was said to be mechanical and inescapable, and thus supported the accepted deterministic position.

The shift from the mechanical to the dialectical, Lenin-inspired point of view coincided with the beginning of the new decade. The author points out that this transition did not proceed as smoothly or as rapidly as was officially desired, and describes the six-year period from 1930 to 1936 as one of "exhortation and criticism." The practical needs of industry and the principle of party vigilance were combined in the slogan "unity of theory and practice—with practice leading theory," a tenet which we, in our loftier moments, regard as spelling death to proper scientific inquiry. The picture of the New Man emerged clearly. He became the conscious, purposive builder of the socialist state. The mechanistic model of man adapting passively to a mechanical equilibrium is now being decried as being fit for capitalists who want exploitable robots. The hyper-environmentalism has been renounced, and the concept of "self-training" has been added. Conscious understanding provides the necessary basis for "self-training," making man capable of change even as an adult, and thus responsible when he fails to train himself in correct habits. Unconscious determinants of action are no longer acceptable since they would interfere with this process and absolve man from responsibility for his misdeeds. Acceptable motives are needs, interests, ideals, and duty—all of them plastic except for the ultimate goal of becoming the best possible and the most highly rational Soviet citizen.

Psychological research in the USSR since the thirties centers around practical problems of the control of the individual. A large proportion of the work is in child psychology and educational psychology and is concerned with problems of conscious understanding and human learning. Animal learning which is said to lack application to human learning has largely disappeared from consideration. Sensation and perception also are popular fields, partly again for their concrete applicability, partly for their relative safety from political implications which makes them "a providential oasis in an ideological desert." Some work is done in psychopathology, but this primarily in connection with medicine and psychiatry and outside of the field of psychology proper. Even here dynamic "depth" concepts are proscribed. Testing in schools and industry has apparently ceased entirely. Social psychology and the study of attitudes with implications as dangerous as those of the study of unconscious motivation have disappeared from the Russian psychological scene. Since 1950, there is some renewed interest in conditioned reflex psychology or, more specifically, in Pavlov's second signal system. The importance of this interest for the future of Russian psychology has not become very clear as yet.

Towards the end of the book the author asks a very significant question: Given the change in the image of Man that has been the topic of the discussion, what has happened to the men whose picture has been painted? Has the Soviet citizen changed along with his official socio-political portrait? Tentative as the answer must of necessity be, the indications are that it is in the affirmative. Comparisons of Russian emigrés who left before or shortly after the Revolution with those who fled only recently show the latter group to be more practical, better at manipulating and extemporizing,



more self-confident, more rationally controlled in their behavior, and less emotional. While the reasons for these differences may be many, Bauer points out that the recent emigrés survived for many years under the Soviet system and that their "new" characteristics are not only those specified by the predominant ideology but they are also those which make possible survival under the Soviet system. Irrespective of whether the observed changes are due to explicit teaching or represent effective mechanisms of existence, they clearly fall short of the political ideal. External controls, e.g. the secret police, are still necessary and there are no signs that the system could exist without them in the foreseeable future.

Is it presumptuous to come back to the effects of political control on science after having indicated its effect on the human personality itself? If one has illusions about the incorruptibility of Science, the sober and impartial judgments of her servants, he had better not read this book lest he be forced to relinquish some very dear convictions. Dr. Bauer's book demonstrates all too clearly how political theory and expediency can influence psychological theory and experimentation, and it gives us more than a hint that this is not a one-way process only.

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MARIANNE L. SIMMEL

*Cybernetics: Circular Causal and Feedback Mechanisms in Biological and Social Systems, Transactions of the Eighth Conference.* Edited by HEINZ VON FOERSTER, MARGARET MEAD, and HANS LUKAS TEUBER. New York, Josiah Macy, Jr. Foundation, 1952. Pp. xx, 240.

I am not at all sure how to review this book. I am not even sure it is a book. It looks like a book. It is nicely printed and bound like a book, and it will go on a shelf with books, but inside the spinach-green binding some puzzling and un-book-like things are lurking.

It might be a textbook. The chapter titles (I think they are chapters, but that is my own interpretation) sound like someone aspired to cover communication exhaustively: "Communication patterns in groups," "Communication between men," "Communication between sane and insane," "Communication between animals." If the last two chapters on a "Maze-solving machine" and "In search of basic symbols" had been called "Communication between machines," the textbook coverage would have been obvious. But it is not a text; there is no index, facts are often misquoted, references are scarce, and the comprehensive titles are not supported by comprehensive articles.

It is not poetry. It is not a novel. It could be a play; if the setting of the conference were described, if the seating arrangement were shown for each act, if a few more stage directions were given for the players, I would feel sure it was a play. The plot is pretty poor, but modern drama sometimes seems almost as verbose and inconclusive. But it is not a play. It is not any of the things you ordinarily expect to find when you browse in your favorite bookstore. The standard appreciative and critical responses to the traditional literary forms are not available to the reviewer of this publication.

It is, in fact, a conference dressed up to look like a book. Under the guidance of their "chronic chairman," Warren McCulloch, some 23 top-notch people talked to each other and a recording machine for two days. Then Von Foerster, Mead, and Teuber tried to make sentences out of what they said and the Josiah Macy, Jr.



Foundation paid to have it printed. This is the third time it has happened, so the editors have had some practice and the foundation must know what it is supporting. But in spite of the brilliance of the contributors, the diligence of the editors, the generosity of the foundation, and the best efforts of the printer to make it look like a book, the result is still a conference.

Some people love conferences. They are bright people and can make their affection sound rational. I am not a conference lover myself, although I can see little harm in them, and I do not wish to argue their value. But is it necessary to *publish* the conferences? This practice is spreading and it is time to ask what it achieves. A published conference is not a standard avenue of communication; the contributions usually have been or will be published elsewhere in journals and books. A published conference does not even have the advantage of the social contact that a conference provides. A published conference has all the worst features of both conferences and publications and almost none of their virtues. If the practice is continued it can debase the value of scientific communication until we feel as little responsibility for our formally published words as we do for our cocktail conversations.

Now let's examine the document itself. The curtain rises on Alex Bavelas, who describes two experiments on five-man groups. The members coöperate in fixed communication-nets to solve simple problems. These elegant experiments had their origin in the group dynamics of Lewin and his colleagues. In the attempt to relate Bavelas' report to information theory, feedback, game-theory, computers, and cybernetics, the discussants wander far afield.

The second act finds I. A. Richards in the center of the stage. Richards is so eloquent that he doesn't always bother to think, but when he does bother the results are fresh and interesting. He uses the term "feed-forward," to describe the activity that goes on when we search for something. He tells a little about his philosophy and technique of teaching reading, and he delivers various interesting opinions about esthetics, criticism, Harvard students, dialectic, quotation marks, linguistics, and Bohr's Principle of Complementarity. I do not yet see how all these things belong in Richard's talk, but they give the discussants plenty to work on.

Lawrence Kubie provides a psychiatric theme for third act. With anecdotes he demonstrates that "communication can occur effectively without full conscious participation in the process." Communication in hypnosis is his particular interest. McCulloch adds a few anecdotes of his own and the discussion goes into the kind of symbolic processes that occur in dreams, fantasy, doll play, and so forth. This interlude should convince anyone to whom it is not already obvious that cybernetics is not going to be of much immediate value to the clinician. All the obvious analogies between neurotic brains and misaligned computers have been drawn—and quartered—but clinical practice is pretty much where it was before.

The conference members as a group keep stumbling over a familiar dilemma. One faction in the group feels that a comparison of psychological processes with computing machines can be made only by ignoring the great complexity of the psychological processes. This faction is not content with partial answers and suggestive analogies, however scientific, if they do not cope with the idiographic phenomena of clinical and personal experience. The opposing faction insists on the scientific method in all things and is frustrated by the vagueness and disagreement in the description of these complex, allegedly meta-mechanical phenomena. In the words of Leonard Savage, "We have had, throughout the existence of this group, the im-



portant problem of seeing if there is anything that people do, that can be precisely stated, that may not be done by a machine. We have always known that that hope was in some sense chimerical because there is the famous theorem of Turing, which we used to hear about at every meeting, to the effect that if you could only tell precisely what you want, you could make a machine that would do it. And yet we have continued to hope, in spite of this theorem, which in a sense should end all our hopes that we might discover some kind of behavior which does not deserve to be called mechanical." This dilemma, in one form or another, is familiar in almost every large psychology department in the country; the arguments vary, but the sentiments are unmistakable.

In Act IV, Herbert Birch tells some of the stories about social interaction among animals—behaviors that obviously depend upon some kind of communication. Birch spends most of his time on von Frisch's bees and his own observations of sub-human primates. Heinrich Klüver adds further interesting footnotes, but the discussion bogs down under the semantic weight of defining the term "communication" when applied to animal behavior.

Next Claude Shannon demonstrates a maze-solving machine that is worthy of his reputation for cleverness. Shannon points out the properties of the machine and generally lets his audience draw the analogies with human behavior. As a psychologist, I am happy to report that none of the psychologists present in the audience participate in this analogy-drawing contest. Doubtless any of the psychologists could point out five properties of an animal's maze behavior that the machine does not have for everyone the machine does have. It is a hopeful sign, however, if men of Shannon's ability are willing to worry about a psychological process like learning. Something interesting is bound to appear eventually.

In the last act, Donald MacKay finally gets the discussion onto that high level of speculation that Norbert Wiener and John von Neumann might have sustained, had they been present. MacKay first repeats his distinction among structural, metrical, and selective information. His useful glossary of terms, which was first prepared for the Symposium on Information Theory in London in 1950, is republished here as an appendix. Then with fine optimism he sketches in outline an artificial organism that he believes can learn to recognize patterns in the flux of stimulation. Unlike Shannon's actual machine, MacKay's conceptual machine is built of probabilistic elements that make abstractions by a kind of natural selection of the fittest.

Since the boundary between cybernetics and science fiction has never been overly sharp, those of us seriously interested in the social and psychological applications of this kind of thinking have often wished for some standard of scientific respectability in this young discipline. The Macy conferences could supply such a standard, for the participants are uniformly men of solid achievement and deserved recognition in their own fields. Instead of becoming a scientific standard-bearer, however, the conference has chosen to emphasize interdisciplinary propaganda. After eight conferences this line is running a little thin.

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GEORGE A. MILLER

*Color in Business, Science, and Industry.* By DEANE B. JUDD. New York, John Wiley and Sons, 1952. Pp. ix, 401.

Visual scientists are concerned largely with the nature of the visual mechanism and the development of suitable theoretical models to integrate the known facts.



Judd's interests along these lines are well known. In this book, however, he presents a different aspect of his interest in color, an area in which he has been very productive throughout his long stay at the National Bureau of Standards where he is now Chief of the Colorimetry Unit. The interest here rests heavily on the stimulus-aspect of the visual situation and the book deals primarily with problems of color measurement and specification.

Judd has written a straightforward, clear and well integrated account of the practical application of visual psychophysics to the problems of color measurement in industry. The book's purpose is avowedly practical and applied. It is intended to provide the business man and his technologist with the information necessary for color inspection, measurement, and control of both the raw material and finished product. At a time, however, when some psychologists have written off color diagrams as having little other than pedagogical importance and still others erroneously see the only utility of chromaticity diagrams as perceptual charts, it is clear that one need not be either a business man or his technologist to benefit from reading Judd's book.

The book is divided into three major parts, each divided in turn into appropriate sections rather than conventional chapters. Part I, entitled "Basic Facts," presents a very readable elementary account of the operation of the eye, a discussion of basic color terms, a clear formulation of the principles of color matching that are fundamental to all colorimetry, and a good brief description of the color deficiencies. In his discussion of color matching, Judd makes an important distinction between the additive mixtures produced by adding lights and similar mixtures produced on rotating sector discs which are time-weighted averages. Part II, "Tools and Technics," constitutes the heart of the book and is a thorough account of the problem of color specification and measurement. It is intended primarily to assist anyone interested in color specification for commercial purposes, and a solid footing is provided throughout by repeated reference to specific practical problems. The detailed presentation of available techniques and standards will, however, also interest any psychologist who plans experiments requiring rigorous specification of visual stimuli.

There is a complete and easily followed description of the use of the CIE system of color specification. This is the official abbreviation of the French name, Commission Internationale de l'Éclairage, and replaces by international agreement the initials ICI formerly widely used in America. Visual colorimeters and colorimetry by difference also are reviewed and a thorough and systematic survey is presented of the available material standards of color. The psychologist interested in the theoretical aspects of sensory scales may be surprised to learn how important the problems of uniform color scaling have become to industry and commerce. A final section on the color languages employed by various specialized groups summarizes the terms and definitions for the most important color concepts used by American industry. The compilation emphasizes the diversity of interest in color.

Part III treats briefly the "Physics and Psychophysics of Colorant Layers" (colorants are pigments or dyes which absorb differentially in the visible spectrum) and may be of interest to the psychologist concerned with complex *Erscheinungsweisen* stimuli (gloss, turbid media, clear media, and so forth). This difficult problem is analyzed here from the point of view of the manufacturer concerned with identifying and formulating compounded colorants.



The book as a whole documents in striking fashion the impetus given to research and technological development by industrial advances and requirements. There is room for disagreement on some general statements of theoretical importance, for example, when Judd ascribes the limitations of three-light rendition in television, color photography or three-color process printing to the kinds of visual receptors we have. Such statements are incidental, for the most part, and do not bear directly on either the purpose or usefulness of the book. The book fully achieves its stated purpose: "first to tell how the eye works, second to list the tools available to assist in color measurement and third to show how to select the best tool for a given color-measuring job." The writing moreover, is lucid and unusually interesting, and the book is well supplied with useful tables and illustrations.

Color Control Division  
Eastman Kodak Company

LEO M. HURVICH

*Social Psychology*. By ROBERT E. L. FARIS. New York, Ronald Press, 1952. Pp. vii, 420.

Many social psychologists of all persuasions—psychological, sociological, and anthropological—are struggling to state the nature of their field and its problems. For each there is pressure to differentiate his work from that of his parent field, but not all have succeeded as well as the sociologist who wrote the introductory text under review. For Faris, social psychology is "the study of those aspects of human personal behavior which are developed and controlled by the interaction which takes place between the individual and his small intimate circle of associations known as the primary group . . . materials from the subject fields of either sociology or psychology which do not bear directly on this conception of the subject have been excluded in the interest of a clear and integral organization" (p. iii). "Human personal behavior" is not explicitly defined. We find, however, that primary groups ("group" is not defined) are characterized by a high degree of intimacy, affection, feelings of similarity, and influence among their members (pp. 250-51). Examples of such groups would be the idealized family of American tradition or a small group of friends. Thus Faris' task is one of specifying the results for human behavior of man's immersion in the conditions and experiences provided by primary groups.

The book that springs from this focus has six major parts: (1) an overview of the importance of social experiences for human behavior, the rôle of such experiences in (2) motivation, in (3) self-consciousness, in (4) a miscellany of other psychological processes, in (5) inter-individual differences, and, as a conclusion, (6) a chapter on the history and problems of social psychology.

We are first impressed with the behaviors lacking in men who have had no experiences in primary groups. Such men, it is claimed, have no minds and no self-percepts—"all that is distinctly human in the behavior of man results from his participation in organized social life" (p. 3). The main discussion then begins with an extended treatment of motivation. Instinct and libido are rejected along with related concepts, such as drive, that locate motivation in the biology of the individual. The intention is to show that human motives correspond to the goals which are valued as a result of rewarding experiences. Linked with Dewey's notion of physiology as providing a kind of general, undirected impulsiveness, this means that the acting individual experiences his environment selectively, and, in a sense, helps to create his own motives



in the process of learning what will reward and what will deprive. The pre-existing society, through the agency of primary groups, provides most of the experiences that this unformed organism will have and hence it supplies the raw materials from which his motives will develop.

The second emergent from the experiences of the individual in primary groups is self-consciousness. Faris describes its emergence by first reviewing and advocating Dewey's identification of mind with the "organization of behavior" (p. 107) and of consciousness with "a trial-and-error probing of a crisis" (p. 91). He takes occasion to repeat Dewey's critique of the reflex-arc concept and of the theory of conditioning as an "explanation of complex activity, of learning . . . and of consciousness" (p. 78). He supports Dewey's contrast of the unwitting nature of habit with the nature of conscious behavior. Self-percept (self-consciousness) is viewed from the standpoint of George H. Mead, and its consequences for behavior are considered. Between these chapters on the rise of consciousness and of self-consciousness is a chapter certain to raise controversy. In it, Faris examines the concept of the unconscious—especially the unconscious defined as consisting of repressed materials—and rejects it for want of acceptable supporting evidence.

The third group of chapters reviews much of the conventional material on the results of social experience for learning, perception, memory, attitudes, beliefs, and abilities. The fourth section of the work treats individual differences in behavior as functions of differences in primary-group experiences. Considerable space is given to general chapters on the neurotic rôle and on personality disorganization. There is a final chapter on trends and problems in social psychology distinguished chiefly by a list of basic research problems.

For this reviewer, the strengths of the book, and they are considerable, lie in its presentation for the general reader of the social psychology of George H. Mead, in its reiteration of the implications of Dewey's view of original human nature, in the clarity with which it states a point of view on such controversial matters as the nature of repression and of personal disorganization, and the truly courageous and much-needed effort to list the questions that have top priority in the future of social psychological research, Faris' ideas, and the problems that he considers of first importance, will be familiar to many sociologists. They deserve more attention among psychologists.

University of Michigan

G. E. SWANSON

*Psychosurgical Problems*. Edited by FRED A. METTLER. New York, The Blakiston Company, 1952. Pp. xii, 357.

*Shock Treatment, Psychosurgery*. By LOTHAR B. KALINOWSKY and PAUL H. HOCH. New York, Grune & Stratton, 1952. Pp. xii, 396.

The first of these two books on the somatic therapies in psychiatry presents the results of an extension of the Greystone Project which in 1949 was described in *Selective Ablation of the Frontal Lobes*. The second is a survey of a variety of shock, psychosurgical, and pharmacological treatments used in the practice of psychiatry. Though both books report material in some detail, they are rather loosely written, making it difficult for the reader to determine the present status of the somatic therapies. Nevertheless the persistent and patient reader will find in them important information on which to base an appraisal of these methods of treatment.

In the research on *Psychosurgical Problems*, intended to clean up after *Selective*



*Ablation of the Frontal Lobes*, the temptation to attack new problems was not resisted. Thus, instead of using the available Ss to demonstrate reliably the effect of venous ligation, the investigators used some to study the effects of thalamotomy, some to study the effects of transorbital lobotomy, and some to study the effects of thermocoagulation. As a result, the findings are inconclusive of anything in particular and suggestive of everything in general. The suggestions, though, would encourage the desperate practitioner to try psychosurgery and the investigator to continue his research.

The first chapter is commendable for its delineation of the pitfalls in research involving mental patients. Certainly the emphasis on the need for adequate experimental controls is well placed and should serve as a caution to other investigators. The chapters on the surgical techniques and the medical condition of the patients are explicit and detailed. These chapters, which survey the results of other relevant studies, may be criticized for going beyond the data; albeit with apology, they carry unwarranted conviction. The two rather new approaches, attitude-change and time-sampling, are interesting additions to the methodology of psychological assessment and when sharpened may prove to be the most revealing of all the techniques discussed. All the chapters on assessment deal admirably with the problem of the practice-effect and doggedly pursuing the elusive N. The psychiatric report is based upon data of unspecified reliability, and the effectiveness of operation cannot in consequence be determined.

The concluding chapter reiterates the almost insurmountable methodological problems in assessing the effects of somatic treatments on psychiatric conditions. The problems bear reiteration. The general conclusions to be derived are neatly summarized but need to be qualified by reference to the individual chapters. The sally into theory construction is certainly inadequate; neither all the possibilities nor all the data are considered, and the terms used are so general that the hypotheses probably defy experimental test. The book raises many problems though it offers few answers for them. Nevertheless therapists as well as investigators interested in the effects of psychosurgery will want to be familiar with the material presented in appraising their convictions about the usefulness of operations on the brain as therapeutic procedures in psychiatry.

*Shock Treatments, Psychosurgery* is a second and revised edition of the 1946 publication. Recent advances in the somatic therapies have been woven into the earlier text and many new sections have been added. The chapters are systematically organized and the bibliography is extensive. Readers interested in acquiring a general familiarity with the somatic treatments will be satisfied with the discursive style and summary reporting of investigations, but those interested in the experimental details and in the critical evaluation of experimental literature will be disappointed. Moreover, the frequent juxtaposition of experimentally supported statements and the authors' clinical opinions will probably annoy the reader looking for substantiated, unbiased conclusions. The vague and undefined descriptions of behavior make it difficult to determine what is meant by a change in the patient. The chapter on theoretical considerations, though valuable in calling attention to the various theories held with respect to the *modus operandi* of the somatic therapies, fails to provide the information necessary to an evaluation of these theoretical positions. It is probable that the state of our knowledge about the effects of the somatic therapies precludes a satisfying theoretical chapter on the subject.



Both books are valuable contributions to the field if only in that they point out how little is known about shock treatments, psychosurgery, and other somatic therapies.

Yale University

H. ENGER ROSVOLD

*Psychology*. By ROSS STAGNER and T. F. KARWOSKI. New York, McGraw Hill Book Co., 1952. Pp. xiii, 582.

This book is similar to many recent texts in introductory psychology in that it competently and adequately deals with the recent data of psychology and presents its findings in a well-illustrated and readable manner. The purpose of this text is to provide the student with an over-all view of man and his nature. Its contents, with few exceptions, include the traditional and expected material. This book however differs from other texts in two important respects. It attempts to organize the varied data of psychology under the heading of homeostasis and it essays a psychoanalytic description of personality development.

The authors believe that inasmuch as the concept of homeostasis is compatible with the thinking of almost all schools of thought, the use of this concept may preclude the customary presentation to the introductory psychology student of a confusing array of facts. To make homeostasis sufficiently flexible for their purposes, the authors distinguish between static and dynamic homeostasis. The former is the more frequently encountered physiological interpretation, the latter is used to incorporate the phenomena of social life. The authors chose dynamic homeostasis rather than adjustment to interpret social phenomena because they claim that its action is more easily observed. This enlarged use of homeostasis to explain both biological and social phenomena frequently has a tacked-on and forced quality, e.g. "homeostasis and receptors," "homeostasis and perception," "homeostasis and personality," and so forth. In the discussion of social motives the authors find it necessary to utilize principles other than homeostasis, for, as they themselves admit, the concept does not completely explain psychological data. Progress, according to Stagner and Karwoski, occurs when man proceeds to higher levels of equilibrium by trying out changes. It may be questioned whether even dynamic homeostasis allows for this process or whether it merely provides for an oscillation without specific direction. As a result of the varied statements made about the nature of homeostasis, one wonders just what its rôle is thought to be. While we may agree, as the authors claim in defense of their organizing principle, that "facts without theory" become meaningless, the converse is also true.

The authors sketch in six pages the Freudian theory of personality development and manage to include in that space some discussion of the infantile passive stage, the oral stage, the anal stage, the genital stage, the pleasure and reality principles, the latency period, the normal homosexual stage, the id, ego, super-ego, and so forth. It is doubtful whether the Freudian theory of personality development should be presented in an introductory text, not because it cannot be assimilated, but because of the lack of time available to the instructor. As a result of this premium on time, the historical context of the Freudian contribution to psychology may be ignored and its present use and misuse treated rather hastily. Consequently the beginning student may obtain an unusually peculiar and distorted idea about psychoanalysis.



There are other minor points, as in most texts, which will leave the reader pleased, displeased, or puzzled: e.g. the discussion of the differential effects of deprivation and satiation on sex and love, the unnecessarily detailed material included in the chapter on conditioning, the emphasis on heredity in explaining individual differences, and the relatively small space given to discussions of smell and taste. The basic issue which confronts one in considering the use of this text is whether the data of psychology have advanced sufficiently to be organized under a systematic principle or whether as yet one needs to be more eclectic in this still young science. Stagner and Karwoski tend to the former interpretation, the reviewer to the latter.

State College of Washington

F. L. MARCUSE

*The Drawing Completion Test: A Projective Technique for the Investigation of Personality.* By C. MARIAN KINGET. New York, Grune & Stratton, 1952. Pp. 238.

This volume describes the development and use of a projective test, based upon a technique previously employed in the Horn-Hellersberg Test. The rational of the present technique derives from the work of S. Sander (of the University of Leipzig) and from the subsequent work of Wartegg, whose test blank is employed by Kinget. The test consists of eight squares, each containing such elements (or "signs") as a dot, a small black square, an arc, or two or three lines. The S is requested to make a drawing in each square, utilizing the given elements. Drawings are made in pencil, erasures are permitted, and S may follow any prescribed sequence in his eight drawing-completions, the drawing sequences being recorded by the examiner. There is no time limit.

The drawings are examined with respect to three major variables: (1) *stimulus-drawing relations*, i.e. the resemblance between the Gestalt-qualities of the completed drawing and those of the original elements given on the blank; (2) *content*, with special reference to the presence of scribbling, abstractions, and pictures of nature, of objects, or of phantasies; and (3) *execution*, which includes form-level, line-pressure, covering, shading, and composition. What is essentially a six-point scale is used in scoring each drawing on each of the above variables, although symbols, rather than numbers, are used to designate the scores. No objective norms are reported. In discussing the interpretation of scores, Kinget reverts to the usual vague and subjective statements which have plagued most projective tests for many years.

The validation sample is described as consisting of 383 adults, between the ages of 18 and 50; presumably, all were Europeans. The criterion data were obtained by: (1) a self-report inventory, (2) a forced-choice test, and (3) a rating scale filled out by friends and acquaintances of the Ss. One searches in vain for an adequate report of the procedures employed in analyzing these data. Despite Symonds' statement, in the *Foreword*, that he is impressed by "the criterion against which she determined the significance of each element of the drawings" (p. v), no objective method is described whereby the criterion status of each S was determined. After pointing out (p. 21) that there was considerable lack of agreement among the three criterion measures, Kinget rejected any attempt to arrive at a composite criterion in favor of a criterion based upon "the main trends manifested" in the three criterion measures. There is no indication of the degree of consistency necessary to constitute a "main trend."



Here, then, we are confronted with a highly subjective instrument. Until such time as more objective scoring procedures are developed, and until the validity of the test has been more clearly established, it is virtually worthless as a measuring instrument. For these reasons, the unqualified and popularized account of this test in such a magazine as *Life* (June 9, 1952, p. 65 ff.) would seem to be premature and misleading.

The Psychological Corporation

JOHN P. FOLEY, JR.

*The Ape in Our House.* By CATHY HAYES. New York, Harper and Brothers, 1951. Pp. vi, 247.

*The Ape in Our House* is a charming and delightful book in which the protagonist is a young chimpanzee, and the heroine is undoubtedly the author. Ostensibly the book describes the behavior of a chimpanzee baby reared in an American home, but the home, like so many American homes, becomes progressively modified by the infant being reared. The fundamental characteristics of human society are represented in the Hayes' home, and the described goal of the investigation, to determine if chimpanzees can "adjust to the human way of life, and make use of the tools of our culture," may be regarded as achieved. Viki demonstrates a very considerable ability to adapt to our culture, a capacity hardly surpassing, however, the ability of the Hayeses to adapt to Viki's.

This book, half literature and half science, is written with affection and warmth. The accounts of the "imaginary play toy" and the conversation with the Ann Arbor policeman are alone worth its modest cost. Hardened as is the reviewer to monkey jokes, he found the puns, quips, and anecdotes amusing.

Unlike most animal books, the literary charm is not tainted by factual distortion, and the reviewer has talked with many casual acquaintances of Viki's who were less restrained in describing her exploits than is the author. Detailed and factual accounts are presented on three major exploratory experiments: linguistic training, imitative sets, and visual and manipulatory exploration. The spectacular stalemate in the heroic attempt to teach the ape to talk is in keeping with the earlier literature, but no one before has conducted a series of experiments which could be regarded in any way as definitive. Different and highly positive results were obtained on the formation of imitative sets, and these data are important both in their own right and in their implications for future research. The data on the visual and manipulatory behavior are striking, but somewhat less new and unexpected.

Although it has long been known that the chimpanzee is capable of a wide repertoire of social behavior, the information presented by the book on the range, variety, and nuances of affectionate and bellicose behavior is most informative. One cannot help but be struck by the sharp differences in social responsiveness that seem to differentiate apes from monkeys, differences far greater than those demonstrated in formal learning tasks.

But the book is much more than a study in the development of chimpanzee affection. It is a thought-provoking treatise on the way in which affective relations in all primates, human and subhuman, develop. With this fact in mind, the book might well be made required reading for all expectant parents, who, after reading it, should cease to worry whether the child will be a boy or a girl.

University of Wisconsin

HARRY F. HARLOW



*Studies in the Psychology of Reading.* By WILLIAM C. MORSE, FRANCIS A. BALLANTINE, and W. ROBERT DIXON. Ann Arbor, University of Michigan Press, 1951. Pp. ix, 188.

This volume reports three studies of the psychology of reading. The first, by W. C. Morse, was concerned with changes in the eye-movements of fifth and seventh grade pupils reading materials which are easy (two grades below), at grade, and difficult (two grades above). All materials were descriptive science and were evaluated for difficulty by the Lorge and by the Yoakam formulas. Seventh-grade pupils revealed more efficient eye-movements than the fifth-grade pupils on material read by both, and there was no consistent change in eye-movements with increase in difficulty of materials.

The second study, by F. A. Ballantine, was concerned with age-changes in measures of eye-movement during silent reading. Eye-movements of 20 Ss in each of grades II, IV, VI, VIII, X, and XII were photographed. Each child read an easy passage (second grade material) and a passage at grade, the material being semi-scientific in nature. Eye-movement measures improved rapidly from grade II to IV, leveling off from IV to VI and improving again from VI to VIII, with little growth thereafter. As in the first study, patterns of eye-movement were not influenced by the difficulty of the reading material. It is possible, however, that in both cases differences due to difficulty were masked by a strong set derived from the instructions given the Ss.

The third study, by W. R. Dixon, was concerned with eye-movements during the reading of material in physics, history, and education by specialists in these and other fields. Materials in the three fields were equated for difficulty. The Ss were 16 professors and 16 graduate students from each field. Each subject read material in his own field and also the materials in the other two fields. A tendency was found for Ss to read materials in their own special fields most efficiently. Neither the history nor the science materials in the present study produced a special type of reading, although the training in history tended to speed up reading. It was concluded that familiarity of material may be regarded as a factor in reading, but that different types of material probably do not automatically elicit different types of reading. The first conclusion is sound, but the second should be limited strictly to the materials read in this study and to the conditions of the experiment. Apparently the material was straight-forward discourse; passages in the different fields were of equal difficulty, and the Ss read all passages with approximately the same attitude. These conditions would tend to produce uniform reading performance. Furthermore, the selections were short (100 words or less)—too short, perhaps, to foster a reading attitude specific to the content. It is likely that the technique of Robinson and Hall, using long selections, is more appropriate for studying the relation of content to reading performance. It does not seem logical (to take an extreme example) that it is efficient to read an arithmetical or physics problem in the same way as an item from the newspaper.

These three experiments constitute important contributions to the psychology of reading. They do not give final answers to the questions investigated, but they are more carefully designed than most studies in the field and add materially to our knowledge.

University of Minnesota

MILES A. TINKER



*Adrenal Cortex*. Transactions of the Second Conference. ELAINE P. RALLI, Editor, Josiah Macy, Jr. Foundation, New York City, 1951. Pp. 209.

Five reports made to a limited group of participants: (i) The present state of the chemistry of ACTH, by Choh Hao Li, Biochemistry, University of California; (ii) Regulation of pituitary adrenocorticotrophic activity, by George Sayers, Pharmacology, University of Utah; (iii) The relation of adrenal cortical hormones to the hypersensitive state, by Thomas F. Dougherty, Anatomy, University of Utah; (iv) Hyaluronidase and the adrenal cortical hormones, by Jeanette Opsahl, Physiological Chemistry, Yale University, and (v) Further clinical studies with ACTH and adrenal cortical hormones, by George W. Thorn, Harvard Medical.

After thirty or more fruitful years of the 'interdisciplinary' sort of conference—an undertaking promoted by the young National and Social Science Research Councils—Director Fremont-Smith may well reflect here upon the high achievements of the Macy Foundation. Since the multi-science conclaves appear, however, to achieve most where the several contributing subjects remain stable and productive, it seems scarcely adequate to set down as one of their prime virtues the overcoming of an "artificial fragmentation of science which university departmentalization almost inevitably produces" (p. 7). In such a performance as the present, success will seem to many to flow rather from the collection of several individuals, each capable in his own *Fach* and each able to profit from long and intimate association with fellows bound together in a common undertaking. The total issue of many researches, under many procedures and by many methodologies, could scarcely have been attained by fusing together biochemistry, biophysics, anatomy, physiology, pharmacology, and the like, into "the wholeness and the unity of nature."

Stanford University

MADISON BENTLEY

*An Introduction to Projective Techniques*. Edited by HAROLD H. ANDERSON and GLADYS L. ANDERSON. New York, Prentice-Hall, 1951. Pp. 720.

*Thematic Test Analysis*. By EDWIN S. SCHNEIDMAN. New York, Grune and Stratton, 1951. Pp. xi, 320.

The first of these volumes provides a thorough, almost encyclopedic, coverage of the field of projective techniques. The first section is devoted to theoretical problems, the second reviews the current status of the Rorschach test, the third surveys a variety of projective devices, the fourth deals with pattern analysis and qualitative aspects of the Stanford-Binet and Wechsler-Bellevue scales, and the fifth discusses the use of projective tests in therapy. The treatment is in general thorough and realistic, although the chapters show the uneven quality inevitably associated with separate authorship. There are differences not only in style, quality, and depth, but in the clinical and research emphases underlying the presentation of each technique. Since each author is a leading proponent of the technique he describes, these differences in emphasis undoubtedly reflect the current status of work in the field.

One agrees with Dr. Murray who remarks in his foreword to Schneidman's book that it is a daring contribution to the clinical literature of psychology. The book focuses on two tests—TAT and MAPS—and its major purpose is to compare several expert interpretations of the same protocols; but other psychometric data and considerable psychiatric case-material are given for John Doe, the personality under scrutiny. Readers who seek a detailed description of the various methods of thematic



analysis will be disappointed. It is impossible within the compass of a few pages to provide a working blue print of an interpretive procedure, although the brief chapters by each of the various contributors do provide a suggestion of how their methods differ. The interpretations sample the diverse orientations current in American clinical psychology. Some contributors adopt a formal method of analysis in conjunction with an interpretation of *S*'s production in terms of deviations from expectancies in the normal population. Others combine a formal scoring system with the intuitive artistry of clinical evaluation. Some contributors analyze the thematic material in terms of psychoanalytic mechanisms; some in terms of themes, social rôles, and attitudes; some in a fashion analogous to a psychiatric mental status, describing and quantifying mood and affect of the patient's productions. There is little emphasis on formal analysis of story structure. Some writers develop an integrated picture of the patient's personality, his conflicts, the social rôles he plays, or the psychoanalytic mechanisms which motivate him. Some are willing to venture a diagnosis and there is remarkable agreement in diagnostic classification. Two are willing to predict reactions to therapy. In a final section, the various contributors compare the relative merits of the TAT and MAPS.

Worcester State Hospital

LESLIE PHILLIPS

*Applications of Psychology.* Edited by L. L. THURSTONE. New York, Harper and Brothers, 1952. Pp. 209.

This collection of eleven essays in honor of Walter V. Bingham was announced on the occasion of his seventieth birthday. A preface by Thurstone briefly describes and commends Bingham's contributions to psychology. The various chapters were contributed by Ferguson, Thurstone, Flanagan, Moore, Guilford, Wells, Strong, Bills, Lee, Cleeton, and Stalnaker. Bingham's many friends will applaud his recognition as the dean of applied psychology, although they may regret that the book does not contain a fuller biographical account and that the contents are not more uniformly related to the several significant phases of Bingham's fruitful career.

Stanford University

THOMAS W. HARRELL

*Einführung in die Psychologie.* By HUBERT ROHRACHER. Fourth revised edition. Vienna, Urban & Schwarzenberg, 1951. Pp. vi, 568.

After an introductory section on the aims of psychology, the author proceeds to discussions of standard topics such as sensation and perception, learning, motivation, emotion, and the development of personality. Many of the views expressed would seem rather strange to the author's American contemporaries. In his analysis of perception Rohrer places a good deal of emphasis on past experience and conscious (*sic*) secondary cues. For example, size constancy is explained primarily in terms of retinal displacement, the secondary factors being the conscious knowledge of *O* concerning his movement and the likelihood that the object, whose image is retinally displaced, could move. "Learning" for Rohrer, is "the production of identical excitation processes through repeated conscious experiences of the same content, which processes make for definite structural changes in definite ganglion cells." He emphasizes "that the term 'set' utilized, if somewhat shame-facedly, in most learning theories has . . . become superfluous." Writing on the development of personality the author notes that modern man represents an intermediate stage



in phylogenesis and attributes man's basic conflictfulness to this "in-between existence."

It is not easy to evaluate a book such as this in a setting which is so clearly different from that in which it was written. One can say at least that the author might have profited from a study of the literature of the last 15 years.

College of Medicine

University of Illinois

MARIANNE L. SIMMEL

*Problèmes Actuels de la Phénoménologie.* Edited by H. L. VAN BREDÁ. Paris, Desclée de Brouwer, 1952. Pp. 165.

*Phénoménologie de la Rencontre.* By F. J. J. BUYTENDIJK. Paris, Desclée de Brouwer, 1952. Pp. 59.

At the time of his death the files of Edmund Husserl were found to contain more than 300 unpublished notes and articles, totaling approximately 10,000 pages. In 1938 these, plus Husserl's library and numerous documents relating to his academic career, were assembled at the University of Louvain to constitute the Husserl Archives. Under the direction of Professor van Breda, Louvain has since then become the center of research on Husserl's philosophy. The little volume which he has edited presents the proceedings of an international colloquium on problems of phenomenology, held in Brussels during the early part of April, 1951. Among the chief participants were Professors Pierre Thévenaz (Lausanne), Herman J. Pos (Amsterdam), Eugen Fink (Freiburg), Maurice Merleau-Ponty (Paris), Paul Ricoeur (Strasbourg), and Jean Wahl (Paris). While most of the discussions are quite properly of more interest to the philosopher than to the psychologist—for Husserl was first and last a philosopher—the thoughtful psychologist will find them stimulating, if only because they help to clarify the distinction between philosophical and psychological phenomenology. Of direct psychological interest is Ricoeur's discussion of "the methods and tasks of a phenomenology of the will," and Merleau-Ponty's remarks on the phenomenology of language. The latter is a brief but beautifully clear statement of an approach to the theory of communication that has recently fallen out of favor.

Professor Buytendijk's slender volume deals with a problem which always pursues the phenomenologist, namely, the modes of apprehension of the 'other one.' Buytendijk's method of descriptive analysis leads him into animal psychology, child psychology, the psychology of language, and the psychology of religion. Some readers will be troubled by the religious overtones, and all will deplore the inadequacy of the documentation; but the essay is rich in suggestive observations and should give pause to those who are prone to consider the problem a simple one.

Cornell University

R. B. MACLEOD

*Introduction to Social Psychology.* By E. LLEWELLYN QUEENER. New York, William Sloane Associates, 1951. Pp. xiv, 493.

Social psychology is "presented as the systematic application of established psychological theory to one particular stimulus-response relationship—the relationship of organism to organism." All social behavior may be influenced by, what Queener terms, human-wide, culture-wide, class-wide, caste-wide, sex-wide, crisis-wide, group-wide, and individual-wide variables and knowledge about each of these variables



is necessary for predicting the behavior of one person toward another. The task of social psychology is to analyze these variables in terms of psychological theory. Although field theory, perceptual theory, psychoanalytic theory, and learning theory are all presented as suitable psychological tools for analysis, almost all of the psychological analysis in the text, of which there is much, is made in terms of learning theory. Basically, the learning theory employed is the Hullian behaviorism of Miller and Dollard. A perceptual theorist might be astonished at Queener's treatment of propaganda in these terms but, regardless of whether or not he agrees with the psychological analysis presented, he cannot overlook the desirability of a systematic discussion.

In addition to the separate treatment of each of the variables of social behavior, the text also includes chapters on propaganda, on the relation of social psychology to clinical and industrial psychology, on theory in social research, and on method in social research. In comparison with such a systematic social psychology as presented by Krech and Crutchfield, this book is more elementary and more molecular. It is a systematic, yet simple, treatment which deserves serious consideration as a text in introductory social psychology.

Knox College

ROBERT S. HARPER

*Collected Papers in Psychology.* By E. C. TOLMAN. Berkeley, University of California Press, 1951. Pp. xiv, 269.

This group of 19 papers which appeared in the years between 1922 and 1948 was selected by Professor Tolman himself to provide a representative picture of the development of his theoretical position. Even the earliest papers, concerned as they are with long-familiar concepts and viewpoints (the objectivity of cognition and purpose, molar vs. molecular theory, operational behaviorism, the defense of 'rat-psychology,' and so forth) retain a remarkable freshness and vigor. The reader is impressed anew with the many fundamental similarities between Tolman's systematic orientation and that of his greatest critics. A few borrowed words and diagrams are all that can be found to justify the classification of Tolman as a "gestalt-minded psychologist"—a popular term of disapprobation in the camp of the enemy; he might with equal aptness (or ineptness) be labeled a psychoanalyst. Whatever the justification for this view, these papers will well repay careful reading by the student of systematic psychology.

M. E. B.

*I. E. S. Lighting Handbook: The Standard Lighting Guide.* Second edition. New York, Illuminating Engineering Society, 1952. Pp. xiii, 978.

The second edition of the Illuminating Engineering Society's *Handbook* represents a thoroughgoing revision of the first (1947) edition. The work was done by 34 of the Society's committees whose membership, under the direction of C. L. Crouch, labored for three years to produce this condensation of "essential information on light and lighting." If the performance of the 'Q and Q Committee,' in whose deliberations on Chapter 2 ("Light and vision") this reviewer participated, was representative, the reader may be assured that the *Handbook* is the product of a painstaking quest for accuracy. In view of the modern controversy over methods



of evaluating visual fatigue and efficiency, the second chapter posed rather difficult problems which proved to be impossible of resolution on the basis of available data. Illuminating standards must yet remain matters of opinion; and in the opinion of this reviewer the specifications proposed in the Footcandle Table of Chapter 9 and the recommendations made throughout the volume are in most respects quite reasonable. There is, of course, no substitute for careful research on these important problems. A large portion of the volume is devoted to lighting applications, and there is a wealth of information which should prove to be of considerable value to the researcher in the field of vision.

M. E. B.

*Doubt and Certainty in Science.* By J. Z. YOUNG. London, Oxford University Press, 1951. Pp. viii, 168.

The 1950 B.B.C. Reith Lectures, which Professor Young was invited to deliver, comprise the present book. The first three lectures—which draw heavily upon the work of Sherrington, Adrian, Cajal, and Le Gros Clark, as well as Young's own research with the octopus—give a good, although brief, summary of what the American calls physiological psychology. The fourth and fifth lectures take up the problem of learning from the point of view of brain physiology. The last three lectures are concerned with a theoretical relation between brain functioning and the history of the human race.

Young conceives of the brain as analogous to the modern computing machines. Although in this respect he does not differ greatly from Hebb or from Wiener's group, it is difficult to find elsewhere so many facts of physiology explicitly mentioned and fitted into such a consistent model. Having adopted the thesis "that the use of words to ensure co-operation is the essential biological feature of modern man," and believing that "our thoughts can conveniently be organized by focusing our attention on the importance of communication and the way it is ensured by the brain," Young then proceeds to show how neural networks are acquired by the individual, to suggest how these networks are useful to man in his daily living, and to indicate the significance of such cultural factors as inventions and religion for the development of these networks.

He explains his lectures by saying: "I hope to be able to show . . . that in this system of brain action lie clues for understanding the development of man as a communicating, family, social, religious, and scientific animal. At all stages we find first random behaviour, as we call it experiment or doubting. Then through observations of connexions between features that occur repeatedly, there is the recognition of similarities and recurrences, the establishment of laws, of certainty." He has more than fulfilled his hopes. Young has succeeded in fitting physiological psychology into the whole schema of life. He has related the work of the brain physiologists to the work of the psychologists and, more importantly for the unification of knowledge, to that of the cultural anthropologists, sociologists, and historians. This book should be, at the very least, highly recommended reading for everyone in any way concerned with psychology, and required reading for all psychologists.

Knox College

ROBERT S. HARPER



*La Genèse de l'idée de Hasard Chez l'Enfant.* By JEAN PIAGET and BARBEL IN-  
HELDER. Paris, Presses Universitaires de France, 1951. Pp. 265.

Piaget adds another volume to his long series of works dealing with intellectual development of the child. The current volume is concerned with stages in the development of the concept of chance as observed in physical reality, e.g. probability distributions, combinations and permutations, and drawing of lots. It concludes by integrating the findings of the research with the conceptions of operational thinking discussed in Piaget's previous works, e.g. on intelligence.

The general procedure involves the establishment of an experimental situation in which chance is involved and asking the child to explain what has occurred. For example, in studying chance as operating in the phenomenon of mixture, a situation is set up in which different colored marbles are rolled into a tray and the child is asked to explain and predict the numbers of the different colored marbles that come out. The interviews are presented as evidence for certain conclusions rather than as complete protocols from which the general conclusions emerge.

Stages of development are derived from the situations described, which can be generalized as follows: the first stage (4-7 years of age) involves the inability to accept chance as operative; then a second stage (7-11) appears in which an awareness of chance begins to emerge; and finally in the third stage (11-12) a synthesis of probability and the operations of deductive thinking occur. The authors hold that these stages are interdependent and progressive. They are not too exact about age-ranges, which are presented as rough approximations. There is no explicit attempt to explain individual differences; one gets the feeling that the stages are inherent in the genetic development of the individual.

Ingenious as the experimental situations are, a certain doubt is cast on the generality and scientific quality of the results by the absence of experimental controls and the lack of a careful description of the children used as Ss. Theoretical discussion is for the most part philosophical and speculative. In general, this volume offers the interested reader a stimulating set of questions which, when answered, will enable us to understand more fully the development of operational thinking. It would be a challenging area for psychologists to study with greater experimental precision and theoretical sophistication.

Merrill Palmer School

IRVING SIGEL

*Psychology in Industry.* By J. STANLEY GRAY. New York, McGraw-Hill, 1952. Pp. vii, 401.

The emphasis of this book is on the physical aspects of the working situation. There are chapters on methods of work, on nutrition and rest, and on lighting and ventilation, as well as the usual chapters which appear in introductory texts on industrial psychology. Within the framework that the author has chosen, the book is an excellent one, and ample use has been made of the pertinent research. One might have expected, however, to find some attention to the growing literature on man-machine relations and on the social aspects of the working environment.

Reviewing a new book in industrial psychology serves to emphasize how much research is badly needed. Many of the findings reported in literature, and therefore in this book, are specific to the situations studied, and only further research in other



situations will indicate to what extent they may be generalized. At the present time industrial psychology must be considered a collection of methodologies as well as a body of knowledge, and from this point of view the primary weakness of the volume under review is its lack of methodological emphasis. For example, the whole criterion problem is conspicuous by its absence, and the need for test-validation in each specific situation should be stated more explicitly.

One final comment, which is not specific to this book, but concerns mosts current texts in industrial and personnel psychology: Recently several writers have pointed out that "morale" is a purely formal term, devoid of content, and therefore to assume implicitly a one-to-one relationship between morale and productivity is to make a value judgment without being aware that a judgment is being made. Striking employees could have very high "morale." Consequently, the question, "Morale for what ends?" should be answered explicitly to avoid confusion in the meaning of the term.

Purdue University

C. H. LAWSHE

*The Collected Papers of Adolph Meyer.* EUNICE E. WINTERS, General Editor. Vol. iv: *Mental Hygiene*. Baltimore, The Johns Hopkins Press, 1952. Pp. xxviii, 557.

The last of the huge collection of Meyer's writings, articles, reviews, addresses, essays, expositions, criticisms, and other publications. Much of the material might have been more simply ordered and condensed in the book which Dr. Meyer never wrote. The *Papers* would seem to be most useful to students, disciples, colleagues, and friends, and to historians of the disciplines, movements, and advances in the medical and hygienic regions here explored.

This last volume stands nearest to *Psychiatry* (Vol. ii). Chief topics dwell upon what Meyer called his psychology of *Ergasiology*, "a live organism in action," biologically, socially and temporally organized and consolidated; upon the 'mental hygiene movement; and upon hospital organization and operation, care of patients, and social work. Again are exemplified the author's extraordinary abilities in communion with the individual patient or student, and in small instructional groups. Alexander Leighton's appreciative but critical *Introduction* vividly recalls to this reviewer a long friendship, many conferential meetings, and illuminating ward-visits with Meyer to grateful and relieved patients.

Stanford University

MADISON BENTLEY

*Textbook of Refraction.* By EDWIN F. TAIT. Philadelphia, Saunders, 1951. Pp. x, 418.

Written from a clinical point of view and presuming an acquaintance with optics, this book describes the present state of knowledge about the prescribing of eye glasses. The information is useful to the psychological experimenter; it also challenges him to bridge the gap between clear vision as defined by the operations of the ophthalmologist and visual perception as understood by the psychologist. The discrepancy is alarmingly wide.

Cornell University

JAMES J. GIBSON



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By H. W. STEVENSON, University of Texas

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